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THE PSYCHOLOGICAL AND EDUCATIONAL WORK OF GRANVILLE STANLEY HALL

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The beginning and early growth of a differentiated science of psychology in America was the result chiefly of the efforts of three men,—William James, George Trumbull Ladd, and Granville Stanley Hall. Of these, the first two never broke with philosophy. In his *Psychology*, James' chapter and section heads are for the most part expressive of philosophical topics; and his psychology—brilliant and penetrating though it be—is on the whole a new method of solving epistemological and ethical problems rather than a distinct science. This fact was of enormous importance for making psychology thoroughly respectable at the early date at which he wrote. With Ladd, psychology became a more definite branch of study and attained a certain body of content largely due to the Wundtian influence under which he wrote and to the specific task he set himself in the title of his text-book—the first of its kind in America and itself a signal service to psychology.[1] Nevertheless he returned, after making his psychological contributions, to the philosophical and theological fold "which he had loved long since, and lost awhile." Hall alone made on the other hand a distinct progressive departure from philosophy, which especially in its historical aspects had at one time a great appeal to him. Ultimately in his thinking it is philosophy which loses its kingdom at the hands of genetic psychology by falling

[1]*The Elements of Physiological Psychology*, 1887. *Cf.* preface to this first edition.

in line with all human manifestations as reflecting hidden goals or needs of the personality at particular stages of development.[2]

Stanley Hall was possessed of a vigor, curiosity and intolerance of restraint which made adjustment to the piety and complacent aridity of the New England of the sixties an impossibility. The obvious opening for the intellectually interested individual of his day—the ministry—became increasingly distasteful, and the first of his two triennia of study in Europe, with the deep draughts of the higher criticism then taken, led to a widening of the breach which completed his unfitness for a theological career, and which also sharpened his dissatisfaction with the New England educational situation.[3] His spirit of criticism of prevailing trends, his revolt, and his enthusiasm for European institutions aroused the distrust of the type of college president then common,[4] and so served as a handicap in most of the academic openings of the time. These very qualities, however, rendered him peculiarly adapted to such an institution as the newly founded and for psychology very happily timed

[2] *Cf.*, in *Founders of Modern Psychology*, 54 f., Hall's comments on Zeller's manner of discussing philosophers, which is the reason for the latter's being included among the "founders." "Sometimes it seems as if he (Zeller) were telling the shades of the old thinkers in the light of modern times what they really meant to say and how and why they came to the views they held. . . . . The tendency he started will not be complete until we regard the delineation of the ideas of Plato, Schopenhauer, and all between as what the Freudians call the patent content of an underlying vaster and more coherent latent content in which the higher psycho-analysis must find the original determinants of the systems which shaped and moulded and expressed opinions. Sometime the great systems and schools we now revere may be regarded as very much edited and refined types of folklore, partial satisfactions of so many kinds of instinctive wishes to believe, psychological documents of attitudes," etc. *Cf.* also 59, 73, 75, *etc.* *Cf.* further Why Kant is Passing, this JOURNAL, 23, 1912, 370-426. The ultimately suicidal nature of this method for genetic psychology—itself an expression of human personality—might be urged.

[3] *Life and Confessions of a Psychologist*, Chap. IV. *Cf.* 178, Hall's experiences at Union Theological Seminary. "After preaching our trial sermon before the institution we visited the president for criticisms. When I entered his study for this purpose, instead of discussing my sermon with me he at once knelt and prayed that I be shown the true light and saved from mortal errors of doctrine, and then excused me without a word." *Cf.* also 183, 219 ff.

[4] *Life and Confessions*, 196: "I thought I had a modest appointment in a large midwestern State university, but the president requested me by letter to tell him what I had done abroad, and on receiving my reply wrote me cancelling the engagement because he thought the history of philosophy which I wished to present in his institution would 'unsettle men and teach them to hold no opinions'."

Johns Hopkins University, whose first president, Dr. Daniel Coit Gilman, was suffused with ideals of scientific advancement rather than those of the prevailing propaganda. In 1882 came the opening at the Hopkins, with the opportunity to proceed with the work for which the publishing of the first edition of Wundt's *Grungzüge* in 1874 and Hall's second period in Germany spent largely in Leipzig with Wundt and Ludwig, the experimental physiologist, had developed an all-absorbing impulsion. This was the founding at the Hopkins in 1882 of the first psychological laboratory. No more timely or greater service to American psychology is conceivable than this of not only bringing to it the report of European methods and advances, but of transplanting bodily an energetic branch from the central growing trunk of world progress in extension of all phases of human knowledge. The story is a vivid one, and no complete complex feel of the growth of psychology in America is possible without reading in reflective and grateful mood Hall's memoirs and early papers on philosophy and psychology in America.[5]

Among the influences which led Hall to his activity in experimental psychology was the one which in James constituted the main determinant of his whole psychological contribution,—namely, the possibility it offered for the solution of problems which philosophy had pronounced insoluble or toward the solving of which no methods heretofore open to philosophy had brought satisfactory results. Other influences were, undoubtedly, his already apparent and growing conviction that the genetic approach, the evolutionary point of view, offered a new world of possibilities for explanation, and his passion for unifying, which together with technological interests was later to lead him to a "higher" reconciliation even with the Apostles' creed—a kind of "practical reason."[6] In 1878, in one of his earliest published articles,[7] he sounds an energetic warning against the employ-

[5]*Life and Confessions. Cf.* also Philosophy in the United States, *Mind*, 4, 1879, 89-105; The New Psychology, *Andover Review*, 1885, an introductory lecture delivered at the Hopkins, part of which also appeared in *Mind*, 10, 1885, 245-249; early papers in *The Nation*, from 1878 on, and the *North American Review* of the same period.

[6]*Life and Confessions*, 359, 422, 570.

[7]The Muscular Perception of Space, *Mind*, 3, 1878, 433-450.

ment in philosophy or elsewhere of such words as "unknowable." Sir William Hamilton and the "intuitive school" of philosophy had pronounced insoluble the problem of space perception. Hall, in the current of the stream of researches started largely by Johannes Müller and with his work in muscle-physiology in the laboratories of Ludwig and du Bois-Reymond[8] hard upon him, advanced the contrary view that the physiological and psychological study of the muscles and their sensations will yield the very basis of space perception and the key to its development. In developing this theme he assembles the results of many studies which in his mind point strongly to the existence of muscular sensation (at that date not fully established), and at the same time expresses the view that the discovery of the innervation-sense as the ulterior explanation of most or all facts of physiological psychology marks one of the most important epochs in the history of philosophy. The muscular sense is unique in that it alone does not show incommensurability between stimulus and sensation,—*i. e.*, actual motion of limbs arouses feeling (variously called by the author 'sense' and 'perception') of motion. Its essential character *as* motion is not changeable by any subjective or objective analysis; and so it follows that every sensation of motion is itself spatial. The muscle-sense is rudimentary in man; it is the first and most immediate of the senses. It enters into all experiences—is inexpugnable from all sensation—because of the constant flowing of nerve energy into the muscle, which thus responds to every variation in sensation; so the thinking self is one and indivisible in the whole organism, instead of the brain alone being the organ of mind. Nothing is spatial in intellect which was not first in sensation. The unknowable is that upon which the individual cannot react (here and elsewhere the article contains statements curiously suggestive of many tenets of modern "objective" psychology!). The burden of proof lies upon those who assert that, because space is the logical antecedent of motion, it is superimposed upon non-spatial sensation as an intelligible form of mind.

[8]*Life and Confessions*, 209. Ueber die Abhängigkeit der Reactionszeiten vom Ort des Reizes (with von Kries), and Die willkürliche Muskelaction (with Hugo Kronecker), *Arch. f. Anat. u. Physiol.* (His u. Braune), *Physiol. Abt., Supp. Bd.* 1879, 1-10 and 11-47.

Hall proceeds in a glowing vein to interpret and forecast. Soul-life begins in contractile tissue. The earliest sensation would be a vague area of nerve-muscle substance feeling its own motion as it moved. Here alone (though in a spatial respect only) "subject concurs with, is co-existent with, pervades object"—here the full solution of the space question lies. This primitive sensation-form is evanescent. Experience and elaboration make it a sign of external activity instead of a mere empty form of self-assertion. Psychical life, beginning in muscular substance, retires from muscle to nerve and from nerve-fibre to nerve-cell. Hall also suggests however that, since every property of nerve is now found in muscle tissue, sensibility in excentric portions does not demand complete neural circulation. Movement explains all things, but must itself be self-known. It is the element of space-perception *a priori* to perception of the special senses. Its nature enables experimental psychology to challenge the dogmatic dualism of the "scientific school" of metaphysicians.

The suggestive, stimulating quality of the writer is apparent,—the vistas of ideas whose attending excitement presses and over-rides questionable analogies and the demands for more minute and critical analysis and reservation of opinion which would give pause to a thinker of the more cautious type. This conviction of the strategic position of the cutaneous and muscular senses, their genetic and therefore explanatory significance, seems to have furnished a considerable share of the impetus to a number of the Hopkins studies which in turn have played a not inconsiderable rôle in later views in the field of space perception, especially in the discussion of the question of "local signs."

In his inaugural lecture at Baltimore[9] Hall further emphasized the importance in experimental psychology of work upon muscular action, of the extension of studies on the reflex, and of the rôle of tactual and visual sensations in throwing light upon the nature and laws of motion. Laboratory analysis of supposed indecomposable elements of mind—visual, tactual, auditory—have led to clearing and often to solution of philosophical themes. Moreover, Helmholtz' determination of the rate of the

[9]*Op. cit.*, Note 5, p. 3.

nerve-impulse, together with our knowledge of the elements in the "personal equation" (reaction time), enables us to make accurate measurement of the time and hence of the complexity and familiarity of many simpler mental processes, and to determine the effects on them of attention, toxic agents, fatigue, practice, etc.[10] The study of rhythm, periodicities of all kinds, will throw much light upon the consciousness of time, given in philosophy an exceedingly superficial treatment as a form of the sensory. Other promising fields, Hall thought, were being opened in the study of the brain, the determining of the situation of functional centers and the course of fibre-bundles; but he warned against the danger of forgetting, in a confirmed materialism, the utter incommensurability between the objective relations so determined and the closer, more intimate conscious processes. He emphasized also the importance of the study of abnormal states and individuals, and he described and discussed what he regarded as the essential personal qualifications for the student in this field in a fashion which has much of permanent interest and value.

Here was virgin territory indeed,—a wealth of vivid ideas, prospects, problems definite and more waiting to be definitized. With all of the loose ends, perilous slashings at Gordian knots, wavering terminology, what a feast nevertheless for the future

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[10]Hall here voiced a current feeling of the great possibilities of the reaction-time experiment in various applications, the results from which however have been rather barren in contrast to the early hopes. Hall himself had already made a study of the effects of hypnotism upon the reaction time in an ingeniously conceived effort to throw light on the nature of the phenomenon of hypnotism, then a favorite subject for inquiry. He tested the reaction-time in the normal and the hypnotized states of a remarkable hypnotic subject then in town, arguing that, if hypnosis were an attention-phenomenon (a current view which Hall himself favored), then, since the reaction-time is reduced by strong concentration of attention, the subject should react much more rapidly when hypnotized than he was able to do in the normal state. Three hundred and twenty-nine reactions in three successive sittings (after which the subject had to leave the city) showed that the reactions in the hypnotized state were only about 57% as long as those in the normal state, although there was great variation and all reactions were abnormally slow for this subject. The author also made word-association experiments, and pointed out certain peculiarities in the reaction-words in the hypnotized state. Rather more than half of the article is devoted to interpretations and speculations, many of them suggestive and, as it proved, provocative of a lively discussion. (Reaction-time and Attention in the Hypnotic State, *Mind*, 8, 1883, 170–182. *Cf.* articles by E. Gurney in later numbers of *Mind*; also Hall's Recent Researches in Hypnotism, *Mind*, 6, 1881, 98-104.)

investigator![11] And the papers produced by the Hopkins group during the years immediately following showed serious and promising beginnings and definite contributions to a goodly number of the fields adumbrated. Hall seems frequently to have encouraged his students to include, in addition to their experimental findings and interpretation, suggestions of further vistas, possible significance of results, and the like. "Alas for the young investigator who does not show signs of thinking or intuiting beyond the data which his method or apparatus supplies, for laziness often hides under the guise of scientific reserve!"[12]

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[11]This list fails to do justice to Hall's full versatility and to the omnivorous character of his genius even at this early date. He had previously published a paper on the perception of color in which he offered some experimental evidence which seemed to him to militate against the possibility of the existence of red-, green- and blue-sensing cones, and wound up with a suggestion of a mechanical color-theory based upon the supposition that the cones are composed of transverse discs of progressively varying diameter which vibrate sympathetically with the different wave-lengths of light,—thus, the smaller ones of the tip yielding red. Mechanical agitation of neighboring plates gives after-images. This theory attracted some sympathetic notice at the time (*cf.* Grant Allen, *Mind,* 4, 1879, 267-268), but fell under Wundt's blanket condemnation of mechanical color theories (*Grundzüge,* 2, 1902, 429) and the later set against them. (See Color Perception, *Proc. Amer. Acad. of Arts and Sciences, N. S.* 5, 1878, 402-413.) Moreover, Hall had made a somewhat incomplete but very useful and suggestive study of Laura Bridgman (*Mind,* 4, 1879, 149-172), investigating her memory, imagery, dreams, use of sound and color words, and her cutaneous and muscular sensitivity. He found her to possess markedly increased tactile sensitivity. No evidence appeared for the existence of any dream content excepting tactual; the same thing was true of her imagery of memory and of imagination. He found decided emotionally expressive movements of her facial musculature, in which fact he saw an argument against Wundt's opinion that facial expression arises in movements calculated to modify vision, hearing, smell and taste. This study has been quoted frequently (*cf., e. g.,* Wundt, *op. cit.,* 2, 454. For a modern text, *cf.* C. Griffith, *General Introd. to Psychol.,* 1923, 258).

Several papers (including translations) on philosophy were at hand or already published (Outlines of Dr. J. A. Dorner's System of Theology, *Presbyterian Quarterly Rev.,* 1 and 2; Notes on Hegel and his Critics, *Jour. of Spec. Phil.,* 12, 1878, 93-103); the first of his list of books, which included several articles published elsewhere together with a series of impressions gathered in the course of his periods of study in Germany (*Aspects of German Culture,* 1881, pp. 320); papers on education and pedagogy (Chairs of Pedagogy in our Higher Institutions of Learning, *Bur. of Ed. Circulars of Inform. No.* 2, 1882, 35-44; The Education of the Will, *Amer. Inst. of Instruction,* 1882; also *Ped. Sem.,* 1892, 72-89; Educational Needs, *N. Amer. Rev.,* 136, 1883, 284-290); and the beginnings of the long line of articles on child-study, based on work done before the Hopkins period (Contents of Children's Minds, *Princeton Rev.,* 11, 1883; later in *Ped. Sem.,* 1, 1891, 139-173 and elsewhere; *The Study of Children,* 1883, pp. 13).

[12]*Life and Confessions,* 561.

Hall tells us that, although his title at the Hopkins was "Professor of Psychology and Pedagogy," his chief time and effort were focussed on psychology.[13] He also taught the history of philosophy, his goal being "to teach the whole field of the history of philosophy in such a way as to incline my students to a sympathetic attitude toward all philosophical systems and to make them in turn idealists, pantheists, materialists, associationists, and all the rest to the end that there be no indoctrination or effort at discipleship but that each should choose his own position intelligently and according to his own predisposition, an ideal which I freely expressed and which was never opposed." His students at the Hopkins were an unusually gifted group,—all able, most of them brilliant; and a very high proportion have since achieved eminence. Among them were Donaldson, his assistant in neurology, Jastrow, W. H. Burnham, Cattell, Edward Cowles, John Dewey, Hyslop, Noyes, G. T. W. Patrick, and Sanford. Of the thirty-odd books and papers which Hall lists as constituting the production of this group, himself included, during the years from 1883 to 1889, three were pedagogical, four philosophical, two in psychology of the abnormal, and at least fifteen (the great majority) in experimental psychology. Among the last-mentioned group were contributions to the fields of cutaneous sensation—including Donaldson's well-known work on the temperature senses,[14] in which he reports their punctiform character independently discovered at about the same time by Blix and by Goldscheider, and records the negatively resulting efforts to identify the sense-organs by examination of excised bits of punctiferous skin. Cattell's famous experiments on association and other reaction times were begun here,[15] and Sanford's, on reaction time and on the legibility of small letters.[16]

The published experimental papers of this period with which Hall's name is directly connected consisted in a series of four which were brought out in *Mind* and in the early numbers of

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[13]*Life and Confessions*, 226 ff.

[14]On the Temperature Sense, *Mind*, 10, 1885, 399-416.

[15]The Time it Takes to See and Name Objects, *Mind*, 11, 1886, 63-65; The Time Taken up by Cerebral Operations, *ibid.*, 220, 377, 524; Experiments on the Association of Ideas, *ibid.*, 12, 1887, 68.

[16]Personal Equation, this JOURNAL, 2, 1889, 3, 271, 403; Relative Legibility of the Small Letters, *ibid.*, 1, 1888, 402-435.

this JOURNAL. Of these, the first to appear was one undertaken with E. M. Hartwell entitled "Bilateral Asymmetry of Function".[17] This was essentially an attack upon the "problem of the relation of right and left handedness to the more general law of bilateral symmetry" of function. Of the seventeen pages of the article ten are devoted to an introductory section in which are briefly stated an enormous number of previous observations or allegations regarding functional, anatomical and pathological asymmetries in the domains of sensation, motor reactions, the nervous system, embryonic development, and perception, winding up with views on the unity or duality of the soul. Exact references and often even the names of authors are absent, and the writers dismiss the whole collection with the statement that not all can be considered final. With a feeling of regret that this critical hint was not amplified into an evaluation of the data in terms of methods or related evidence the reader proceeds to the experimental section. The subjects were chiefly four young men, two right-handed and two left-handed, although control experiments were made upon "many people." None of the numerical results is given, not even averages or probable errors, an omission more excusable at that early date,—but instead the authors formulate conclusions "from tables too extensive to print." The experiments proper fall into five divisions; their methods and conclusions are as follows. (1) Subjectively equal diverging lateral movements of the two arms were measured by recording the distance traversed by the index finger-tips along the edge of a ruler, the subject being blindfolded. The preferred hand made the greater excursion, this rule holding more definitely with movements from the shoulder. The unilateral excess was less with slower or corrected movements, when vision was allowed, and when the movements were successive. Individual differences were marked and individual exceptions to main rules fairly common, one observer especially being divergent. (2) A vertical ruler was crossed with a horizontal one so that four quadrants were formed, and up-down movements were introduced with the primary position as a starting point, conditions otherwise being the same as in (1). The preferred hand still made the greater excursion, al-

[17] *Mind*, 9, 1884, 93-109.

though gravity, where it figured, was a more important factor and was never completely corrected by the subjects. (3) The writers tested their subjects' accuracy in bringing the two index fingers together in the median plane by means of symmetrical movements from the shoulders, eyes being closed, (*a*) at a point directly in front, (*b*) one directly over the head, and (*c*) one low behind the back. An ingenious arrangement employed by Bowditch was used,—two thimbles, the one tipped with a short projecting point and the other with a miniature target in plane transverse to the axis of the thimble, the two equalized in weight. The hits were found to cluster about a center often "rather" remote from the center of the target, which was fairly constant for one person but which bore no apparent relation to handedness. (4) A series of reaction experiments followed. The stimulus was auditory (a falling ball) and one set of reactions was made with the right hand to stimulation of the right ear, another with the converse conditions. The hearing of the two ears was found to be normal and equal in the case of every subject, and five reactions involving one side alternated with an equal number involving the other until many hundred had been secured. Three of the four observers gave slightly though constantly greater reaction-times with the preferred hand, but the fourth gave equally constant shorter times with this hand. The authors believe that the findings with the first three subjects, if they prove to be a rule, throw some doubt upon Wundt's identification of apperception with the generation of the motor impulse, which would lead us to expect shorter reactions on the preferred side. (5) Clenching power of the two hands was measured by a simple spring dynamometer. The preferred hand could always exert the more force. Eye-fixation (attention) on one hand always intensified its force, and work of one hand always reduced the simultaneous clenching power of the other. Tests of the ability to duplicate after two sec. a submaximal clenching movement indicated subsequent underestimation when one hand was employed in the duplicating effort, but the reverse when both were used.

The authors are convinced that their results indicate no need for assuming occult (soul) causes for departures from bilateral symmetry. The study of bilateral muscle-tensions, the only

acts of will, will reveal the key to a common principle for paired organs, and the solution of the problem now seems likely to shed light on the nature of consciousness. They regard their results as in the main tentative, and point out the need of more extended study. They make no attempt to correlate their findings with the lengthy series of observations mentioned in the first section.

The following year (1885) saw the appearance of the second paper, "Studies of Rhythm", by Hall and Jastrow.[18] A circular brass plate with notched edges in which projecting brass slots could be set at any desired intervals rotated past a projecting sharpened quill so that a snap was produced when a slot struck the point. By setting the slots in the proper notches a known tempo of snaps could be produced. The experiments began with a study of the accuracy of counting rapidly succeeding clicks varying in number from two to sixty-five, the intervals between clicks being .0895 and .0523 sec. Although the observers were highly certain of the accuracy of their count, nevertheless this invariably fell short of the correct number after four to eight snaps had sounded. This result the authors explain by the fact that the number of auditory impressions which can be distinguished in a second is much greater than the number of even the simplest movements which can be produced in the same time. They stress the complexity of the counting process. The second click is at hand before the reaction to the first is quite out of the way, the third click finds the second reaction still less complete, and so on, until a click comes early enough in the reaction to the previous one to be confused with the stimulus to that reaction. Further experiments were concerned with determining just observable differences in interval, with results which are suggestive only for the reasons that they lack adequate statistical treatment and that they are based on too few experiments. Finally, the authors undertook to determine whether the illusion of filled and unfilled space holds likewise for time, and the results were definite and positive for four observers.

A year later Hall and Donaldson brought out the results of an extensive series of investigations in a paper entitled "Motor

[18] *Mind*, 11, 1886, 55-62.

Sensations on the Skin."[19] The kinesimeter used by Donaldson,[20] with certain well-conceived accessories, provided a piece of apparatus by means of which a button or point could be brought in contact with the skin and moved over its surface at a uniform though variable rate, following skin-contours without variation in pressure. The weight and size of the contact-surface could be varied at will, and the direction of movement could be reversed. As to smoothness of control of stimulus thus secured the writers give us no quantitative evidence, and the mechanical principles employed by them are not above reproach.[21] They worked with up and down movements (toward and from the head), reserving study of transverse movements for a later paper. Seven tables of averages (without variations) present the findings, from which the following are the more important conclusions. (1) For all conditions of rate, weight and surface tested, the observers when in doubt judged the movement to be headward. Moreover, a weight too light to produce sensation when the movement was downward often did so when the direction was reversed. For this result the authors suggest a two-fold explanation,—man's greater familiarity with downward movements (rain, sweat, etc.) as compared with the likelihood of noticing upward movement as usually caused by living things, and the fact that upward movement is against the prevailing direction of the hairs. (2) An indeterminate sense of motion sometimes precedes any impression of direction. (3) Judgment of downward motion requires more time than does that of headward motion. (4) Where rate of movement is varied, the judgment is quicker with more rapid rates, but the actual distance covered is fairly constant. This distance is far smaller than the limen of dual impression for the same region of skin, and in the minds of the authors it indicates the existence of smaller motor "sensory circles" within Weber's discriminative circles. (5) When by a special apparatus observers were allowed to reproduce rate and distance with the other hand they

[19] *Mind*, 10, 1885, 557-572.

[20] *Op. cit.*, Note 14, p. 8. *Cf.* also E. B. Titchener, *Experimental Psychology, Instructor's Manual, Qualitative*, 94.

[21] The kinesimeter of Hall and Donaldson has been criticised in a searching manner and improved by E. W. Scripture and E. B. Titchener. *Cf.* this JOURNAL, 6, 1894, 425, and 7, 1895, 150.

estimated up-movements as longer than objectively equal down-movements. They overestimated relatively long movements and underestimated short ones. (6) With varied rate—the observers still reproducing the movement with the other hand—a given distance was reproduced as shorter when rapidly traversed, though great variation occurred. (7) Marked variation occurred in the sensitivity of different parts of the body. (8) Increasing the weight decreases the time needful for a judgment, and heavy weights seem to move faster than light ones going at the same rate, indicating in the authors' minds that a summation of extensive or qualitative data affects estimates of rate, and that if there be a simple motor sense in the skin it is not independent of aid from other sources.

(9) The authors then undertook a more intensive study of a small area of skin, eliminating variables by shaving and by mapping and avoiding temperature spots. The subjects still judged motion and direction within spaces far less than those needful for distinguishing two points, but warmth and especially cold spots were helpful in fixing locality and in judging motion. Shaving was followed, somewhat to the authors' surprise, by an increase in the difference between the times for 'up' and 'down' judgments. (10) Efforts to investigate further the phenomenon of sense of motion preceding that of direction yielded no results either quantitative or qualitative. (11) Investigations with an electrode as pressure-point indicated a great complexity and diversity of dermal sensations with uniform movement down the arm, and the rate of movement seemed to vary. (12) Experiments with extreme rates of motion indicated that with very slow motion (1 mm. in 10 to 18 sec.) a weight seemed at rest when it really moved 6 to 11 cm.; and, again, it was possible to produce extremely rapid motion of about three inches without the subjects' being able to tell at which end the motion began.

The writers conclude "that 'local signs' are quite heterogeneous, and that, in the strong tendency we have to move the touching dermal surface over objects in contact with it, we are seeking not merely to multiply but to diversify our sensuous data for judging the nature of the impressions and to fill up the dermal 'blind spots' between which impressions are sifted

in to us." Various facts indicate "the skin as not only the primeval and most reliable source of our knowledge of the external world, or the archaeological field of psychology but as a just opening experimental domain of great breadth, where work seems now possible that may compare in both quantity and quality with that accomplished in physiological optics, and which may shed new light on some of the most fundamental problems of psychical action and unfolding."

The next year saw the final one of the four studies, a paper by Hall and Motora entitled "Dermal Sensitiveness to Gradual Pressure Changes."[22] A weight travelling along a delicately balanced metal beam caused uniform variation in the pressure exerted at the peripheral ends of the beam. Of these, one bore a variable counterweight and the other a stimulus-button which rested on the area of skin employed, the tip of the left forefinger, which was held in a constant position and could thus be stimulated by any desired initial weight and rate of increment or decrement. The arm was kept at a constant height, to equalize blood-pressure, and the subject signalled when he became aware that the weight was increasing or decreasing.

The work consisted in one main series of experiments and three less extensive series undertaken with variations of the principal method. (1) Six tables of averages of twenty experiments each for six observers present the numerical results of the main experiment,—the authors stating that "as the experiments progressed the two chief causes of variation, viz., changing degrees of attention and of certainty, steadily diminished." Erroneous judgments were so rare as to be negligible. The first table shows the time in seconds of the judgments with uniform rate of increment and initial weights of 5, 10, 20, 30, 40, 50, 60, 65, 70, 75, 80, 85, 100, 200 and 500 gms. Five of the six observers gave slower judgments with the lighter and heavier initial weights. Of these, three showed fairly definite optimal initial weights of 70, 80, and 100 gms. respectively, while with the other two the time of judgment did not decrease in sufficiently progressive fashion to permit of a reliable inference as to optima. The sixth subject—a highly trained cabinet maker —showed far greater rapidity of judgment than did any other subject, judgments of the five-gm. weight being most rapid of

[22]This JOURNAL, 1, 1887, 72-98.

all. The second table shows the effect of varying the rate of increment, with an initial weight of fifty gms. chosen as a result of the above-mentioned findings. More or less regularly progressive increase in the time of judgment appeared with decrease in rate of increment.

Four following tables give the findings of the above two expressed in terms of gms. added or subtracted before difference was perceived (three and five) and the ratios of these numbers to the initial weights (four and six). The writers believe that their results indicate a relation very inaccurately approaching the constancy expressed by Weber's law,—a relation which is inexact, appears only within limits, and is subject to wide individual variations.

The results indicate far lesser sensibility to growing differences in pressure than to differences in weights successively applied. The writers discuss possible sources of error in the elasticity of the skin, change in area depressed as weight increases, and local anaemia with prolonged stimuli, but do not consider these as sufficient materially to affect sensitivity; it would require other methods to produce a change sufficient to crush or harm tissue without the subject being aware of it, after the analogy of frogs killed without struggle by increasing temperatures when the change is sufficiently gradual.

The writers go into the question of the nature of the conscious processes involved in the judgments which their method elicited. The observers did not experience continuity of change, either quantitative or qualitative, but instead attention singled out a degree of pressure and compared it with a remembered earlier degree. Decision was difficult and slow as compared with that present in successive weight estimation, because of lack of intervals for rest of terminal organs with consequent increase in their sensitivity, and because three instead of two factors must be held in mind, namely, initial pressure, possibility of increase, and possibility of decrease. Hence no inferences from the work of Weber and Fechner can be applied to the present experiment.

(2) In a second less extensive series of experiments the authors used initial weights of 1000, 500, 250, and 125 gms. These were increased or reduced by known amounts, and in some cases allowed to remain stationary. Three subjects gave fifteen

judgments each with increasing, decreasing, or unchanging stimuli and the corresponding three categories of judgment. A table of averages is given from which it appears to require much more time to perceive a stationary or a decreasing stimulus than an increasing one. Moreover, many errors of overestimation occurred, stationary stimuli being judged as increasing, and decreasing ones as stationary. This finding the authors explain in part by fatigue and expulsion of blood due to heaviness of weights.

(3) Sensitivity to negative pressures (pulls) was studied in the next two series. Adhesive plaster was fastened to the stimulus button, so that this stuck to the contact-finger. Ten pairs of alternate just detectable pressures and pulls were given and the results were averaged. The point of subjective neutrality or the apparent tactile zero—a point well marked for consciousness—was, curiously enough, somewhat more negative (on the pulling side) than the point of actual neutrality. The authors believe this to be a kind of mental compensation for the fact that in common tactile experience some pressure is practically always present. (4) In the final series, the apparatus was set at the apparent tactile zero and varied in a positive or negative direction until the observer recognized either pressure or pull. The sensations for a time proved to be surprisingly indistinguishable; for a moment the change might appear to be *plus*, then *minus*, in a fashion suggestive of binocular rivalry. After this latter analogy the authors called the phenomenon *antinomous dermal rivalry*, and explained it in part by pressures at the edges of the finger occasioned by the pull from the central portion.[23] Relative to the minimal stimuli required for tactile sensitivity, enormous weights had to be added or corresponding pulls exerted in order to elicit certain judgment. With slow rate of change, a much smaller degree of difference is correctly recognized. The region on the scale of pressure stimuli here concerned is one, the authors state, regarding whose effects practically nothing is known; it lies outside of the range of validity of the psychophysical law, and needs careful further investigation.

[23]A similar finding is reported in a recent paper. *Cf.* M. J. Zigler, An Experimental Study of the Perception of Stickiness, this JOURNAL, 34, 1923, 80.

In these four studies we have the experimental development which the field of cutaneous and muscular phenomena—so keenly interesting to him—received at the hands of Hall himself and his co-workers. The studies show great vigor and fertility of idea and resource. Ingenious apparatus was devised and adapted. Finished in some respects, in many they bear the stamp of the pioneer study. In them the literature was worked over for the striking and suggestive observation rather than for purposes of evaluation and orientation. They reveal striking trends, but methods employed were inadequate for the discovery of definite laws. They teem with hidden complexities and unexpected findings which await more complete description and determination of conditions. Further analysis has made highly probable the complex character of the consciousness of motion and other mental phenomena which Hall was inclined at the time to view as elementary. The studies in question did much to map out the field and to prepare it for the investigator with more intensive attack. They have attracted no little attention in connection with contemporary and later handling of the problems of tactual space perception, especially the discussions of the local sign, and the factors which affect the limen. Ladd among others presents in great detail the results of Hall and Donaldson relative to motor sensations on the skin, using them to support his view of the complex character of the local sign.[24] James mentions the same findings in the same connection.[25] Wundt refers in a footnote to Hall and Motora's findings as to the influence of rate of increment upon the difference limen for pressure.[26] What the subsequent history of the Hopkins laboratory would have been, had Hall continued his work there, we can only conjecture. In later courses on the feelings which he gave at Clark University he stressed the same fundamental driving idea that was influential in promoting the Hopkins studies,—namely, touch as the mother of, the genetic key to, all sensation and feeling; and he had at this time the unrealized hope of producing some day a large treatise on the feelings. Many things indicate, however, that the [illegible]

[24] *Elements of Physiological Psychology*, 411; 1911 ed. with Woodworth, 404 ff.

[25] *Principles of Psychology*, 2, 1890, 155. *Cf.* also 281.

[26] *Grundzüge*, 1, 1902, 537.

studies would have been made by others. Hall's interest in child study and its application was well over the horizon. In a paper published in 1894[27] the "practical reason" and the pedagogical point of view are well under way; the method of teaching philosophy stressed at the Hopkins in somewhat diluted with a view to the need of immature minds; and regret is expressed that some of the (then) "new" psychologists go no further, but are content only to make methods more exact and results more refined.

But in addition to the laboratory and to the experimental contributions, one other outstanding service to psychology marks Hall's period at the Johns Hopkins University,—the founding in 1887 of this JOURNAL, concerning which Hall's own story is as follows.[28] "Toward the latter part of my stay here I had a memorable call one Sunday from J. Pearsall Smith of Philadelphia, an entire stranger to me who had learned something of the Hopkins work in psychology. He suggested that I found a journal and then and there gave me a check for five hundred dollars 'as a starter.' I had long desired to do this and President Gilman favored it, for the establishment of departmental journals was one of the prominent items in the program of the Johns Hopkins, so that at last, with great trepidation, I printed and circulated a prospectus, gathered material, and issued the first number, printing if I am not mistaken, the enormously excessive number of 3,000 copies and finding, when this number was distributed, that the bills for it alone footed up to seventeen hundred dollars. There were few subscribers and the deficit had to be made up from my own savings. . . . I may add that at one time it had cost me eight thousand dollars more than it had brought in, and although we were not able, after we entered the war, to publish the six hundred pages a year planned, it is now in its thirty-second volume (1921) and brings a modest profit—all without advertising itself or anything else. It was also very difficult to find good material. As the earlier volumes show, most of the small-type part of it, a half or more, which was devoted to reviews, was

[27]The New Psychology as a Basis of Education, *Forum*, 17, 1894, 710-720.

[28]*Life and Confessions*, 227 f.

written by myself. I read voraciously and epitomized over a wide field. . . . I sought to give the Journal wide scope, including the most salient tendencies in religious philosophy, logic, aesthetics, the significant work in the domain of psychiatry, child study, anthropology, etc." Pearsall Smith, whose chief interest was in psychical research, gave up even his subscription when the JOURNAL criticised this movement.

The experimental papers of the Johns Hopkins period mark Hall's last contributions to experimental psychology; after the long break in the stream of his psychological productivity occasioned by the founding of Clark University and the harassing administrative difficulties which absorbed his time he returned to quite a different type of psychological activity. The story of the early days of Clark University, the bright promise of opportunity for a second and even better Johns Hopkins which alone tempted him to leave his work, the long unhappy struggle owing to withholding of expected funds, the enforced reduction of departments, the reluctant compromises, constitute the material of a long chapter in the *Life and Confessions* and, we are told, the chief motive to its publication. During these years Hall managed to give many lectures dealing with educational topics and applications of psychology and to put out a stream of articles for the most part of similar character, but interspersed with an occasional historical essay and with numerous book reviews and notes. And most important of all, he succeeded in salvaging in addition to sufficient material means for limited university activity the all-important spiritual endowment of a university,—an atmosphere of entire freedom, a tradition of the supreme importance of contribution however small to the sum-total of human knowledge, and a variety of methods and points of view which provided untold intellectual stimulation through discussion and criticism. However much Hall grew in the direction of impatience with the slow and to him often insignificant progress of the laboratory worker, nevertheless the facilities for laboratory research never suffered at his hands, and the individual workers themselves often felt in Hall the administrator a pride in, and encouragement of, their work "patently" denied by Hall the geneticist.

During the early Clark period occurred the founding of the American Psychological Association. Hall, in conference with Ladd and others, planned a society of psychologists, and on July 8th, 1892, a group of more than twenty met at Clark University. The Association was formed as a result of this meeting, with Hall as the first president, and Jastrow as the secretary-treasurer. Three months before his death Hall was elected president for the second time,—the only individual with the exception of James to receive this honor.

In 1893 Hall resumed university teaching at Clark, Dr. E. C. Sanford having in the meantime taken over the laboratory. During the years immediately preceding, the tasks which he had undertaken had accentuated his interest in the needs of the student and in the function of the university and the school system. This practical interest and the interest in genetic psychology and child study—well established long before he left the Johns Hopkins—now assumed the dominant position in Hall's activity. The long series of questionary studies of the middle and later nineties resulted, and the outcome shows in the series of four volumes which Hall published during the years 1904 to 1911—the large two-volume *Adolescence,* the abridgement of this in *Youth,* the *Aspects of Child Life and Education,* and *Educational Problems,* the two-volume companion of *Adolescence.* Here are presented vast compendia embracing the great bulk of the findings, conclusions and beliefs of the whole civilized world on the subjects treated or those bearing on them, collected during many preceding years for university lectures. Generously incorporated are the yieldings of questionary studies which began to appear in 1895. Permeating the whole and furnishing the main source of whatever the books possess by way of organization and system is the author's conviction that the understanding of the present is possible only in the light of the whole evolutionary process, a conviction which led to, and was in turn reinforced by, a lecture course on animal and primitive human life, instincts and habits. Two articles constitute the only offshoots of this course in psychological literature,[29]

[29]A Glance at the Phyletic Background of Genetic Psychology, this Journal, 19, 1908, 149-212; and What we Owe to the Tree-Life of our Ape-like Ancestors, *Ped. Sem.*, 23, 1916, 94-119.

for the reason that Hall regarded it as valuable principally in connection with pedagogy. "No course I have ever given sent so many of my students to the library or, I think, contributed quite so much to give them a general and wholesome conception of man's place in nature."[30] It began with views of the lifeless earth and theories of the origin of life in the world, and followed the progress of life from pre-protozoic forms up to historic times. All of Hall's later interests were definitely foreshadowed in the above activities.

Hall is most widely known as a psychologist, and it is thus that he commonly styles himself. Inasmuch as his other activities have to a great extent emanated from and depended upon the psychology of his mature years, it is this which demands the greatest emphasis in any consideration of his intellectual contribution. Closely associated with his psychology at every hand is his educational teaching. "The largest possible aspect of all the facts of life and mind is educational and the only complete history is the story of the influences that have advanced or retarded the development of man toward his completion. . . Thus psychology and the higher pedagogy are one and inseparable."[31] Owing to this peculiar connection which Hall makes between his psychology and its application, the temper of his work would be in a measure lost by attempting too separate a consideration of the two. What, then, is Hall's psychology, and how does it "work" in its most immediate applications?

In the introduction to *Adolescence*, which Hall tells us is essentially his first book, we are informed that the work is based upon the author's psychology, which should logically have been published first. The sections to which we are mainly referred for the psychology are entitled "Feelings and Psychic Evolution." Hall's projected work on sensation and feeling, which would probably have been the *Psychology* to which he referred, never appeared; so for the psychology of his mature and most productive period we must look chiefly to *Adolescence* and its—in large measure—companion and supplementary work, *Educational Problems*. For the rounding-out of his

[30]Cf. the description of this course in *Life and Confessions*, 363 ff.
[31]*Adolescence*, ix.

psychology and its educational and social bearings we must turn to *Senescence* (1922), which in a very real sense is a continuation of the trend established in the other two works. The object of the chapter which in the main presents this psychology is "to state in a brief and summary way . . . the general conceptions of the soul" that underlie his theme.

We find that Hall's psychology consists essentially in a basic law which constitutes the major premise of his whole thinking, a method of inference suggested by this law, and a body of facts accumulated from the literature and from questionary studies and interpreted in the light of this law.

The fundamental law, which Hall calls a "general psychonomic law", is an adaptation to the mental sphere of the concepts of biological evolution and the dependent doctrine of physical recapitulation, developed largely by Haeckel and Herbert Spencer. It assumes that the psychical life and its expressions in the individual develop from birth onward through a series of stages more or less closely corresponding to successive cycles of habits through which life and especially early man and his immediate ancestors are conceived to have passed. "As soon as I first heard it in my youth I think I must have been hypnotized by the word 'evolution', which was music to my ear and seemed to fit my mouth better than any other . . . . I think my curiosity somehow got an early tilt toward origins, and even in college (1863-1867) I brought much censure upon myself by advocating the view that man had sprung from ape-hood. . . For my German teacher, Trendelenburg, *Werden* or becoming was the prime category and the mother of all others, and this helped to predispose me to accept all I could understand of the Hegelian logic, in which all innate ideas evolve by an inner necessity from those that precede, by his three steps—thesis, antithesis, synthesis. . . Schelling's system ranked next because it treated all organic and even inorganic nature as steps in the unfoldment of a mighty process . . . Thus there was a kind of mystic, poetic stage of prelusion by which Darwin, Huxley, Spencer, Haeckel and even Tyndall were, it seemed to me, prepared for in my philosophic history, and when these loomed large upon my horizon they were devoured with the utmost avidity. . . . To conceive the whole world,

material and spiritual, as an organic unity, to eliminate all breaks and supernaturalism, and to realize that everything within and without was hoary with age, so that in most experiences we are dealing only with the topmost twigs of vast but deeply buried trees, gave me a totally new aspect of life. . . . I was bat-eyed to difficulties and impatient at objections, and had a blind spot in my mind for every break in the developmental order and implicit faith that if there anywhere seemed to be gaps it was only because we lacked adequate knowledge. . . . I was once introduced to an audience by an overzealous friend as the Darwin of the mind, and extravagant and absurd as I knew this to be, it gave me more inner satisfaction than any compliment ever paid me by the most perfervid friend."[32]

The above quotations explain much of the spirit with which Hall writes of his fundamental "general psychonomic law which assumes that we are influenced in our deeper more temperamental dispositions by the life-habits and codes of conduct of we know not what unnumbered hosts of ancestors, which like a cloud of witnesses are present throughout our lives, and that our souls are echo-chambers in which their whispers reverberate".[33] "Our soul is full in all its parts of faint hints, rudimentary specters flitting for an instant at some moment of our individual life and then gone forever, dim and scarcely audible murmurs of a great and prolonged life, hot, intense, richly dight with incident and detail that is no more; a slight automatism, perhaps, being the sole relic of the most central experiences of many generations, a fleeting fancy all that survives of ages of toil and blood, a feeling that only peeps out for a moment in infancy, the far-off dying echo of what was once the voice of a great multitude. Yet these psychophores, whatever they are, are wax to receive and marble to retain." They are "somehow represented in the lower . . brain levels, the sequence up the cord, medulla, basal ganglia, cerebellum being a better picture of the real evolution of mind when we can read its meaning aright than the chambered nautilus gives us of its stages of growth."[34]

[32] *Life and Confessions*, 357ff. Cf. also *Amer. Anthrop.*, *N. S.* 6, 1904, 539.
[33] *Adolescence*, 2, 61.
[34] *Adolescence*, 2, 65.

The application in individuals of the "psychonomic law," far from being a simple and straightforward matter, is complicated by many circumstances. The chief of these consists in the vastness of the "collective soul," which is co-extensive with life. Much soul is lost; for whenever a species became extinct, a soul-type vanished. Every animal group may represent some one quality specialized and promoting the survival of that species. The human soul is one of many types. The conscious adult does not reflect the universe, but he is rather a fragment detached from the great world of soul, "well fitted to illustrate some aspects and hopelessly unable to exemplify or know other regions in the cosmos of soul. In him many different traits thus may contend for expression, this conflict being particularly likely to occur in offspring of different racial stocks. Layers successively acquired may be reversed in their appearance in the individual, while recent salient occurrences often appear to sink into fathomless oblivion, and very ancient tendencies may push up even into consciousness." Certain functions, notably sex, are variable up and down the scale. Large brain, acquired late in the phylum, is early in the individual. Thus the individual often distorts the racial order.[35]

According to Hall the normal growth of the individual mind necessitates the living-through of each stage, because the development of any one stage is the normal stimulus to the development of the succeeding stage. This principle is the guiding thread to his whole doctrine of education, although its application frequently has to be tempered in the interests of practical modern demands. Environmental factors, however, may cause the appearance of reactions before their normal time; this produces undesirable precocity. The result is the over-studious child, or the child otherwise prematurely habituated to adult ways. Perversions of certain kinds and accentuation of, *e. g.*, sex before the later rational processes which it should accompany may thus ensue. Moreover, certain normally earlier stages thus crowded out instead of being lived may emerge later as criminal tendencies, which are essentially misplaced reversions to lower savage strata.

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[35] *Adolescence*, Chap. X., esp. 68. *Educational Problems*, I, 201f.

Such is Hall's fundamental law. In his mind it has certain bearings upon the present and future status of man. Despite elements of civilization that interfere with growth, "I am an optimist root and core, not merely because an evolutionist must hold that the best and not the worst will survive and prevail, but because in most though not yet in all of these fields I see clearly the beginnings of better things."[36] "Tentative as is now our knowledge, it is sufficient to generate a deep hope that man is yet in the making, that the best things have not yet happened in his history and that perhaps his present stage is at the same time the point of departure of a yet higher one related to all that adolescence now gives, as it is to the stages that preceded. Assuming the bionomic law, infant growth means being loaded with paleo-atavistic qualities in a manner more conformable to Weismannism, embryonic growth being yet purer, while the pubescent increment is relatively neo-atavistic. From this it would seem to follow that the more complete and established the maturity before offspring are produced, the greater the probability of phyletic progress in successive generations."[37]

Hall's central law immediately raises many problems, and suggests a programme. The child and the race are each a key to the other. "We shall never truly know ourselves till we know the mind of animals, and most especially those in our line of descent. We must recognize that some of them are our superiors in certain respects; that while we explain them by explication of those traits wherein we excel, they no whit less explain us by those of their traits which are superior to ours and of which our souls contain only relics; that if in general we are their realized entelechy, they are the key by which alone we can unlock many of the mysteries of our own origin and nature." "We must collect states of mind, sentiments, phenomena long since lapsed, psychic facts that appear faintly and perhaps but once in a lifetime, and that only in few and rare individuals, impulses that it may be never anywhere arise above the threshold, but manifest themselves only in automatisms, acts, behavior, things neglected, trivial and incidental, such as Darwin

[36] *Adolescence*, I, xviii.
[37] *Ibid.*, 50.

says are often most vital. We must go to school to the folk-soul, learn of criminals and defectives, animals, and in some sense go back to Aristotle in rebasing psychology on biology, and realize that we know the soul best when we can best write its history in the world, and that there are no finalities save formulae of development."[38] Consequently the method must be objective, carefully observational; and the "genetic" psychologist, while he must use introspection in the old way, or reinforced and perfected by experimental methods wherever they serve his purpose, will find it necessary, almost in exact proportion as his work becomes fundamental, to gather his data empirically from the comparative study of lower forms of life and of children and from the collation of many minds beside his own.[39] With this programme in mind, Hall chose the questionary method, supplementing the returns obtained by many students and by himself with the results of innumerable statistical and other studies and recorded observations obtained in the course of his amazingly extensive reading.

The results of these aims and methods in Hall's hands, together with their educational applications, compose the bulk of the four volumes of *Adolescence* and *Educational Problems*, and in a great measure of *Senescence* as well.[40] Hall never evolved more than the barest rudiments of a system of psychology in the ordinary sense; though he makes frequent use of general psychological concepts, he is at no great pains to define or to interrelate them, frequently indeed declaring definition impossible.[41] His impatience with classification was apparently a reaction from the to him hampering classifications of current philosophical or semi-philosophical systems. "The same revolution in the studies that deal with the soul impends that von Baer and Darwin represented for the body. Before their day, everything was classification, nomenclature, fixed species, just as with the pregenetic psychologists everything was faculties and processes, analyses and categories, as if the adult human mind as we know

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[38]*Ibid.*, 2, 62; 1, vii f.

[39]*Ibid.*, 2, 62.

[40]*Adolescence*, 1904, 1373 pp.; *Educational Problems*, 1911, 1425 pp.; *Senescence: The Last Half of Life*, 1922, 518 pp.

[41]*Cf. Psychol. Bulletin*, 3, 1906, 53.

it were a fixed and settled thing."[42] Hall's psychology is a kind of nature study of childrens' minds and expressions, rather than a psychology in the scientific meaning of the term.

• The terms "soul" and "mind" appear at times to have a slightly different connotation for Hall. "Most summarily, the idea of soul we hold to is in its lower stages indistinguishable from that of life, and so far in a sense we revert to Aristotle in holding that any truly scientific psychology must be first of all biological. Mind is almost, possibly quite, co-extensive with life, *at least animal life* "(italics not in text). A bit later, we have: "Mind and life are one and inseparable."[43] The terms are employed almost interchangeably, and if there is any difference it would seem to consist in a slight tendency to identify "mind" with the more plastic and adaptive ("intellectual") phenomena and "soul" with the more ancient, fixed life of instinct and feeling. We find a thoroughgoing dualism and interchangeable causality; Hall did not realize the monism adumbrated in his early paper on the Muscular Perception of Space (*cf.* Note 7, p. 3). The soul reflects the growth not of the brain alone, but of every part or organ. It is as profoundly sexed as the body.[44] There are as many types of mind as of body, and we can truly know soul only through body, and conversely. "A brain without mind is as impossible as a mind without brain, every normal and pathological change in either affecting the other."[45] In the light of the foregoing, the author's frequent references to "collective soul" and "mind continuous with life", related probably to his pantheistic leanings, are either figurative or purely mystical. Much the same thing is true of "man-soul," a term which appears with slightly increasing frequency through the series of his books and articles and goes through a kind of imperfectly progressive evolution

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[42] *Adolescence*, 2, 62.

[43] *Ibid.*, 63. *Cf.* also *op. cit.*, Note 29, this JOURNAL, 200: "For one, I prefer to defy the current horror of anthropomorphism and to show a decent respect to continuity. Only to speculative and monoideistic minds can the question between some psychic rudiment and tropism be so put that we must cleave to the one and despise the other answer, for both are at the same time true and both are involved in the same reactions."

[44] *Adolescence*, 2, 57f.

[45] *Ibid.*, 63f. Hall became impatient with the controversy between parallelism and interactionism and dismisses it as fruitless in *Life and Confessions*, 431.

(analogous to the developing convictions of messiahship and son-ship which he elsewhere attributes to Jesus) from "man-soul" through "Man-soul" to "Mansoul."

Various divisions or processes of the "soul" or "mind" are designated and more or less clearly differentiated. Most stressed is that department which is composed of "feelings," "instincts," "feeling-instincts," "instinct-feelings," "sentiments." We are given little by way of distinction of these terms, —in fact, Hall has asserted vigorously that the feelings cannot be defined, and the best course with regard to them is to confess our ignorance (*op. cit.*, Note 41, p. 26). He regards the James-Lange theory as really only a philosophical speculation, entirely beyond the present reach of scientific investigation.[46] Hall is more interested in accounting for them, however. The feeling-instincts are "the deeper, older and more fixed unary or binary or at most ternary compounds that were matured and compacted before man arose." They are, *e. g.*, "the aggressiveness and cruelty of the carnivora, and the timidity and deceit of creatures long preyed upon." "Hunger, love, pride, and many other instinctive feelings, to say nothing of pleasure and pain, can be traced far down through the scale of vertebrate and to invertebrate life".[47] Belonging to this group are "the many-voiced comments, the sense of assent and dissent, . . . the illation of strength or the esthetic responses, the play of intuitions, the impulses to do or not to do, automatic tensions or contractions."[48] The feelings cannot be well remembered, or indeed they are entirely forgotten, because they are blind and inconsistent, lacking the intellectual factor of experience.[49] The feeling-instincts are assumed to be the psychophores or "bearers of mental heredity," some of which persist below the threshold of consciousness throughout our lives, while others "affect conduct as if striving to be relived and competing for the focus of attention or perhaps leading a submerged life in nearly faded automatisms."[50] These feeling-instincts may be "made over as instincts," or they may be "transformed into habits." The phrases are obscure, and the reader finds little to aid him in

[46] *Life and Confessions*, 430.
[47] *Adolescence*, 2, 60 f.
[48] *Ibid.*, 67. [49] *Ibid.*, 73, 91.
[50] *Ibid.*, 68.

an investigation of Hall's use of the term "instinct;" the word is employed in many connections, in a fashion literary rather than scientific, and even rarely in a manner which would seem to indicate that it does not imply heredity. Thus, in connection with childish fugues, he refers to "the instinct to follow a man or a vehicle. . When this truant instinct is strong *and hereditary*, as is often the case, . . the impulse . . may be irresistible and almost epileptic" (italics not in text).[51] Again, "such children often have a weak instinct for property." Young men may be dominated by "conservative or radical instincts."[52] "Subordination of each member to the whole and to a leader cultivates the social and co-operative instincts."[53] Of condemned juveniles, "472 lied from instinct and weakness."[54] An autobiographic instinct even is mentioned.[55]

In addition to the instinctive or feeling side of mind and soul, Hall has much to say about the conscious side, or consciousness, in spite of the calamitous possibilities which he thinks arise from the introduction of this term.[56] The occurrence of consciousness is favored by relatively quiescent intervals; it tends not to develop in periods of ferment and rapid change. Two main types of consciousness seem to exist for Hall,—a kind of surveillant consciousness or "enjoyment" of phenomena, and an adaptive consciousness, similar to that recognized by certain of the "functional" psychologists, *e. g.*, by Angell. The first type exists in a crude form in childhood. "The unconsciousness of the normal child makes it, though naked, not ashamed . . . Its consciousness is in this respect not unlike that of animals."[57] "The consciousness of childhood is molted, and a new, larger, better consciousness must be developed."[58] In the adult mind this type of consciousness is very limited, fragmentary, presentative of only the topmost twigs of the deeply buried tree of the soul or like a tiny flickering flame in a vast dark museum filled with the bones and fossils of ancient life. "Conscious life . . . in the best of us is pitifully unorganized and loose-jointed, and it differs per-

[51]*Adolescence*, 2, 376. [52]*Ibid.*, 87. [53]*Ibid.*, 2, 221. [54]*Ibid.*, 353. [55]*Life and Confessions*, 395.

[56]"Wherever the term consciousness is introduced there is confusion and no one ever yet accepted another's definition of this protean term." *Op. cit.*, Note 29, p. 20, this JOURNAL, 200.

[57]*Adolescence*, 2, 72. [58]*Ibid.*, 96.

haps most from the body in its fragmentary, incomplete and heterogeneous nature."[59] "The very self-consciousness that burns so intensely at some point, with attention often so obsessive, blinds us from seeing the larger rest of ourselves." "Highest, narrowest, most apical, and mobile as a tongue is the affective state of the present flitting moment, related to general personal mind as little as it is to the impersonal phyletic, or as it again to general soul."[60] Some intimation there is that consciousness might penetrate further, but how we are not told: "Each has, at least ideally, a capacity to comprehend much if not most of the experience of the race from the beginning, but this experience is dormant in us unless brought out by objective life or observation."[61]

The second type, the organizing and adaptive consciousness, arose late in the species and appears correspondingly late in the life of the individual. It appears to play a dual rôle. At times Hall views it as abnormal, a little disreputable. "We cannot believe that consciousness is even quite the efflorescence of the human plant. It may be a wart raised by the sting of sin, a product of alienation or a remedial process. . . Consciousness seems in some of its aspects more likely a fall or a process of purgation so far as it is merely adaptive." "Our consciousness is but a single stage and one type of mind: a late, partial, and perhaps essentially abnormal and remedial outcrop of the great underlying life of man-soul."[62] Feeling may after all do the work of this consciousness: "Feeling may be de-specialization, dissolution, and preliminary to evolution along new lines";[63] and women and soldiers at least should have none of it. "She (woman) works by intuition and feeling . . . If she abandons her natural naïveté and takes up the burden of guiding and accounting for her life by consciousness, she is likely to lose more than she gains, according to the old saw that she who deliberates is lost."[64] "Conscience is the very acme of self-consciousness. It involves deliberation and excludes most of those energies of the soul that are *bewusstseinsunfähig* or which cannot get into the narrow field of consciousness. The case of

[59] *Ibid.*, 67.
[60] *Ibid.*, 66. [61] *Ibid.*, 65.
[62] *Ibid.*, 67; 1, vii. [63] *Ibid.*, 2, 67. [64] *Ibid.*, 562.

conscience must be submitted to an inner oracle, but the brief which consciousness submits can never contain all the data. Hence comes casuistry and every kind of perversion, *e. g.*, the conscientious objector."[65] But on the other hand the adaptive form of consciousness is an organizing and developing agent performing an exceedingly important function for the successful adjustment of the individual. If the larger, better consciousness is not developed, increased exposure and vulnerability will bring deterioration. "From the oldest trophic and vaso-motor functions and organs mediating pleasure and pain and perhaps other fundamental feeling-instincts, up the complex hierarchy of powers when the projection system itself slowly comes under some, perhaps tangential, organ of thought, and during adolescence when a suddenly widened area of life is governed and perhaps reconstructed by intelligence, this viaticum is easily and rapidly passed in normal psychogenesis, but for the full understanding of it the entire animal series . . . is none too large a basis. Conscious thought, noetics, intellect, reason, are popular and provisional terms for the last or neopsychic stages in this process, and all their higher forms are probably rarer and developed later in the average human being than is usually held."[66] Hall does not tell us what processes constitute the adaptive consciousness, but from such passages as the above and from the similarity of functions ascribed it is highly probable that these are the higher intellectual activities of reasoning and judgment, owing their character to the associative as opposed to the merely projicient apparatus. "In all this flux and chaos (jostling of barbaric and animal traits), however, common sense, that knows and adjusts to facts and to the external world, and the sciences of nature are the two solidest of all foundations and are represented by the solidest and most firmly woven brain texture; if man can ever bring order into the rest of his confused psychic life it must be by going back to these and working out and upward from them by observational methods in the inner as well as the outer world."[67] And it is precisely this function that is glorified in old age and constitutes its supreme ideal contribution.[68]

[65]*Morale*, 3. [66]*Adolescence*, 2, 450.
[67]*Ibid.*, 69. [68]*Senescence*, Chap. 8.

Further efforts to work out Hall's criteria of mind and of consciousness and to line them up with various conceptions of the nature of consciousness—or perhaps any such efforts at all—are superfluous, so little was he concerned with matters of system. He makes occasional and more or less fleeting use of all or most of these conceptions. Neither did Hall have the slightest interest in working out a consistent envisagement and arrangement of the simpler and more complex processes of consciousness. He recognizes a kind of progression: "Sensation, perception and understanding represent roughly the various degrees of appropriation of apperception, the whole process of which is too integral to be partitioned."[69] The processes of childhood are those bound up with the projicient system,—the senses, activities, with rudimentary consciousness and practically no reasoning. Space and time are immediate realities bound up with touch and movement, and reality itself is an immediate datum of touch. Whether the senses in childhood belong to "intellect" or to feeling is uncertain, especially in the case of touch, under which Hall considers pain, a word which (after the older terminology) usually means for him the opposite of pleasure. Had he been interested in the issue, his reference of the senses back into a time far antedating the growth of consciousness in the adaptive sense at least might have led him to classify them as feelings, certainly as something not belonging to this consciousness. He does indeed—in common with other genetic writers—consider touch as a kind of primitive experience bearing the rudiments both of feeling and of the later "higher" senses. The organizing and synthetizing functions of intelligence are on the contrary late; and old age, if it realizes its complete destiny, elaborates these. Early and late adolescence and middle life are characterized by the progressive ordering of the riot of the feelings.

In the course of his writings Hall has much to say about imagination, and makes not infrequent references to ideational types,—"eye-mindedness," etc.,—which he thinks explain in a measure certain types of human activity. His attempts to assign imagination to a certain age and period are not completely successful, and its position with relation to the texture of pro-

[69]Why Kant is Passing, this JOURNAL, 23, 1912, 372.

cesses on the main line of development seems somewhat external. "Cloud forms have almost created the imagination."[70] "It is often a well-marked epoch when the young child first learns that it can imagine and state things that have no objective counterpart in its life, and there is often a weird intoxication when some absurd and monstrous statement is made, while the first sensation of a deliberate break with truth causes a real excitement which is often the birth pang of the imagination. More commonly this is seen in childish play, which owes a part of its charm to self-deception. Children make believe they are animals, doctors, ogres, play school, that they are dead, mimic all they see and hear. Idealizing temperaments sometimes prompt children of three or four to suddenly assert that they saw a pig with five ears, apples on a cherry tree, and other Munchausen wonders, which really means that they have had a mental combination independently of experience. Sometimes their fancy is almost visualization and develops into a kind of mythopeic faculty which spins clever yarns and suggests in a sense, ... that all their life is imagination."[71] "Puberty is the birthday of the imagination. This has its morning twilight in reverie, and if brilliant and vivid, supplements every limitation, makes the feeble athletic, the beggar rich, knows no limitations of time or place, and is, in a word, the totalizing faculty. In its world all wishes are actualized, and hundreds of our returns, elsewhere reported, show that in many sane children, their own surroundings not only shrivel but become dim and shadowy compared with the realm of fancy . . . The ingenuous youth sees visions or dreams somewhat according as he is ear- or eye-minded. The optical centers are especially sensitized to new harmonies of form and especially color. . . . Thought and the two higher senses are never so vivid and intense because they are just ready to bifurcate, each to lead its own life, so that the two are now peculiarly liable to be confused, intellectual processes to take sense forms, and *vice versa*. It is for this reason that hallucinations, false sight and especially hearing, is most common."[72] Hall relates sensory types (whether ideational or perceptual is not always clear) [illegible] *Anlagen*: "The eye-minded man is perhaps more disposed to

[70]*Adolescence*, 2, 69. [71]*Ibid.*, 1, 350. [72]*Ibid.*, 313f.

idealism than the practical motor-minded type. The blind have the most unfaltering sense of the reality of the external world because they are nearer the original tangible form in which reality was first given."[73] But apparently "thought" is something of more dignity than and of different stuff from imagination: school youth suffer because of over-stress of the concrete, with insufficient emphasis upon "the higher and more mental associations of likeness," relatively removed from activity, as opposed to "thinking", that is, "mere contiguity in space or sequence in time." "If their psychic operations can be called thought it is of that elementary and half animal kind that consists in imagery." "They lack even the elements of imagination which makes new combinations and is creative because they are dominated by mental pictures of the sensory".[74]

Turning now from Hall's fundamental psychological doctrines and "*apercus*" to a consideration of their results in his hands,—chiefly found in the bulk of the treatment of *Adolescence*, *Educational Problems* and in considerable measure in *Senescence*,—we note that in addition to statistical studies they consist for the most part in enumerations of what human beings at various ages—especially adolescence—are prone to think about, attend to, and react emotionally upon, and of what they attempt to do or actually do, and how much they feel and imagine and "think." Interspersed everywhere are "genetic" interpretations,—references to analogous phenomena in phylum and race,—and educational and other practical suggestions and considerations, which latter are often of great value, and which taken by and large constitute Hall's major contribution. Analysis or scientific description of mental process or reaction or emotion is rarely undertaken excepting in those instances where the object or goal of the reaction is obscure and hence a measure of specification of the reaction itself is necessary,—*e. g.*, in the case of reactions called "automatisms".[75] The net yieldings are

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[73]*Ibid.*, 2, 46. [74]*Ibid.*, 2, 463.

[75]*Adolescence*, 1, 154ff. The treatment of automatisms here given—their progressive incorporation into voluntary systems—while not in all respects novel is vividly put and exceedingly suggestive, full of valuable hints for the teacher, clinical psychologist and psychiatrist. *Cf.* also A Synthetic Genetic Study of Fear, this JOURNAL, 25, 1914, 149-200 and 321-392, for examples of the author's more complete descriptions of emotions.

essentially an envisagement of certain demarcated stages in the growth, both mental and physical, of the human organism, with a vast accumulation of questionary and statistical data regarding each stage; of the growth-stages of certain parts of the organism; of certain modifications of growth due to sex, and of the results of asymmetrical or impeded growth.

Growth is the subject of the first three chapters of *Adolescence*,—growth in height and weight, of parts and organs, and of motor power and functions. The bulk of these longish chapters is devoted to assembling the results of numerous studies of the rate of growth from embryonic to mature stages, and to inquiries into the factors which influence growth, to canvassing of opinions relative to the existence of optimal rates of growth, and many similar questions. The immense labor of gathering this material, and the intimations of the possible practical significance of its various phases, make the preparing of the book an outstanding service to the practical worker and to the investigator in the fields of human adjustment and maladjustment. The prepubescent retardation, with the subsequent pubescent outburst in which girls precede—revealed in all the studies—is of great interest to Hall; he regards it[76] as a relic of pre-savage and savage epochs, in the first of which the female was forced to assume maternal functions before "nature had completed her preparation" for them, with resultant slowing of growth, while in the later period taboos arose which postponed impregnation and so favored growth activity. Or natural selection may have played a part; since female coyness and reserve develop before full sexual desire, the largest and most vigorous girls would succeed in resisting impregnation and would later produce maturer offspring.

From the numerous phenomena of disproportionate growth of parts Hall draws the following conclusion: "If the soul grows with every part of the body, its development is not continuous, uniform or proportionate, but with successive nodes, the earlier stages ever a little more strange and alien to the newer, like dimly remembered past lives to a transmigrationist."[77]

[76]How seriously Hall took any one of his "genetic" explanations is a problematic matter; he remarks, "I have constantly suggested genetic explanations of all degrees of probability" (*Adolescence*, viii).

[77]*Adolescence*, I, 128.

The theme of the muscles and their functional growth is an inspiring one to Hall. Muscles are the organs of will, thought and feeling, the media of accomplishment of all human edifice and of character. The development of the muscles in both race and individual according to Hall progresses from that of the larger, fundamental muscles to the finer, accessory ones "that wag the tongue and fingers." This is one of Hall's most insisted-upon precepts for pedagogy; in his education, the child should be encouraged in his impulses to develop his larger muscles before he begins exercising the finer ones. Current tendencies to force him to school before eight years of age are dangerous for the reason that premature exercise of accessory muscles tends to develop precocity, miniature adulthood, with all its attendant risk of a later appearance of "savage" tendencies thus crowded out at their normal time of appearance. Muscular development is closely correlated with sexual maturing and Hall finds much to convince him that this in boys is the result of fighting for desired members of the opposite sex, which latter has been and is a principal cause of conflict. The treatment of "motor automatisms"—by which is meant the total of "unconscious" movements such as swayings, tappings, fingering of objects; in short the total of movements sometimes referred to as random—has many elements of practical insight and valuable suggestion, however one may regard the genetic interpretations offered. These movements present survivals of old adjustments worked out by remote ancestors, and their display constitutes a necessary and proper stage preliminary to the development of higher powers. Perhaps they should even be encouraged, if we knew just when, by stimulating reflexes and so promoting "low-level connection between afferent and efferent processes that brings the organism into relation with the world of sense." The age of control with inhibition follows: the child must learn to sit still, a difficult task. But almost immediately Hall recalls the more modern conception of inhibition as a long-circuiting of processes and a directing of energy elsewhere, and the "psychonomic law" is for a time in abeyance while the author makes many observations of insight and suggestiveness especially in the matter of present social conditions which impede desirable growth-processes. The automatisms

are the raw material of intellect, feeling and will, and education should so guide events that none is left to expend its strength outside of the domain of voluntary control, integrated habit-patterns. Thus integrated, the fullest strength is available,—witness the superiority of the brain-worker to the manual laborer in tests with the dynamometer because of the former's ability to turn into one outlet energy generated over a larger surface.

But the integrating process may be arrested. Automatisms are similar to ideas, which tend to grow to delusional "intensity" unless checked by others; thus movement may "pass to fixed attitudes and postures of limbs and body, disturbing the normal balance of flexors and extensors." "They are motor analogues of the stray, segregated psychoses that make the subconscious activities now beginning to be studied".[78] Children of twelve who sway, can't sit still, possess these automatisms in most marked form and are often the constitutionally duller ones. The chief of the avoidable causes of such anarchical movements is overpressure, misfit between environment and nature. To avoid this, safeguards are needed superior to gymnastics; and the sad inadequacy of the many "schemelets" extant is shown by the puny bodies and nervous youths so commonly seen at the present day. Hall discusses the inadequacy of the modern industrial city with its passive amusements or at best activities under unhealthy conditions as a substitute for the older supremely educative home life—especially on the farm—in a manner the reading of which could not but be beneficial to any individual who attempts to work with youth in the city, and which has already exerted a marked influence. As to remedial measures: the farm is the best educator. Industrial education is beneficial in proportion as other phases (scientific, historical) are emphasized in addition to the mere acquiring of skill. Sloyd tends to the grave educational error of overstress on method and loss of content and goal and individuality of emphasis. Many forms [illegible] expense of leg muscles. Studies showing the sporadic growth of skills indicate the need of wider knowledge of characteristic "plateau" periods with stress on sense or perceptual training which will encourage subsequent motor spurts.

[78] Ibid., 160.

The psychology and pedagogy of play follow, in the discussion of which, after the recapitulatory interpretation, occur many points illuminating to the individual who would increase motivation and zest in childrens' activities, the main difference between play and work lying in strength of motivation. "The heart of youth goes out to play, as if man remembered a paradise." "Interest and play are one and inseparable as body and soul. Duty itself is not adequately conceived and felt if it is not pleasure, and it is generally too feeble and fitful in the young to awaken much energy or duration of action. . . . Work is menial, cheerless, grinding, regular, and requires more precision and accuracy and, because attended with less ease and pleasure and economy of movement, is more liable to produce erratic habits. The worst product of striving to do things with defective psychic impulsion is fatigue in its common forms, which slows down the pace, multiplies errors and inaccuracies, and develops slovenly habits, ennui, flitting will spectres, velleities and caprices and neurasthenic symptoms generally. . . . Instill into a boy's soul learning which he sees and feels not to have the slightest worth and which cannot become a part of his active life and increase it, and his freshness, spontaneity, and the fountains of play slowly run dry in him, and his youth fades to early desiccation."[79]

Following the cues suggested by asymmetries and blockages of growth at lower levels, Hall takes up in two succeeding chapters the problems of diseases of body and mind, and juvenile faults, immoralities, and crimes. His thesis is that, in the light of the evidence that ontogeny often reverses or fails to follow phylogeny, we have at adolescence a period of great temporary plasticity, a loosening of the bonds that hold developing parts into a more or less unitary whole, and a struggling among ancestral elements which are compactly knitted together in proportion to the time of their association in the phylum. The bulk of the text is devoted to the assembling of studies upon the incidence at adolescence of various diseases and insanities, the effects of school upon health, and the like. Mental peculiarities of adolescence receive much space, and for these Hall draws heavily upon his questionary returns, finding many perversions

[79] *Ibid.*, 233 ff.

of appetite, irregularities of sleep, intensification of dream life and fancy, over-meticulousness, consciousness of self and over-assertion of individuality with silliness largely due to fear of ridicule in connection with developing fancies and sentiments, marked tendencies to imitate due to the very uncertainty and turmoil and furnishing a basis for the "social instinct," dramatic trends, loquacity and also shyness. All youth is for a short time at least genius, and genius is the apotheosis of adolescence in gifted individuals. "Excess of normal vitality not only safely can but must explore the beginnings of many morbidities, both to know the more varied and intense possibilities of human life and to evoke the sanifying correctives."[80] Sex functions appear first as irradiating activity and later as localized sensations, not as yet associated with the opposite sex, before the brain reacts on the sex organs and control is established. Along with these manifold tendencies the cerebral controlling mechanisms (judgment and "common sense") are developing and normal personality results in proportion as these show their influence. Any factors that make for precocity—springing up of sex and other instincts before normal control processes can assert themselves—are favorable for the development of morbidity and mental aberration, and to the formation of a blasé outlook, with deadening of admiration and enthusiasm. Hall thinks that previous bad pedagogy and many conditions incidental to city life and civilization with its prolonging of the period of apprenticeship are among the leading causes of such arrest. He suggests also the rôle of race mixture.

Youthful offences are indications of the difficulty experienced by youth in adjusting itself to social conditions. Normal children manifest at various stages of their development all the main "savage" and atavistic tendencies which, if disproportionately developed or not checked by tendencies which normally appear later, and if made more consequential by adult bodies, constitute the criminal picture. Criminals are essentially over-[illegible]

who offend are as a rule smaller and to a relatively great extent characterized by asymmetries together with others of the so-called "stigmata of degeneracy" of the Italian school; in his [illegible]

[80] Ibid., 321.

opinion, biological considerations would favor the view that physical asymmetry is prone to accompany exceptional character and conduct. Decadent stocks and retrograding functions are frequent biological phenomena. Among the causes of "crime" in juveniles, then, we must reckon the existence of an "innately criminal" mind, a "degenerate" in whose case perhaps the "animal part of his nature is abnormally and congenitally disproportionate to the intellectual," and who is sure to be an easy victim of the influence of the seasoned criminal with his "society-owes-me-a-living" code. Poor intelligence, which checks the development of the controlling process, is another cause, and a discussion of tests of intelligence occurs in *Educational Problems*.[81] Many environmental causes are discussed,—poverty and degradation in the home, hunger, weather, school, restriction, inadequate training. "There is a pregnant truth in the saying that every society has just the kind and number of criminals that it deserves." "One thing is certain, that the great body of crime is not to be essentially reduced by criminal codes, however skilfully drawn, but only by bettering the individual and social conditions of the community at large."[82] The remedies are, first, understanding, living with the "criminal" youths. "Those smitten with the institution craze or with any extreme correctionalist views will never solve the problem of criminal youths." Those who deal with such youths "should be educators with souls full of fatherhood and motherhood, and they should understand that the darkest criminal propensities are frequently offset by the best qualities, that in the criminal constitution there are precisely the same ingredients, although perhaps differently compounded, . . as in themselves."[83] Flogging and scolding have advantages, if used judiciously. Reading of exciting adventure stories in tender years may serve as a catharsis in the Aristotelian sense, "to arouse betimes the higher faculties which develop later, and whose function it is to deplete the bad centers and suppress or inhibit their activity." Boxing furnishes both vent for and encouragement to control of the anger impulse. Improving the efficiency of the schools as agents in moral training by utilization of the sentiment of

[81] Vol. 2, Chap. XI. *Cf.* also Chap. XII.
[82] *Adolescence*, 1, 341f. [83] *Ibid.*, 407f.

justice is important; pain must follow and measure the offence by natural methods, and there must be no personal unmotivated clemency.

The handling of sexual development and periodicity and of adolescent love includes the usual discussions of the vast biological rôle of sex and love, and a massing of data on physiology and growth of sex organs, effects of castration, studies and opinions regarding nutritive and other accompaniments of periodicity in women. The assumed effects upon the soul of the organic basis of sex, especially in women, prompt discussions of attitudes and psychical phenomena related to sex whose existence in any save a few abnormal women is very problematical, and suggests a regime of periodic rest and retiring to meditations upon the vast racial trust which is hers that the author, in a less strained mood, would probably condemn as admirably calculated to produce the sympathy-craving, symptom-mongering neurotic invalid whose appearance he elswhere deplores. Part of the description of adolescent calf-love is exceedingly striking and judicious, both in *Adolescence* and in the chapter on the budding girl in *Educational Problems*, although its character is essentially literary rather than scientific. Long chapters on the adolescent quickening to nature and on the phenomena of religious conversion are included, with pedagogical suggestions. The importance of a broad religious training in guiding adolescent feelings, especially love, to a love of righteousness, of humanity and of the universe is stressed; this is the most powerful means of steering in safety through the storm and stress period. There is no domain of education, from kindergarten to university, from trades to classics, and no type of institution or provision for atypicals, but receives consideration and pedagogical suggestions.

In the course of the writings under consideration, Hall works out more or less detailed descriptions of at least five stages of life, with suggestions regarding the racial period which [illegible] of pedagogy desirable for each one. The period of infancy and early childhood harks back to remote animal ancestors. The child should be much left to nature, the parents and teachers following the promptings of love; [illegible]

to exercise the larger muscles are the order of the day. A second unique period in human life is the age from eight to twelve. The brain is nearly adult in size and weight, health is almost at its best, activity is greater and more varied than at any other time, there is peculiar endurance and resistance to fatigue. "The child develops a life of his own outside the home circle, and its natural interests are never so independent of adult influence. Perception is very acute. . . . . Reason, true morality, religion, sympathy, love and esthetic enjoyment are but very slightly developed. Everything in short suggests the culmination of one stage of life as if it thus represented what was once, for a very protracted and relatively stationary period, the age of maturity in some remote, perhaps pigmoid stage, . . when in a warm climate the young of our species once shifted for themselves. Heredity is more stable . . . the elements of personality are few but are well organized and on a simple effective plan." At eight years, Hall reluctantly concedes, the child must start school and precociously work the smaller muscles, for he is a candidate for a highly developed humanity. The insight necessary to give response to school tasks is not present, the wisest requirements seem alien, but the senses are alert and reactions are vigorous. Consequently this is the age for drill, breaking the child into basic habits of all sorts with the least amount of explanation or coquetting for natural interest. The method should be authoritative, dogmatic. Duty and the spirit of obedience must be instilled.

The adolescent, next, is neo-atavistic. Development is more saltatory, suggestive of some ancient period of storm and stress when old moorings were broken and a higher level was attained. There are increased morbidity and diminished morality, reconstructions of the functions of every sense, progressive assertion of sex, lust to know nature, and many other changes including those heretofore mentioned.[84] Then comes later adolescence, about the nineteenth year, marked in boys by a practical cessation of growth, by some decline in the curve of health, and by "other phenomena that suggest that, having achieved adult size, there is likely to be a period of slightly reduced vigor as if to rest and adjust after being cast up by a

[84] *Adolescence*, Preface.

flood-tide on the shore of manhood a little exhausted. . . . At this point perhaps the individual represents the phyletic stage where, in its survival value in the struggle for existence, the advantages of increased size and strength began to be surpassed by those of a higher mentality and the main current of evolution swerved thought-ward." Loosened cohesions are being re-compacted. To fit this age, the college should stand for extensive rather than intensive study.[85]

Then follow the stages of maturity, the "youth of old age," and senescence, where the feelings, although often vivid, are suppressed and the intellectual functions and the wisdom which only age can bring are in their glory. If the race is ever to achieve its goal, "ripe and normal old age" must "gather the fruitage of the past" and "penetrate further into the future."[86]

The above is far from being a complete presentation of the many trends and varied phenomena which Hall attributes to adolescence and to other ages, and of his numerous discussions regarding the value of dancing, music, art, and other branches in meeting the needs of the different stages of development. The writer believes, however, that it is typical. From the psychology which Hall developed mainly in *Adolescence* his later-culminating interests in Freudianism and in religious psychology emanated. Hall's stress upon feeling and his centralizing of the phenomena of adolescence about sexual development and the associated evolution of love made him an eager student of the Freudian views. He never fully accepted the Freudian doctrines of the all-dominance of sex, for he regarded hunger as its copartner in directing the struggles of the phylum and hence of the individual, and even made hunger the subject of a course of lectures in which he traced the influence of appetite and the factor of nutrition in shaping the feelings, instincts, habits and mind in general. He also outlined a scheme for applying the Freudian mechanisms to anger.[87] In spite of these and other reservations, however, his enthusiasm for Freudianism grew and [illegible] in his zeal for introduc-

[85] *Ibid.*, 2, 527 f.

[86] *Senescence*, esp. Chap. [illegible]

[87] Anger as a Primary Emotion, and the Application of Freudian Mechanisms to its Phenomena, [illegible]

ing it to America *via* his students and his reviews of literature, and in his bringing to America in 1909 of Freud himself and his then leading European and American disciples.

From his early concern with theology and philosophy Hall carried a deep-seated interest in the psychology of religion—in religion explained as a need and construct of the soul, divorced of any supernaturalism, and (as his interest in youth grew) adapted to the requirements of the ephebic mind as he conceived it. Much of the chapter on Conversion in *Adolescence* is devoted to developing a "human" Jesus and a religion which should create in youth a feeling of unity with humanity and the universe and should guide the developing capacities of love in these directions and so constitute a supreme socializing influence. Trends emanating from this felt need, brought to a focus by the biographical methods of the psychoanalysts, led to the writing of the two-volume *Jesus the Christ in the Light of Psychology,* which appeared in 1917 and is in some respects the best of Hall's works because his methods were best fitted to this type of task. Here he traced extensively religious doctrines and cults current at the time, and developed the thesis that the Christian faith could be explained as a project of the folk and individual soul, or that, if Jesus were an historical character, the ideas of messianity and of son-ship to God could have developed in him as a result of influences extant in his day. The somewhat Hegelian interpretation of the death and resurrection *motif* as a kind of summation of life is much stressed by Hall, who finds it pervading initiation ceremonies, myths, history, and even the evolutionary processes themselves.

In addition to his many books and articles, almost four-hundred in number, Hall's activities during the Clark period included the founding of three additional journals, two of which are still in existence. In 1891 he started, at his own expense, the *Pedagogical Seminary,* a large part of the contents of which in the early volumes came from himself and from Clark colleagues and students. The *American Journal of Religious Psychology and Education* he started with the aid of the University in 1904; at the fifth volume the name was shortened to the *Journal of Religious Psychology,* and the editing passed largely

into the hands of Dr. A. F. Chamberlain, with whose death in 1914 its publication ceased. In 1917, Hall and Dr. L. R. Geissler started the *Journal of Applied Psychology*.

In viewing Hall's work as a whole, many internal contradictions stand out, perhaps because it is in a state of rapid and tumultuous growth and inner incompatible enthusiasms not unlike the adolescent whom Hall describes.[88] These very inner incompatibilities serve to sharpen pedagogical problems and hence have a value to education which is not inconsiderable. Of these conflicting trends, the most marked is the opposition between the order of nature demanded by bionomic and psychonomic laws, on the one hand, and the regimen and control demanded by practical situations, on the other,—a duality which is not without reminders of Kant's distinction between the pure and the practical reason. The "surge and thunder" of the genetic currents lead Hall into positions which he rejects when the set is otherwise, because he is also an observer of insight and possessed of much of the "common sense" which he evaluates so highly at times; despite his antipathy to explaining by the nearest cause,[89] he is not unmindful of it in practical connections. The difficulty appears in the need for the eight-year-old to enter school and run the risk of "precocity" by developing accessory muscles. If the principle of the dangers of a later outcrop of "savage" tendencies and of the basis for a criminal career thus afforded holds, should we not rather re-make the whole system on the order-of-nature basis than take such chances with childhood? Again, the authoritative regimen and stress on duty, obedience, and drill indicated by the nonrational character of the child provide what we later read is the best possible basis for truancy, development of automatisms and lack of volitional concentration; and, moreover, it fails to utilize the zest of the play-tendencies. As Winch pointed out,[90] there is truth on both sides, and later pedagogical resources and "model" schools have gone far toward combining the advantages of the two. The disparity between the need of concreteness and the need of more abstract language symbols is in similar

[88]Cf. review of *Adolescence* by E. L. Thorndike, *Educ. Rev.*, 28, 1904, 217-227.
[89]*Op. cit.*, Note 29, p. 20, this JOURNAL, 181.
[90]W. H. Winch, review of *Adolescence* in *Mind*, N. S. 14, 1905, 259-264.

case. Again, the difficulty appears in the uncertain rôle assigned to the "higher" consciousness, the organizing and adjusting mental functions; composing in the "genetic" key Hall makes these somewhat abortive and conducive to casuistry, but when the key shifts to the practical, they are glorified into "common sense," the greatest hope of youth and the basis of the supreme contribution of age. The regime which the ideal woman should follow in the light of the genetic law would exclude her entirely from the consolations of senecsence. That which alone can save erring youth spells doom to morale in the soldier. If the "genetic" law holds and feelings are the fundamentally reliable guide (*Morale*), why the disadvantage of mental defect? and so forth. The question has important bearings for ethics, but is confusing to the main issues and out of place in psychology, whose problems are quite otherwise. Hall encounters difficulties similarly conditioned in his teaching relative to the fate of stages of development which have been passed or skipped in the individual. If the complete realization of one stage, in so far as possible, is the essential condition to the stimulation of the next, how is precocity possible? And if a stage is crowded out by circumstances, how can it break out later in the absence of its requisite stimulus? Hall takes refuge in the reversions of stages and the jostling of diverse traits, and in the view that environmental suggestion is equally capable of evoking a stage; so many concessions are made, however, that one suspects the significance if not the fact of any "order of nature." Difficulties incidental to practical needs for repressing "animal" and "savage" stages have frequently been indicated by Hall's critics.

Hall's unqualified acceptance of the doctrine of the inheritance of acquired characteristics leads him to the view that the acquisitions of the individual cannot be transmitted and so accrue to the race unless maturity is reached before reproduction, a position which bodes ill for the race ever acquiring the later valuable maturity of old age unless we draw heavily on the principle of the variability of sex on the age scale.

Another "internal" difficulty appears in the inability to establish stages in the individual of sufficient definiteness to warrant the phylogenetic explanation. Both the paleo-atavistic child and the neo-atavistic adolescent have "births" or "golden

ages" of sense, of perception, of imagination. Both require exercise of fundamental muscles. The descriptions of the crudities of behavior—especially that concerned in interest in the opposite sex—of the child and the adolescent have much in common.

Many difficulties of an external character may be urged against Hall's psychology,—difficulties of method and of fact. Hot-footed in his quest for data to fill out the gaps in the known evolutionary process and indifferent to other problems, he was too impatient to use the slow and to him insufferably tedious methods of the laboratory. In 1904 he expressed regret that some of the "new psychologists" (which at that date meant experimental psychologists) go no further, being content only to make methods more exact and results more refined.[91] "Scientific psychology was becoming more and more prone to flout the good old Aristotelian dictum to the effect that it was only affectation to treat any subject by more exact methods than the subject matter demanded. Hence I felt that laboratory psychology had been influenced too much by the exact physical sciences and not enough by biology." "Introspection made no attempt to explain the flitting vestigial and marginal states" that its devotees "recorded, or to trace the differences in these phenomena to their origin in individual disposition or experience, and drew few and meagre conclusions from the vast body of data which they accumulated."[92] In addition to such complaint, Hall's objection to the introspective technique at least was based on his view of the limitations of consciousness, that it must perforce be useless in proportion as work becomes fundamental,—although he takes it more seriously in his attack

[91]The New Psychology as a Basis of Education, *Forum*, 17, 1894, 710-720.

[92]*Life and Confessions*, 361, 433. At the time of the first intense interest in the higher thought processes as attacked in the laboratory, shortly after the publication of Titchener's *Experimental Psychology of the Thought Processes*, one of Hall's students incorporated the concepts of the *Aufgabe*, the *Einstellung* and the *determinierende Tendenz* in his thesis. A stormy time followed; but Hall's lectures soon began to teem with [illegible] called at the quarters of certain of the laboratory students with the request that they review their data with him very carefully. His hope that the laboratory had its hands on the tail of an ancient lead—if such hopes he had—were apparently disappointed, however, for in *Life and Confessions* (361) he speaks of the concepts in question as "something like categories".

upon Kant's schemata.[93] Hall's revolt against exact methods was extreme, and he failed to employ them even in departments of "genetic" psychology where they might have done valuable service, *e. g.*, in work upon variations in sensitivity with age. But for the most part Hall's objection that the more controlled methods could be of little immediate aid to his problems was justified, and he chose the only one available for obtaining in sufficient quantity and form the data he needed. Many of his critics have held that Hall employed the questionary in a fashion not adequate to rule out its obvious sources of error.[94] The objections advanced are not without ground, and many of Hall's norms of behavior will undoubtedly undergo a shift in a direction away from the emotional and perfervid extreme. His use of statistical and other studies in supplementing the questionary, moreover, is open to the objection of over-epitomizing and under-evaluating; the labors of compilation have been immense, and the service is great, but more extensive presentation of mean variations and of critical interpretation in the light of method would have added much both to the labor and to the value.

Finally, the major assumptions of Hall's psychology have everywhere been called into question. The attacks upon the culture-epoch theory, the doubtful status of the doctrine of the inheritance of acquired neuromuscular dispositions, and the light shed by anthropology upon the true character of the mind of primitive man are too well known to require comment. Hall's phylogenetic interpretations and anthropological data are frequently not corroborated by workers in the fields in question.[95] Biology in Hall's hands often has a teleological ring, and the use of natural selection as a warrant for optimism is far from clear. The direct application of biological principles to consciousness arouses grave difficulties, however illuminating these principles may be when applied to behavior; such concepts as those of

[93]Why Kant is Passing, *op. cit.*, 373, "No experimental or introspective study of the processes of perceiving and comprehending things ever found the filmiest trace of any part of these schemata." Kant might have answered that his categories affect consciousness without penetrating it.

[94]*Cf.* esp. E. L. Thorndike, The Original Nature of Man, *Educational Psychology*, 1, 28-37. For a statement of the advantages and limitations of the questionary, *cf.* Titchener, *op. cit.*, Note 20, 387.

[95]*Cf.*, *e. g.*, Van Waters, The Adolescent Girl among Primitive Peoples, *Jour. of Relig. Psy.*, 6 and 7, 1913 and 1914, 375-421 and 75-120.

"psychophores" or mental heredity are either figurative or purely mystical. Consciousness has a way of being most keen and pointed in moments of emotional flux, and often of lowering at more static periods. Accessory as well as fundamental muscles figure in earliest infantile reactions and throughout the course of habit formation. And similarly with reasoning, imagination, feeling; more careful studies show a gradual growth of these functions, extending to an ever widening subject-matter, rather than a sudden appearance at any one stage. Studies of the conditioning of emotional reactions indicate the possibility of very young children forming intense emotional attachments which in some cases may have permanent consequences.

The man of science marked by reserve, his enthusiasms conditioned to matters of method and system, intolerant of loose ends and contradictions, his eyes strained for the exception which may test the rule, evaluating his findings in the light of method and technique rather than in terms of their positive or negative character or far-reachingness, choosing his problems not without regard to his conception of the state of development of his science and their possibility of solution by reliable methods, will never count Hall among his fellows, however much he may value the contribution of Hall in pushing forward the frontiers of interest in science. For in almost all respects Hall is his opposite. Possessed of an enormous energy and vitality which unfitted him for the labor of patient attack on a limited front, he belonged in large measure to the type which includes new pioneers of imagination. The individual of this type must have great unifying instruments, ideas which almost at the first encountering leap into place and provide outlets for torrents which refuse to be hampered by the slow process of verification, but instead rush into new continents, whose tenability will have to be determined by later careful prospecting and examining. The dominant theme is its own category of criticism; results and theories are judged in terms of their possibility for unifying [illegible] the dominant explanatory concept rather than in terms of consistency and method. One field after another becomes the scene of activity. In a mind possessed of impressibility and powers of rapid apprehension,

the drive of the idea makes possible a vast deal of reading, because of the adventure of finding grist for its mill, but it inhibits critical estimate of what is read. Nothing slows down the process; the thinker sees whatever thought it is that possesses him pervading and explaining the universe; he becomes almost a prophet, a mystic, an arriver at ultimate reality. So far-reaching in its possible effects upon human happiness and self-knowledge does the idea—self-stripped of obstacles—appear, that its possessor unwittingly turns propagandist. Such enthusiasm speeds abroad; many are inspired to specialize, and large audiences of workers are shaken and set about the task of application.

In considerable measure Hall approximates this type. He does not present it in its most rugged form, however, possibly because he never completely shook off the influence of the puritan outlook and doctrines among which he spent his earliest boyhood. His revolt, he tells us, created a certain sadness, a deep feeling of isolation, of being misunderstood, and he "confesses" in himself a certain apologetic strain.[96] This appears in a perceptible emasculation in his writings when he is touching upon such themes as would be most emotionally loaded for the conservative, somewhat smug puritan spirit. In connection with his handling of youthful "crimes", where certain remarks (quoted on p. 40) indicate insight into the need for a vigorous and searching analysis of the social structure in its responsibility for crime, he takes his main refuge in a view similar to the seriously discredited position of the Italian school, and refers to a "criminal type," to "degenerates," to the "animal part" of nature, etc., in a fashion that smacks almost of certain semi-theological treatments, although it has also a certain harmony with "genetic" psychology. His ascription to the savage and the child of traits present in the majority of untrained adult human beings—"With children as well as savages, truth depends largely upon personal likes and dislikes"—perhaps reflects the same temper. Still more does this trend emerge in relation to the treatment of sex, especially in its perverse manifestations, where occur expressions of a rather tense ethical character which ring strangely in a scientifically aimed treatise

[96] *Life and Confessions*, 594, 571, 575, 589.

and lead to uncertainty as to what fundamentally the author's position is. Some biographer of wisdom and insight, working possibly along lines of the same general direction as those laid down by Kempf,[97] may in time explain in a thoroughgoing fashion this trend, which more than any other prevents Hall's books from being an expression of genius, however much of genius there was in his personality.

But if in one sense Hall was too conscious of his puritan "folk-soul" audience, in another sense he was too oblivious of existing forms. His psychology itself was too much based on revolt, on complaint with existing philosophical systems as opposed to careful "inside" criticism of them, and was too anarchistic, to be likely to withstand the test of time. The refinement of concepts and critical studies in the fields of anthropology, psychology, and to a great extent of biology, failed to find sufficient place in his thinking to give his psychological work the alignment with progress necessary for permanent position and real contribution. And he made the systematic error of trying to find well-marked stages and distinctions in his given material instead of working out clear concepts which might serve as the basis of a scientific classification. In casting about for bases of comparison, in speculating as to what field would have furnished the most adequate outlet for his genius, one is tempted to consider literature. Where Hall is at his best, both his method and his observations have been essentially literary,—the products of insightful noting. This trait appears in the descriptions of youth and childhood, when the "psychonomic law" is temporarily set aside; in the description of the menace of modern civilization to youth; and in many other instances where he discusses practical needs and applications. Many of Hall's views have been expressed in different connections in literature; thus, one of his criteria of success, sympathy, is closely akin to what Anatole France recognized as a mark of genius.[98] He has a gift for the living word and phrase, a style which embodies the feeling of genius. But the fact of his just missing hardihood, or whatever it is which at times prevents complete absorption in his theme,—probably somehow related,

[97] *Psychopathology*, 1921.
[98] Gsell's *Opinions of Anatole France*.

as we have suggested, to his struggles with his early puritan environment,—would have hampered him more in attaining greatness in literature; here, too, his leanings to propaganda would have been a more serious retarding influence, and also the lack of restraint in his style, the occasional figure which beclouds, the obscurity or repetition of figures which had strong appeal and are often very beautiful.

It seems not unlikely, then, that Hall found the type of work which could give his genius the best expression,—that of pioneer and prophet of a young and growing science sufficiently related to practical needs to offer opportunity for the enthusiastic propagandist. Here he could perform his most brilliant and distinguished service. Here lack of system was the least handicap, for the times were not ripe for system in psychology. His chief temperamental needs were socially polarized,—to extend, to impress students and others, to apply, to create opportunities for freedom and avenues for publication of work; and for this his style was supremely fitted. He was rarely equipped for the task of "generating interest over a large surface," which the worker, with his problems upon him, can turn into a narrow outlet; and from workers in the field of education have come the most enthusiastic tributes. Hall, the "passionate lover" of youth and childhood, has served youth more than anything else, and especially handicapped, adult-ridden youth. There is no field of education but has felt the influence emanating from him and has been markedly benefited; and if every worker who is concerned with the handling of childhood and adolescents were familiar with the assembled information, tenets and spirit of Hall's writings, a mighty stride would be taken toward the solution of the problems of criminology and a new day would dawn for youth.

SOME PRESENT TENDENCIES OF PSYCHOLOGY[1]

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In this address I shall not attempt to prophesy what Psychology will achieve or what changes it will undergo in the far distant future. My purpose is rather to examine present-day tendencies and, by contrasting them with the past history of Psychology, to reach some fairly valid predictions of the more immediately forthcoming developments of the science.

Hitherto, psychologists have generally adopted the physicist's methods of analysing composite into more elementary phenomena, and the physicist's thorough-going mechanical explanation. But the inadequacy of these procedures is fast becoming recognised. The doctrine, for example, of the association of ideas has proved by no means sufficient to explain the processes of recognition and thought. More careful introspection has discovered in these processes thoughts without words, mere acts of knowing and states of knowledge, which embrace far more than can be stated in terms of the elements of which the machinery of associationism is composed.

Psychologists are beginning to recognise that the elements which they have abstracted are conceptions which are never experienced as such, and from which—as such—living experience has never really been developed. For example, the perception of objects has not arisen, as many of the older psychologists supposed, from a synthesis of different elementary sensations. A sensation is an abstraction never actually experienced in isolation. The young organism's consciousness develops by the experience of 'situations' and 'objects'. The so-called 'complex' perception comes *first*, growing with experience in complexity, and analysed, as it thus grows, into so-called 'simpler' parts,—mere abstractions,—which by ultimate analysis become relatively lifeless, formless and meaningless.

Again, the comparison, or the association, between two experiences *a* and *b* does not involve merely these separate, isolated experiences, but the primary 'more complex' experience *a-b*, which must be treated as a unitary whole, and be regarded as a necessary precedent to the act of comparison or reproduction of the parts which may be carved out of it.

The psychology of the future will come to realise that the combination of simple mental processes into complex ones plays a far less important part in the mental development of the race

[1]An address delivered at Cornell University, August, 1924.

and in the mental life-history of the individual than the differentiation of vague, ill-defined wholes into parts and their crystallisation into clearer, cleaner-cut facets. The development of meaning in cognition, of emotion in feeling, and of purpose in volition is not to be described or explained in terms merely of originally almost meaningless sensations and images, of a hypothetically elementary series of feeling-tones ranging between pleasure and displeasure, and of blind impulse, respectively,—the mental atoms reached by introspection biassed by analogies from the realm of physics. Atomism of this sort can yield only part—no doubt an important part—of the truth in psychology and physiology. It deals only with mechanism and leaves out of account meaning, purpose and selective direction.

Yet another defect of this mental anatomy and atomism must be mentioned. Psychologists have been content to assume that dissociated parts of experience represent the elements of which the whole is made up. The psychology of the future must avoid such doubtful assumptions. The abnormal gait of a tabetic does not represent the ancestral gait at a remote epoch; nor do the personalities obtained under conditions of mental dissociation necessarily represent by-gone personalities.

There are still many who, on similar erroneous grounds, believe that the protopathic and epicritic systems, as conceived and distinguished by Head and his collaborators, represent two different stages in the evolution of cutaneous sensibility, that an 'all-or-none sensibility' preceded a 'graded' sensibility which alone permitted of effective spatial localisation and discrimination. It has likewise been supposed by Rivers that in the evolution of living forms instinct preceded intelligence, the latter representing a totally new system which suddenly entered, displacing or incorporating various parts of the old. It is not thus that evolution has proceeded.

The inadequacy and the unreliability of experimental introspection are becoming evident. In the Müller-Lyer illusion, for example, it has been found by Lewis and others that continued practice may gradually reduce and finally abolish the illusion, though the subject be throughout unaware of its presence or nature. Or again, the subject may in absolutely good faith advance reasons for his conduct based on introspection,—reasons which are mere rationalisations,—in place of the actual motives or causes of which, for various reasons, he is in utter ignorance. Closer study of post-hypnotic suggestion, of dreams, of multiple personality, automatic writing, somnambulism, etc., and of intuition, genius and inspiration, is clearly showing how much goes on of which introspection is quite impotent to give any account.

Indeed the view is developing that mind is by no means confined to conscious processes, that mental process—'mental energy', if we may use this doubtful term—must be conceived as something more fundamental than the consciousness of or belonging to the self (the only consciousness available in and for any organism), something which may or may not be accompanied by consciousness, something on which introspection may not always be able to throw light. Mind is thus coming to be conceived as something more primary than consciousness. Psychologists accordingly are finding it convenient (absurd as it may sound) to speak of unconscious, as well as of conscious, mental processes.

This conception of the unconscious mind is, of course, a mere hypothesis, affording a useful terminology in which to describe observed phenomena. The contradiction which, at first sight, seems to be an insuperable objection to combining the two terms 'unconscious' and 'mental' is not more serious than that involved in the physicist's conception of the ether, to which he has attributed qualities and properties quite unthinkable, quite unrealisable in actual experience.

The fact that conscious experiences are the subject's own private property and that they can never be communicated to others save by physical expression,—by gesture or by spoken or written language, *i. e.*, by movement, conduct or behaviour,—has induced some psychologists to endeavour to banish the mental element wholly from psychology, and to insist that scientific psychology means the study of behaviour, the study of outward responses. They claim that psychology can proceed without the consideration or employment of mental terms at all. What, in fact, they are doing is to study the behaviour of the organism for its own sake. They call themselves 'Behaviourists'. In point of fact, they are physiologists, observing reactions to stimuli in the intact organism, instead of—as in most physiological experiments—in isolated organs, tissues, or parts of systems. They have escaped the difficulty of dealing with mental processes, by ignoring, if not by denying, their existence. Such perversity breaks down in actual practice. The Behaviourists, as we see from their writings, cannot get along without employing terms implicative of consciousness. They cannot consider sensation, *e. g.*, colour vision and its defects, the varieties of imagery, processes of imagination, deliberation and the like, without using psychological language. Behaviour must certainly be studied by the psychologist. [illegible] include the investigation of nervous impulses, muscular contractions, glandular secretions and other activities which constitute the reactions of the living body. But nervous processes and the like are not *identical* with conscious processes. [illegible]

tions, percepts, ideas, decisions or emotions. Psychology studies bodily behaviour not, as the physiologist attempts to study it, for its own sake, but for the light it throws on mental processes and on the relation between them and bodily processes.

What this relation may turn out to be it is, of course, impossible to predict. But it is quite likely that neither of the two chief alternative hypotheses now offered may prove ultimately acceptable. It may well be that nervous process and conscious process are not to be regarded as running *parallel* with and separate from one another, like the opposite surfaces, the inside and the outside, of a curved mirror. It may also well be that nervous process and conscious process cannot be crudely conceived as *causally* interacting one on the other. We may one day come to realise that mental and nervous processes are fundamentally inseparable, and that their distinction is possible only by abstraction, the use of which is justifiable only because without it scientific research cannot be carried out. If we consider 'mental energy' as spread far wider than (and not necessarily always associated with) consciousness, there is no part of the nervous system in which we can logically deny its presence. It becomes coexistent with what we term nervous energy, which is itself but a specialised form of the energy of living substance. Because electrical manifestations are all that we know of nervous energy, we are not justified in saying that there is naught but electric energy in the activities of the nervous system. So too, in living matter generally, while physical and chemical reactions are undoubtedly present, who can be certain that we shall not be compelled one day to recognise that such physical and chemical energy is harnessed to, nay rather is manifest as a degradation of, still higher forms of inherent energy, which distinguish the living from the lifeless body? In particular, may not the electric responses accompanying nervous impulses be merely the result of degradation of such a higher form of energy, and may not this 'psycho-neural' energy, as I propose to term it, take on a more specifically *mental* form, as higher nervous tissues, especially the cerebral hemispheres, become involved, and as these higher structures come to be distinguished by increasingly plastic, interdependent and complex functions, as contrasted with lower functions relatively fixed, isolated and simple?

Does mental activity differ so widely from material, above all from vital, activity as hitherto philosophers have taught, and as the general and the scientific public still suppose? Let any one who wishes to answer this question impartially consider the nature of matter according to the most recent hypotheses of physicists. What remains of substance but localised, corpuscular centres of electrical energy, set (perhaps it may be

necessary to suppose) in a frameless, almost inconceivable ether? And what is to prevent us in the future from supposing that such energy may be raised to still higher powers in the form of consciousness? I know nothing, not even the doctrine of conservation of energy, that would make such an hypothesis *a priori* impossible.

What psychology and physiology will have to face is a common problem,—that both in life and in mind there is something which differentiates them from the activities of lifeless substance. A living organism is continually engaged in building up higher from lower forms of energy, in making living substance which is continually breaking down into simpler bodies and liberating simpler, more degraded forms of energy through its own ceaseless activity. Outside the living body, such constructive anabolic processes fade into relative insignificance or occur only under artificially arranged conditions. The lifeless world is characterised by a degradation of energy rather than by progress into higher and still higher forms. *Pari passu* with this distinction, which becomes more evident as we ascend the evolutionary scale of living forms, purpose comes more and more prominently on the scene as something superadded to the apparently blind mechanism of isolated non-living bodies. The living organism is characterised by a struggle for self-existence; it *selects* its environment for the purpose. And it is this 'purpose' that characterises both life and consciousness, dependent on a plasticity, an adaptability, which increases from the lowest forms of living matter to the highest and most developed regions of nervous matter, beginning with a 'purposefulness' imposed on the organism, ending with a 'purposiveness', *i. e.*, a self-consciousness fully realising the existence of purpose, as well as of mechanism, in its activities.

Both physiology and psychology can make, and have made, progress on the supposition that our vital and mental functions are determined by mere mechanism. So they are,—but that is not the whole story; it is a mere abstraction essential, as I have already said, for scientific research. The imperfection of this view is becoming increasingly glaring. All that natural science can say is —given such and such conditions, such and such results must follow. But prophecy of what will follow is possible only within the narrowest limits; it can only be determined by, and based on analogy from, past experience. Who could have foretold that the properties of hydrogen and oxygen, when chemically combined, would yield the totally different properties of water? Who could have foretold the properties of radio-active substances, or the appearance of new living forms in the course of evolution? When once we know all the conditions [illegible]

mechanical standpoint of natural science, and say that, given a repetition of certain conditions, certain results can be anticipated. But are the conditions and their results given merely by chance? Is there truth only in our experience of mechanism, and is our experience of purpose, of the employment of means to an end, a mere illusion? Are variations in living forms merely continuous, blind, accidental and preserved by their suitability in the struggle for existence? Or may they not arise by sudden discontinuous leaps and bounds, and be indicative of some perpetual directive attempt at harmony between the environment and the responses of the organism? Are the marvellous instincts, say in the insect world, explicable by chance variations? Are they even explicable as the inherited habits of acts determined and learnt throughout countless former generations?

Few thoughtful modern biologists would be satisfied with a reply to such questions in the affirmative. Determinateness undoubtedly exists in life, but it is only a part of the whole truth. So it is in mind. We may agree with those psychologists who assert that, given such and such conditions, such and such results must occur. But can we say—given such and such an end, or an obstruction thereto, such and such behaviour will appear so as to achieve that end? Clearly, prediction is possible only within the narrowest limits. Creation, the construction and differentiation of the new, is always proceeding in mental, as well as in biological forms. New thoughts occur, both as conscious creations by the purposive self, and as the 'sports' or inspirations of genius, unconsciously elaborated and presented to the conscious, more fully purposive self for intelligent criticism and acceptance or rejection.

The physicist has always endeavoured to foist physical conceptions on to the psychologist. Fechner endeavoured to change Weber's law from its actually relative to an absolute character. Since then physicists have largely adopted the standpoint of relativity which has long been so prominent in psychology. Psychology has been silently insisting that physiological inhibition is not the mere negation, or absence, of activity, but is itself an active process. So, too, in the future I have no doubt that physical science will recognise that there is more in heaven and earth than is now dreamed of in physical philosophy; that mere mechanism is only a partial expression of the energy of the universe and that, in addition, there lies a directing, differentiating factor immanent in the universe as a unitary system, still further developed in the unitary system of the living, individual organism, reaching its climax in the unitary system of the mental conscious, individual self. Mechanical energy is the blind, purposeless activity that makes a train travel along its rails. But there must be an 'energy' responsible

for a higher kind of work, which directs the train now along one, now along another of alternative paths, in conformity with the needs of the environment and with the preservation and evolution of the universe, the living organism and the conscious self. Is it too fanciful to see the germs of this view in present-day attempts to combine into one comprehensible scheme the two conflicting, but necessary, hypotheses, the corpuscular and the undulating conceptions, of light, the corpuscles apparently representing the luminificent machine, the other the directive vehicle of its energy?

With the first appearance of consciousness, purposefulness, as I have previously stated, becomes translated into purposiveness. By that I meant that purpose not only exists in the universe, but that the living organism becomes conscious of purpose in its own activity. Consciousness, we shall come to recognise, has been evolved for the prime object of purposive choosing,—the choosing of the best of alternative responses, and the choosing of the best of surrounding stimuli. What there be of rudimentary choice in lower mental systems becomes developed, differentiated, distilled as it were, within the highest mental systems. Behaviour or conduct in the living organism is to be regarded not, fundamentally, as a mere blind mechanical conflict between purposeful instincts, each with its own driving impulse towards activity; it results rather from the sanction of the directive, purposive self, the function of the highest and most plastic, complex regions of the nervous system. In illustration of this, let me point out that an instinct differs from a reflex in that it is imperfect in performance on the first occasion of its appearance, and in that it is improvable by experience. Thus instinct must come to be regarded as involving, and as being inseparable from, intelligence.

The situation which calls for emotional behaviour makes a dual appeal to consciousness—a *cognitive* awareness of the situation, as well as an *affective* reaction manifested as excitement, depression, appetition, aversion, interest, fear, anger, sexual feeling or the like. But none of these affective changes is to be considered as bound up with merely one definite instinctive expression. Thus, to achieve its end, the sexual feeling may manifest itself in various reactions,—by courting, by attack, etc. So too, fear may manifest itself in flight, in (rigid or flaccid) quiescence, in fighting at bay, in clinging to the parent, etc. Moreover, it will be realised that each act that forms part of an instinctive reaction, e. g., [illegible] may be connected with and evoked by more than one kind of situation or affective experience associated therewith, and that many instinctive reactions—for example, those connected with sex and reproduction, form a chain the links of which develop

at different periods of life determined by chemical stimuli (hormones), by external environment, by heredity and by experience.

We see now more clearly the important function of consciousness,—the process of selection from among alternative reactions, which culminates in deliberate choice. This selective function enables the Ego to choose which of alternative reactions to the environment it shall evoke, and—most important of all—to discover appropriate stimuli, that is, to choose its own environment. The Ego is the final court of appeal for all volitional conduct. Aided—even burdened—by its vast experience, and by the interests and sentiments derived therefrom, it may at one time strengthen the impulse of one of alternative motives, while at another it may weakly sanction, or even passively observe, the issue of an irresistible impulse, the development of which may have been unconsciously, but by no means purposelessly, determined in conflict with other antagonistic unconscious impulses. Some day it may even be proved that we inherit mutually incompatible mental characters, one or other of which may be similarly selected or inhibited, now at one period of life-history, now at another, by unconscious direction.

If, on the other hand, each situation gave rise only to one single possible reaction, the sole remaining function of consciousness would be to assure that the situation did produce its own effects. This would be and is achieved by maintaining the situation, by facilitating its effects on the self, and by inhibiting other effects that might interfere with the bodily activities which the situation would naturally evoke,—in other words, by preserving a favourable attitude of the self, or attentiveness. No doubt this function is especially related to the affective modes of consciousness: emotion-feelings have been developed for the very object of preventing other consciousness from entering into the experience of the Ego and from interfering with the expression of those feelings.

The environment, as most now believe, acts on the organism by releasing its internal energy. The striated muscle-fibres and certain sensory end-organs secrete material which is stored up, ready to explode, as it were, after the receipt of the appropriate stimulus. Somewhat similarly, parts of the central nervous system have been regarded as mapped out into mechanisms which are thrown into action on the receipt of the appropriate stimulus. Thus psychologists have come to look on the stimulus as merely a trigger mechanism, which enables the organism to fire off its appropriate sensory or motor response.

But the psychology of the future is likely to see the inadequacy of this view. Muscle-fibres maintain a variable state of partial contraction throughout their life,—exhibiting what is

known as 'tone',—without signs of fatigue. So too posture can be preserved and daylight can be tolerated for hours without signs of fatigue. We are beginning to recognise another living function besides that of intensive momentary explosion,—namely, the function of long-continued extensive 'set' or 'attitude', involving, and involved in, the simultaneous control over antagonistic activities by adequate facilitation and inhibition. Such control we see on a higher plane in the attitude of attention, the long-continued set which at the same time involves an inhibition of themes or acts which would conflict with the maintenance of the theme or act under attention.

At lower levels such reciprocal inhibition is reflexly involved in all muscular action. When the spinal cord calls forth flexion of a limb, there is simultaneously set up inhibition—active relaxation—of those muscles that would otherwise extend the limb. It is important to recognise that actual work is performed in effecting such inhibition. On the higher levels we recognise it at once in the conscious or unconscious repression of emotional experience and in the resistance with which such repressions meet in order, as we say, to re-enter consciousness. But, to revert once again to the spinal level, poisons such as strychnine or tetanus may, as is now well known, convert such reflex inhibition into reflex excitation, whereupon every attempt in the spinal cord to bend or to extend a limb brings in simultaneous contraction of opposing flexors and extensors and hence produces the well-known spasms. Thus the functions of the nervous system at a given point are so 'poised' that at one time inhibition, at another excitation may result. Such reversals are especially characteristic of the cerebral cortex, where stimulation of the same cortical spot may at one time produce extension, and at another flexion, yet at another both extension and flexion simultaneously.[2] Here, once again, we meet with a kind of directive agency superposed on, or should we not say integrated with, blind mechanism; a reversing gear or, more generally, a setting of the points, as it were, now in one direction, now in another, now keeping both open, according to internal conditions and requirements.

I believe that psychology will come to recognise this fundamental difference between 'act' and 'attitude', using the former term to denote the firing-off of momentary reactions, and the latter to denote that prolonged activity of the living organism involving directive setting, and consequent activity in reciprocal facilitation and inhibition.[3] According to this view, inhibition

[2]Cf. Graham Brown and C. S. Sherrington, *Proc. Roy. Soc.*, 85, 1912, 277.

[3]Is it too fanciful to seek an analogy in the physicist's conception of definite forces locking at definite points the molecules in a crystal or the atom in a molecule, and so exerting a directive attitude in regard to the whole unit?

is an active process,—not the effect, as some have supposed, of the mere drainage of energy into those paths which are at the same time excited.

They who hold the latter view are naturally inclined to regard the central nervous system as possessing a 'common fund of energy', which may be drained now in one direction, now in another, according to the needs of the organism. The same conception has given rise to the notion of a factor of 'general intelligence' which can be employed now in conjunction with one specific ability, now in conjunction with another.

But the existence of a general factor of intelligence—which, we must bear in mind, is merely a useful working hypothesis, as yet unproven—may be considered from a different aspect, that of evolution. It is conceivable that the different specific abilities in which general intelligence is believed to play a part may really involve different specific intelligences, which have been differentiated out of such general intelligence and for that reason—because of their common ancestry—share in a common factor or feature.

The idea of drainage of energy of some general ability now into one part of the brain, now into another, is an outcome of the still prevailing view that different conscious processes have their seat in different parts of the brain. Physiologists, or rather neuro-pathologists, have for two generations past been endeavouring to map out the cerebral cortex into various sensory centres and sensori-psychic centres in which they suppose visual, auditory and other sensations, perceptions and images to be respectively developed. I am confident that future psychology and future physiology will discard this notion in favour of the view that there are no separate seats of consciousness in the brain, but that consciousness involves activity throughout a very wide region, if not throughout the whole, of the cerebral cortex, and that such terminology as the 'splitting' of consciousness is inadmissible.

For many years now, as a teacher of psychology, I have been wont to emphasize, by the following illustration, the fallacy of inferring that, because blindness or deafness results when a specific area of the cortex is destroyed or interfered with, therefore that area constitutes the 'centre' in which those sensations are produced. If I had to travel by train from Cambridge (representing the stimulus) to King's Cross (representing the correspondent consciousness), it would be absolutely necessary for me to pass through Hitchin. A block occurring at Hitchin would make it impossible for me to reach King's Cross, but I should not be so foolish as therefore to identify Hitchin with King's Cross,—to identify the seat of the block with the centre or seat of the particular consciousness. So too when a given

cortical area suffers injury and the lesion results in some particular loss in consciousness, we are justified in saying only that the integrity of that area is essential for the development of that particular form of consciousness, just as an open path at Hitchin is essential for me to get to London.

I remember communicating this view some twelve years ago to the late Sir Victor Horsley, and it is noteworthy that within the last few months it has been independently enunciated by Henry Head mainly as the outcome of his researches into aphasia. Hitherto the preconceived ideas of neurologists, together with their ignorance of general psychology and their lack of training in the psychophysical methods,—that inestimable legacy of experimental psychology,—have led them to fit the facts of their cases to their theories. They have thus adduced evidence for at least four centres of word-memory: "(1) the auditory word-centre, where the sounds of words are registered; (2) the visual word-centre, where the visual images of letters and words are registered; (3) a glosso-kinaesthetic centre, where the combined impressions which pass to the cortex as the result of movements of the lips, tongue, larynx and other parts concerned with articulate speech are registered; and (4) a cheiro-kinaesthetic (eupraxic) centre, where the sensory impressions resulting from movements concerned in the act of writing are registered."[4]

I have no doubt that the future will prove the absurdity of the conclusion that different kinds of word memories are registered in different centres of the cerebral cortex. Even a neurologist like Head whose psychological insight is acute may fall into a similar fallacy, as when he attempts to deduce the kind of consciousness which he supposes to be inherent in the optic thalamus. The thalamus cannot be regarded as a 'seat' of consciousness. In the future we shall come to recognise that there is only one consciousness—the consciousness of the self—and that there are no special seats of consciousness, although different areas of the cortex and the thalamus are differently involved in producing special kinds and characters of consciousness. But the demarcation of these areas is far from constant, and if they be but slowly destroyed other areas may readily take their place. Even after relatively small sudden lesions, and even after the loss of a considerable amount of cerebral cortex, remarkable recovery may still take place.

So far we have been dealing with the views likely to be held by psychologists in the future. Meanwhile psychological re-search [illegible] proceeds, dividing up investigations, just as [illegible]less research divides the investigations in other sciences, into

[4] *A System of Medicine*, edited by Sir Clifford Allbutt. Article 'Aphasia' by Bastian and Collier.

compartments of specialists, each unfortunately so separated from the rest by water-tight doors that workers find it difficult to obtain a good view of the progress of the subject in its entirety. This differentiation seems likely only to increase in the future; it is only to be hoped that the accompanying isolation may be reduced. In one direction we see the workers in laboratory psychology, dealing with problems of general psychology; in another, psychologists concerned with behaviour, human or animal; in a third, those studying individual and racial mental differences; in a fourth, those comparing the behaviour and the institutions of social units; in a fifth, those investigating the mental changes which may be associated with disorders and diseases of the mental and nervous systems or may occur from accidental or experimental lesions; in a sixth, those engaged in determining the nature, number and interrelation of 'general', 'group' and 'specific' abilities; in a seventh, those studying the best conditions for the exercise of those abilities in the individual or for their transmission to future generations. We may even foresee a group of psychologists occupied in scientifically examining the purposeful unconscious mental archetypes, supposed to occur by Jung, or the spiritualistic claims of what is known as 'psychical research'.

And so we reach an aspect of psychology which has hitherto received no attention in this paper,—its application to everyday and possibly to future life. The prospects of Applied Psychology are already so dazzling that from very fear of exaggeration one hesitates to attempt an estimate of its possible developments.

For even already the progress in Industrial Psychology has clearly demonstrated the assistance which can be rendered by vocational guidance, by vocational selection and by the study of the worker's movements, his spells of work and rest, and his environment, in enhancing not only the health and the happiness of the worker but also the efficiency of his work. With the right man on the right job, with his needless efforts eliminated, the most advantageous arrangement of his material, the most effective distribution of his hours of work and rest, and the most healthy conditions of light, temperature, ventilation, etc., installed, what a Paradise during man's working hours can be envisaged!

Not less promising are the advances in abnormal and educational psychology,—especially in the prevention and treatment of mental and moral disorders, in replacing the current, often unjust, but easily workable conceptions of crime, responsibility and punishment by views that are truer, and more generous, however unwelcome to psychologically untrained minds; in providing suitable education and social environment

"mechanism inverted." The mechanists have preferred the principle of chance as the basic principle for their constructions, and the finalists have declared themselves in favor of a supra-personal principle. They have dichotomized existence into absolute chance on the one hand and absolute teleology on the other. Both are deterministic: mechanism gives a determinism 'from below' and finalism a determinism 'from above.' I shall attempt to show that as far as the reality of freedom as a psychological experience is concerned, both theories are incompetent. Secondly, I wish to present my reasons for the belief that the true ghost to be exorcised is the 'passive fallacy' of intellectualism, which is responsible for both of the abortive substitutes offered in the name of selfhood in the form of mechanism and finalism.

What are the factual data in the lives of men that give rise to the idea that they are personalities? Centrally, I believe it is the inexorable truth that *selfhood is coextensive with the perpetual making of labored choices.* It is this striving nature of life that neither the laboratory of science nor the logic of intellectualism can understand. Science by the very nature of its method is bound to find only chemical machines in organisms. Now all machines have every choice decided for them in their very construction; and as for labor, they are one and all completely yielded to their inertia. How, then, can any scheme of mechanism embrace that most typical characteristic of life, its capacity for creative effort? On the other hand, intellect can give us only this mechanistic account inverted, that is, it can only express the labor of life in terms of an absolute teleology. Mechanism and finalism both offer the mere apparitions of life. From either view, choice is unreal, labor is unreal, chance is unreal; and an image so shorn of the warmth of life is not a living personality but a ghost.

How could the unguided chance of science produce the Iliad of Homer? A child, it is said, not knowing Greek, will believe this combination of the alphabet as 'natural' as any. But the performance will seem natural only so long as the child remains ignorant of its meanings. Similarly, when biologists tell us that organisms have been created in this way, we reply that they have accounted for the *how* of a thing by the price of an unanswerable *why*. Science has adopted the theory of chance origins to patch up its deliberate neglect of teleology. That is, since it will not define an organism in terms of its purposes, it [illegible] a thing define its nature? That of a chemical compound apparently does. But this cannot apply to living creatures, be- [illegible] inexhaustible; we no sooner utter its 'formula' than it has grown

apace. To define life in terms of its origin were to relive it; and to live life teaches that above these configurations of the organism there is the stinging experience of labor which fits into no logical mould. Life indeed lives by a rule of chance, but it is very different from the mathematical chance with which the modern biological atomist trades. There are two distinct kinds of chance: the chance of getting a certain result when a given number of results are possible, and the chance of getting a result desired when the number of alternatives is not given. The first sort of chance belongs to mathematics, and the second belongs to life. We are astonished that a mathematical chance combination of words should give us the Iliad because our reason is easier to mislead than is our intuition, and it is our intuition that is in this case astonished. The particular configuration of words that gives the Iliad may be as natural as any, but the meaning they express is a most unnatural result—unnatural because the meaning could not have been present as a given possibility. The presence of meaning and beauty in the poem could have arisen only as all beauty and meaning arise, from the law of chance which is at once the law of life: the chance "to be or not to be." Thus, between the 'cannot happen' of mathematical chance, and the 'has happened' of finalism, all dynamism is stranded, all psychology becomes scepticism. In a scheme either of pure chance or of absolute teleology, choices are impossible, and to make choices is coextensive with life. In support of this thesis, I shall now parallel this discussion of the results of the theory of chance with a discussion of the implications of the teleology of the absolutist philosophers.

We may take Hoernlé as a fair representative of those who invoke "the order of nature," as they call it, to resolve the enigma of life. Hoernlé completely rejects vitalism "whether biotic energy, or entelechy, or *élan vital*—which yet has the power by way of regulation or control with the physico-chemical processes in the body . . . which can select for realization one of the physically open possibilities[1]." We shall see that with this rejection of psychic causation and the psychic fact of choosing he has already yielded the entire debate to the mechanists; for teleology can under these circumstances only be mechanism in euphonious disguise.

Presently Hoernlé defines his conception of teleology: "When we speak of teleological concepts, we do not mean a design, or plan, or purpose, or desire consciously entertained by any mind, be it of God, of man, or of plant." Here the amazing contradiction he enters upon is complete. By the use of the concept *value*, he says, the efficient cause of mechanism becomes

[1] *Studies in Contemporary Metaphysics*, 1920, ch. 6.

the final cause of teleology. Yet he assures us that a value is not such because it is desired in any way. It is valuable because it serves a purpose, but purpose too means something different from what ordinary mortals experience, being immanent and unconscious. We are of course curious as to the meaning of this 'immanent purposiveness' and we are soon to learn. "When we ask," he says, "what character in natural objects, or in nature as a whole, exhibits this immanent purposiveness, this 'design' most clearly, the answer must surely be that it is organization."

What shall we say of this so-called solution? Hoernlé was right in saying that the difference between a machine and an organism is a certain teleology in the latter. But his teleology is meaningless because it is one without a problem, without real choices, and accordingly without life. His 'organization', like a machine, has purposes, not for itself, but for some other agency. Like a machine, it has all of its problems specified and solved in advance. In truth, in place of a machine, Hoernlé gives us mechanical theory. Vainly does he try to show his concept of the organism as different from mechanism. A machine involves *causes* and *effects*, he declares, while an organization, which is the pattern of life, involves *means* and *ends*. Yet, when we ask what are the means and what the ends of the organism, to remain consistent with his definition of teleology he declares that the parts of an organization are the means and the whole is its end. What strange being is this whose sole purpose or end in existence is to be the sum of its parts? This is surely too mild a rendering of that turbulent drama which we call life. Most paradoxical of all, not only is this as much a ghostly distortion of life as the mechanism which Hoernlé rejects, but he also ends by admitting the vital principle he abhors. He set out to construct an immanent organization that is without "design, or plan, or purpose, or desire;" but is the exercise involved in being an 'organization' so mild as all this? A physical organization could indeed persist with no expenditure except inertia, which is of zero value. Hoernlé is of course seeking to render a consistent account of life as an organization of ideal order; and as such it maintains its integration only by the effort of memory. It will be apparent, then, that we have here all the dynamic strife of life disguised in words. Again the 'passive fallacy,' which refuses to see that the recalcitrant facts of freedom and responsibility, which the intellectualist abhors because they upset all prediction and all logic, must be explained and not denied.

As for our freedom, it is perhaps because simple fiats of will are so ordinary in our lives that they have given rise to that threadbare enigma of psychology, the problem of determinism.

To the query of St. Paul "Hath not the potter power over the clay?" daily experience gives an unqualified affirmative. But dialectic has almost invariably ended with a determination of either a 'hard' or a 'soft' variety. A volitional act possesses an unique character to which mechanical schematism is wholly inapplicable. It is impossible to imagine that the units of an interacting system exert effort; on the contrary, each of them is completely yielded to its inertia. To be in space is of necessity to be subject to all of the enforced movements of space. Every body follows the path of least resistance, we say; but even here we dramatize, for the least resistance is at least *some* resistance. Here is an intrusion into our purest notion of the mechanical, the ineradicable belief that wherever there is motion there is life. However closely we examine the intervals between changes of physical state we shall never discover a *cause* in its true sense. All we shall find will be intermediary states as before. Thus the gaps between states of change, instead of being bridged, become more numerous than before. Cause cannot be discovered in physical nature as such because *the bodies of space are incapable of initiative.* We shall never be able to conceive of causation in the true sense of the word unless we seek for it, not in the realm of the mechanical, but in the realm of the vital. The experiences of causation and of time are inextricably bound together; and so intimately are they parts of life that they are in a true sense life itself. Hence the volitional and consequently moral scepticism of Hume and all others who seek to comprehend life and responsibility without admitting the reality of time. Bosanquet argues that true causation is independent of time, since the more completely we know the ground of an event, the less significant time is[2]. If finding causes merely consisted in finding antecendent conditions and events, then we are logically compelled to say that the universe is the cause of all things. This, the conclusion of J. S. Mill, is practically saying nothing at all, for it leaves us none the wiser concerning the particular causes we are in search of. Practically, we never look for all of the antecedent conditions, but single out a few that must serve by proxy for the universe as a whole. In the classic example of the cow that set a city on fire by kicking over a lantern we single out the cow and the lantern as the causes and wholly ignore the city and the rest of the universe, because this limitation lends itself nicely to the problem of the prevention of fires. So in the courts of justice: a stone dropped to earth will jar the entire universe, but the judge confines his survey of effects to the unhappy person who was struck by it. What rule decides which particular antecedent factor will be selected to be the 'cause?'

[2] *Logic*, bk. I, ch. 6.

Clearly, we designate that antecedent as cause which we can control. Thus, it is only as a metaphor that we speak of causes in nature at all, for in all such instances the true causal agency literally intended is a living one. *It is only mind that can cause and it is only mind that can be effected in any intelligible sense.* To be a cause is far more than to be an antecedent state: to cause is to labor and to labor is to overcome resistance. Labor is the price of all creations. The God of theologians is said by them to be able to create universes at the mere behest of thought; but the inconsistency arises from the brute fact that thought too costs an effort. Moreover, it is an effort made under the hard terms of a law of chance which gives no assurance of victory. We have here the true realism of chance and the true realism of life. The chance which science has conceived is a chance with a calculus; but the chance by which we live is real, not because its history can be computed in advance, but rather because it cannot be so computed. It is a realism fully expressed by the phrase "to be or not to be." By what magic does the chance taken by life upset all prediction? It is always because life seeks to transcend the bounds of the given; that is to say, it seeks to transcend the bounds of repetition. Again, this is to say that this sort of chance cannot be escaped because of the effects of time, for it is time that makes exact repetition impossible. If the tropic responses of the hydra will not fit into the statistical mould it is because the hydra learns. Yet it learns so slowly that there is sufficient repetition to make a law of probability of some use in predicting its conduct. What of a new coin, which at first satisfies the expectation 'fifty-fifty' but progressively disappoints it because its edges wear on one side or the other? We must say that the formula is not realized here because the coin, like the hydra, is subject to the effects of time. The hydra progesssively learns while the coin progressively "unlearns" the form given it. When we cast loaded dice, we succeed each time, thanks to 'the uniformity of nature.' When we cast unloaded dice, we may still succeed if we are patient; here chance loses its calculus, and, for once, chances have to be 'taken.' The law of chance is indeed none other than a law of patience; only in this sense can it be truly empirical. It is indeed the most ancient of all the laws of life. It is the "trial and error" of the amoeba which the philosopher too employs where the majority of 'concepts' created in hope are destined never to endure. And of mathematics too it may finally be said that its true business is to take chances [illegible] to define them. It is a notorious fact that never in its empirical researches does science get results that exactly correspond with what chance prediction warrants. In truth, as Poincaré has admirably shown, the very notion of [illegible]

is a paradox. What would be the chance of selecting out of an urn a white ball when it contains only white and black ones but it is not given how many there are of each? There is no calculus that can tell us. Yet it is just such problems that we must solve as the price of evolution to a freer life.

Conclusion.—In the acute form in which the problem of individuality appears today, a thousand issues of the past have come to a head. The decision between the three alternatives of mechanism, finalism and vitalism does not rest finally on reason, but rather on a sort of conscience within us. The problem at issue is not a logical contention concerning the existence or non-existence of mind. The logical differences concern only the question which of the three hypotheses is best suited to account for the facts of mental life. It has so far been the purpose of my treatment to show that neither mechanism nor finalism can render a consistent account of life. Yet the results of such a criticism are wholly negative so far as the positive meaning of life is concerned. For that positive understanding I believe we must go to immediate experience and to introspection. That we are free and responsible, that we labor and risk failure, were not taught us by dialectic. The whole history of philosophy is a monument to the truth that there are, as James expressed it, certain views of the world in which we cannot "feel volitionally at home."

Thus viewed, vitalism alone justifies the reality of will, of pain, of effort, and of decision: all of which are constantly present in life and consciousness. Mechanism awakens a quick rebellion, not so much because of its fallacies as because of its history. It has been invoked by pessimism to explain its failure, by gross hedonism to justify its weakness, and it has given plausibility to the ambitions of oppressors. Thus, in its history as well as in its principle, there appears its incapacity to represent the phases of experience that make up the richest part of life as it is actually lived. Finalism, on the other hand, seeks to escape the weaknesses of mechanism by the futile expedient of accepting its essential meaning under the guise of the more innocent baptism of 'immanent organization' or some such euphonious term. Like Plato's spectator we are bound by a chain that somehow we cannot make ourselves believe is an apparition like the other things about us. This is the chain of loyalty to something creative within us, a chain which to lift is heavy labor.

PROGRESSIVE RELAXATION

By Edmund Jacobson, M.D., Ph.D., F.A.C.P.

For the last six years, a new method to bring quiet to the nervous system has been in clinical use with more than sixty patients. The method first suggested itself during experiments on the involuntary start begun at Harvard University in 1908. Related studies were later continued at Cornell University,[1] and recently, thanks to Professor Carr, at the University of Chicago.[2] Neither these experiments nor the method itself are to be featured in the present paper, but rather certain observations concerning the quieting of psychological activities. Clinical records have been made with great detail, and the interests of investigation rather than merely of therapeutics have been paramount. In particular, about ten qualified patients received a specially prolonged training in psychological observation; and their reports were supplemented in 1922 and 1923 with the aid of thirteen subjects in the psychological laboratory and two in the physiological laboratory.

The problem of diminishing cerebral activities may prove of interest to every psychological school of thought. Students might differ in describing the purpose of the present investigations. Clinicians might see it as a search for a sedative for overactivity of the cerebrospinal and autonomic nervous systems, or for a means to reduce nervous irritability and excitement. Others might say that the search was for a physical means to diminish the psychic processes of an individual at a particular moment or over a period of time, including what James might have called the stream of consciousness, what Wundt and Titchener might have called the state of attention and perhaps also the processes of mental imagery and kinaesthesis, what Marbe and Ach and Watt might call the thought-processes, what Watson and the behaviorists might term subvocal speech, and what every one knows as the emotions. In all of these various expressions for the whole or some part of experience, there is one point of general agreement: each of these writers conceives the occurrence he names as having essential physiological accompaniments. It might therefore be possible to find among such various physiological accompani-

[1] E. Jacobson, this Journal, 23, 1912, 345-369.

[2] A systematic account of experiments on the involuntary start will soon be published by Miss Margaret Miller.

ments some universal component, which might be diminished, or momentarily eliminated, in order to produce a corresponding psychological effect. Doubtless, a study of the data later to be presented will make the problem more clear and open to interpretation in the terms of the reader's choice and experience.

Interest may perhaps be added to the present report, if we first recall some views of various authors that harmonize with our results. The writer is not seeking to prove a motor or any other theory of consciousness, nor was Külpe when he said:

> "It is important to emphasize...that reproduced sensations are by no means the only aid to recollection... Very intensive impressions are usually sensed not only by way of the organ to which they are adequate, but by others as well. *Movements are everywhere important.*[3] It is perhaps not too much to say that a voluntary recollection never takes place without their assistance. When we think of intense cold, our body is thrown into tremulous movement as in shivering; when we imagine an extent of space, our eyes move as they would in surveying it; when we recall a rhyme, we mark its rise and fall with hand or foot. Most important of all, however, are the movements of speech, which stands in unequivocal relation to the perception of every department."[4]

It is but a step from Külpe's views if we should find that the intensive relaxation of movement brings with it a subsidence of voluntary recollection and reflection.

Wundt recalls that Fechner observed during attention to an outer stimulus in the sense-organ—for example, in the ears during hearing, or about the eyes during vision—a slight tenseness (*eine leise Spannung*). Assuming that Fechner was right, it would seem logical, if a means might be discovered to relax the tension, that thereupon the attention to the outer stimulus might be diminished or done away with altogether. Such an assumption would harmonize with our results.

It seems unnecessary for the purposes of the present paper to discuss the views of Wundt, beyond recalling in a general

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[3]Italics mine.

[4]*Outlines of Psychology*, translated by E. B. Titchener, 1895, 187. We are warranted in inferring that, according to Külpe, reflection likewise never takes place without the assistance of movements, for later he adds: "Reflection, psychologically considered, is nothing else than a more or less complicated series of reproductions, associatively originated, and possibly abbreviated by the exclusion of intermediary terms". (446). It need not concern us here that later experiments in Külpe's laboratory disclosed to him that in reflection there may be, in addition to reproductions, certain unanalyzed mental elements; for even if reflection does not take place without these more recently discovered elements, it is nevertheless beyond question that reproductions themselves are a *sine qua non* of reflection, according to Külpe; and voluntary reproduction, he observed, never takes place without the assistance of movement.

way that he maintained that feelings of strain and relaxation are always connected with the processes of attention.[5]

A familiar quotation from Binet also comes to mind at this point:

> "It is enough to remember that all our perceptions, and in particular the important ones, those of sight and touch, contain as integral elements the movements of our eyes and limbs; and that, if movement is ever an essential factor in our really seeing an object, it must be an equally essential factor when we see the same object in imagination (Ribot)."[6]

The well-known observations of Lange and James also will be recalled in connection with the present work. James, for instance, believed that, if in fancy we abstract all bodily symptoms from a strong emotion, there is nothing left behind. First among such symptoms he named "rigidity or relaxation of this or that muscle." However, he lacked an experimental method to test his beliefs. Had it occurred to him to try out the effect of intensive relaxation on the emotions, he would have come upon the method presently to be described.

In the same vein writes Washburn in her book on "Movement and Mental Imagery". In advancing her motor theory, she discusses associations such as occur with a series of nonsense syllables or when a man is about to write a cheque, and she makes what she calls "a crucial assumption for her whole hypothesis,—that there probably are, going on in the muscles, light actual contractions."[7] This theory is similar to the views of Watson and others that thinking is essentially a play of muscles.[8]

Such assumptions as those of Washburn and Watson are theoretical. In the work to be described, the question is investigated whether thinking may take place where there is extreme muscular relaxation. It is not the present purpose to demonstrate any particular theory of consciousness or of be-

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[5]W. Wundt, *Outlines of Psychology*, trans. by C. H. Judd, 1907, 92. Wundt points out that there may be grades of consciousness. Our subjects and patients do not use these terms, but their observations are to the same effect. "Sowohl die psychischen wie die physischen Bedingungen des Bewusstseins weisen uns demnach hin, dass das Gebiet des bewussten Lebens mannigfache Grade umfassen kann. In der That finden wir schon in uns selbst je nach äussern and inneren Bedingungen wechselnde Grade der Bewusstheit, und auf ännliche bleibende Unterschiede lässt die Beobachtung anderer Wesen schliessen. In allen diesen Fällen gilt aber die Fähigkeit der Verbindung psychischer Inhalte als Massstab des Grades der Bewusstheit. Sobald wir Eindrücke nur mangelhaft in den Zusammenhang unserer Vorstellungen einreichen oder uns ihrer später wegen dieses [illegible] [illegible] uns während der betreffenden Zeit einen geringeren Grad des Bewusstseins zu" (*Grundzüge der phys. Psy.*, 3, 1903, 324).

[6][illegible] W. James, *Prin. of Psych.*, [illegible]

[7]M. F. Washburn, *Movement and Mental Imagery*, 1916, 32.

[8][illegible]

havior, but rather to submit a set of observations, which each school of thought may assimilate as may seem fit. Furthermore, it seems unnecessary to enter upon a complete discussion of motor activities and conscious processes, for the conclusions that we shall draw will rest upon our observations, rather than upon previous theories.

The Technique of Progressive Relaxation

Since a detailed description of method has recently been published, only a summary will be here repeated.[9] Needless to say, the method is best learned by the experimenter in person, rather than from a written account.[10] In this respect it is evidently like any other concrete laboratory or clinical method.

The individual is to learn how to relax the principal muscle-groups of his body to any extreme degree. Obviously this implies the reduction of impulses along motor and associated portions of the nervous system, since it is generally agreed among physiologists that there are no specific inhibitory fibers to the striated muscles.

The relaxation is progressive in three respects. (1) The *S* relaxes a group, for instance, the flexors of the right forearm, further and further each minute. (2) He becomes acquainted in a certain order with the principal muscle-groups of his entire body. With each new group he simultaneously relaxes such parts as have previously received practice. (3) As he practises from day to day, he progresses toward a habit of repose. He tends toward a state in which quiet is automatically maintained. This is not to be confused with what has hitherto been known as relaxation or what is popularly called relaxation. For when the unpractised person lies as quietly as he can on a couch, close observation of external signs shows that the relaxation is incomplete. There remains what may be termed *residual tension*. The same may also be inwardly observed, through kinaesthetic sense. Doing away with residual tension is the essential point of the present method. This may not happen in a moment, even in the practised person. Residual tension only gradually disappears; it may take 15 min. progressively to relax a single part, such as the right arm. The desired relaxation begins only at the moment when the individual might appear to an inexperienced observer very well relaxed.

Progressive relaxation is called "general" when the individual lies with closed eyes and relaxes all over. It is called "relative" when he is active, but is relaxed so far as possible during that activity: for instance, the individual sits in a chair and relaxes as far as possible in this posture; or again, he converses, but relaxes so far as he may while doing so. The relaxation is not carried so far that it interferes with the activity of conversing, but those muscles whose activity is not necessary for the action are relaxed, and also a partial relaxation of the muscles in use is secured. A third form of progressive relaxation is "specific relaxation", to be described later. Our present account will deal chiefly with general relaxation.

The individual to be generally relaxed lies upon a couch in a quiet room with eyes closed. If the *E* is relaxing several individuals at a time, he proceeds noiselessly from room to room. This was done at the Univer-

[9] *Journal of Nerv. and Men. Dis.*, 1924.

[10] To coöperate with laboratory investigators desiring to repeat the present observations, preliminary instruction may be furnished in Chicago.

sity of Chicago, where at each session 4 to 8 *S*s practised simultaneously. In laboratory work the period lasts from 1 to 1.5 hr. From time to time during the hour the *E* notes on his record all the instructions that have been given and whatever observations have been made. Therefore it is important to have some form of ready record sheet.

Initial Instructions

The initial instructions given in 1923 at the University of Chicago were as follows: the individual was simply instructed that he was to learn to relax to an extreme degree. Therefore it was useful first to show him what it is to be tense. Lying down with eyes closed, he flexed the right or left forearm, while the *E* held it back in order to make the contraction more marked. The individual then reported whether he noted the sensation of tenseness and where. When successful, he was informed that this experience was by mutual agreement to be called "tenseness."[11] Whenever this same sensation or experience appears in this or other parts of the body, it is to be called "tenseness."[12] As the *S* contracts the part, it is said to him, "This is you doing! What we wish is the reverse of this,—simply not doing." It is of the greatest importance to inform the *S* that he is to make no effort to relax, for, as he finds, making an effort is being tense, and therefore is not to relax.[13]

Since the stimulation of discussion is to be avoided, few words pass between the *E* and the *S*. From time to time, when the director returns to the room, instructions are given as tersely as possible. Never telling the *S* suggestively what is happening or what will happen, the director avoids the use of suggestion. For instance, it is not said, "Now your arm is becoming limp!" or "Your arm will become limp!" Our experience shows that the presence of the director often proves disquieting, and that it is necessary to tell the *S* not to bother about the other's coming and going, but just to keep on relaxing, or to relax all the more when disturbed in this or any other manner.

The Cultivation of the Muscle Sense

As an aid to the individual in relaxing, we have cultivated the muscle-sense.[14] When a muscle-group first receives practice,

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[11]No distinction will be drawn in the present paper between the sensation and the image of muscular contraction. Either will be called "tenseness", or "the experience of muscular contraction."

[12]The well-known "stimulus error" is of course avoided in all of the present work. The *S* is trained to report whether or not he notes the presence of a certain sensation. This sensation was studied by R. S. Woodworth in his work *Le Mouvement*, 1903. The *S* is not at any time asked, "Is your muscle contracting?" but only, "Do you perceive the sensation?"

[illegible]

quite similar to learning any other physical art, such as billiards or dancing. Here, also, effort spoils the result, which must be secured in a relatively relaxed fashion.

[14]The sensation from muscular contraction is described by our [illegible] a rule as dull or faint, readily obscured by other experiences, fairly localized,

the *S* contracts it, and then reports his experience.

On the *S*'s record sheet, + means a positive report with successful localization, − means failure, some number represents the number of reports, S indicates that he reported spontaneously with no hint from the *E*, while I indicates that the location of the contraction was pointed out to him.

The method used to cultivate the muscle-sense, so that faint tensions are recognized and correctly localized, has been previously described. The trained individual, as compared with the beginner, appears to have a much increased awareness of his sensations of muscular contraction and their locality. During activity as well as partial relaxation, he knows where he is tense, in a kaleidoscopic way, and his reports can be checked with objective observation.

The Objective Estimation of Contraction and Relaxation

This has been previously discussed. It includes palpation, passive motion, observation of respiration, of apparent flaccidity, of absence of movement or contraction, graphic records, X-ray films, the characteristic vacuous appearance of face and open eyes when relaxed, and other tests. Further work in this respect will soon be published.

Sample Records

It seems sufficient here to summarize two records, since they are typical of practically all of the others who learned to relax well.

S P. W. There were 16 periods of about 1.25 hr. each, beginning Jan. 8 and ending Feb. 16, 1923. He stated that he had a little knowledge of anatomy. On the first day the initial instruction was given and practice was devoted to the left biceps alone. At first he failed to note tenseness there even when instructed, but finally succeeded −2 +1 I. Although he became fairly quiet for a beginning, the eyelids winked frequently. At the second period, all the muscle-groups of the arms were covered. He noted tenseness in the left arm and the right biceps + 1 I, but toward the end of the hour succeeded upon the first attempt spontaneously with other groups. On one occasion when the *E* entered the room it became obvious that *S* did not really know what was desired, for he lay upon the couch with eyes open. Thereupon the directions were given *first to make the whole arm stiff, but without flexion or extension; next to let go a little, then on and on, past the point where it seemed to him fully relaxed.* At the third period the muscle-groups of the legs were covered, and in some cases he succeeded in recognizing tenseness +1 S. He lay fairly quiet and did not commit the error of opening his eyes. Upon arising at the end of the period, he appeared somewhat drowsy. The fourth period was devoted to the trunk and shoulders. He noted tenseness +1 S, upon drawing in the muscle-groups of the abdomen; similarly in the intercostal muscles upon inspiration, and

but diffuse and ill-outlined, neither agreeable nor disagreeable, but particularly indistinct and characterless. It is to be distinguished from other somatic sensations, due to touch, warmth, cold, tickle, and pain. These distinctions are familiar to the student who has had the average elementary course in experimental psychology.

in the *erector spinae* upon bending the trunk backwards. When the arms are drawn forward, the pectoral muscles come into play. This was noted -1 $+1$ S. Upon drawing the shoulders backwards, he noted tension posteriorly -2 $+1$ S. Shrugging the shoulders is one of the tests for ability to localize tenseness, for the average *S* does not know that the muscles for this act are partly in the neck. Upon shrugging, the present *S* noted tenseness -1 S$+1$ I. The fifth period was devoted to the neck, for he usually lay with his head held stiffly. He was instructed, "Let all parts go as limply as a rag! When they become so, then whatever you have done up to that point, continue on and on!" Later, when he still held his neck stiffly, it was said to him, "Let the head go just like a rubber ball!" At the sixth period he was directed to wrinkle and frown, and noted both $+1$ S. For the first time he now began to appear relaxed. When the *E* raised the arms, they fell limply.

The next three periods were devoted chiefly to the eyes. Since this stage is important, it will be described in detail. The *S* lies with eyes closed, and is instructed to close the lids still more tightly, when the usual question is asked, "What took place?" As a rule the correct reply is made that tenseness is felt in the eyelids. Next, with eyes still closed, he looks to the right and then reports. If he mentions a feeling of "tenseness or muscular movement here," indicating the eyeball, it is recorded $+1$ S. This is repeated looking to the left, then up, then down. Later the instruction is given, 'Just to let the eyes go, not to bother to look in any direction'. The director notes the degree of success by the flaccidity and absence of winking of the closed eyelids. After practice at relaxing the eyes in this way has continued for perhaps 30 min., it is interrupted and the eyes are opened for the next task. Standing at the foot of the couch about 6 feet away from the eyes, the director holds his index fingers on a level but about 3 ft. apart. After looking from finger to finger horizontally from side to side, the *S* is desired to report what happened.[15] If he reports muscular sensation of eye-movement, the fingers are neared to about one-half of the former distance, when the *S* repeats the movement and the observation. This is later done with the fingers held about half an inch apart. Next a single finger is held up while the *S* looks at it for a moment or longer. The report is recorded $+1$S if, for instance, he reports that he saw the finger and notes tenseness in the eyeballs as in convergence. Next the fingers are held one above the other and the *S*'s eyes move vertically from finger to finger through the 3 distances. Finally the lone finger is again held up. After each test the *S* reports. At this point it is convenient to give the *S* an example of relaxing the eyes. The director sits with eyes open, where he can readily be seen by the *S*, and lets the eyes relax so that they appear not to be looking. The forehead and brows must also be so relaxed, and the rest of the body, so far as possible, in order to give a clear illustration. The *S* may describe the appearance of the director as empty and expressionless. P. W.'s relaxation of his open eyes was visibly improved after he had witnessed the example.

With the seventh period, work was begun with imagery. The *S* is not told what he is to look for, but lies with eyes closed, and is directed to look in imagination from finger to finger held wide apart, from side to side. As

[15]It must be emphasized that care is taken to avoid influencing the *S*'s observations. He is to give a general report of what took place at a specified moment. Not alone do his accounts often mention elements other than tenseness, but in the advanced work he is not even directed to look for [illegible] to mention tenseness either as present or absent. Precaution is of particular importance in the work on imagery to be described in the following pages. Throughout the work leading questions are to be shunned, but spontaneous observation is to be cultivated.

a rule, when this is done, the eyeballs can be seen to move a little beneath the closed lids from side to side. Furthermore, observations by many experienced patients and *S*s have concurred that there is a feeling of tenseness in the muscles of the eyeballs, quite similar to that when the movement is made with open eyes, save that it is less intense. Accordingly if the *S* reports a feeling of tenseness or muscular movement, the record is +1 S. This was true in the present case. Leading questions at this stage are particularly to be avoided. The same results were had with imagining the fingers at the middle distance and at the smallest distance apart. From our experience it appears that the tenseness which is momentarily present when the stationary finger is imaged is more difficult to note than that when looking in imagination from finger to finger. At first P. W. was doubtful about it, but finally reported it present. Following such practice with ocular tensions, the *S* who is progressing with the method will lie generally limp, and the eyelids will be strikingly relaxed, without winking. As a rule, he who has been most apt in recognizing slight tension, is soonest to reach this stage. The next step requires greater delicacy of observation than any of the preceding.

The *S* still lies with eyes closed. The instruction is tersely given: "Imagine a motor-car passing." In the present instance the *S* simply reported that he had no definite experience. The director avoids comment, so as to give no leading questions. The *S* was again requested to imagine a motor-car passing, and thereafter to say what happened. Now he reported a very hazy visual image of a motor-car moving, along with a feeling of tenseness in the muscles of the eyeballs. The instruction to imagine 'a man passing,' and later 'a flower fluttering in the wind' also brought the report of very hazy visual images with slight tenseness in the eyeballs. Following this, a period of about 15 min. of ocular and general relaxation was given. The next task was, "Imagine a bird flying in the sky." P. W. reported "No experience." Upon repetition he reported a vague visual image with slight tenseness in the eyeballs. "Eiffel Tower in Paris" brought the report of a more distant visual image, along with slight ocular tenseness. Imagining 'a tree in the distance' gave similar results.

The ninth period began with a review of imagining a "motor-car passing." P. W. reported that he did not see the car, that he had no experience. "A man passing" brought the report of a visual image of a man with a slight tenseness of eyes moving to the left. Instructions to think of 'a blade of grass' and 'a bird in the sky' brought similar accounts. The question was here asked, "What happens when you relax your eyes?" He replied, "There is no tension, no strain, but a tendency to go to sleep." "What else?" He was silent and the question was postponed, to be repeated later.

The *S* was next successively requested to report about what happened in each of the following acts: smiling, pouting, protruding the tongue, retracting the tongue, closing the jaw tightly. In each of these instances he reported tenseness in the correct place. A correct report, following protruding of the tongue, is one of the tests whether a *S* is really succeeding in localizing tenseness, for otherwise the *S* (who does not know anatomy) is not likely to report tenseness both in the tongue and in the floor of the mouth.

At the tenth period, P. W. was instructed to count to ten, whereupon he reported tenseness of the tongue and lips, jaw and larynx. Instructed to whisper from one to ten, he made the same observations, except that the tensions were not so marked. All this was with eyes closed. Upon whispering half as loudly as before, there were analogous results. He whispered so that the director could just notice the slightest movement of the tongue and lips. This brought similar reports, save that the tenseness was much diminished. The next request was to repeat the act, but so faintly that it could not possibly be seen. He now reported that the feeling of tension in

the tongue, lips and larynx was still present, although very faint. He was requested to repeat the act, but very much more feebly still. He stated that he was able to note this tension only in the larynx and tongue. Following this, the usual instruction to relax was given, including the eyes and tongue and larynx and the like.

The next exercises were with auditory imagery. Entering with no previous announcement, the *E* tapped repeatedly but lightly with a pen on the table to the right, and then asked if the *S* could report what happened. Reply was made that the eyes moved toward the sound. Tapping in front and to the left of the *S* brought the same reply. In some of these instances it was possible for the director to observe the eyes move under the closed eyelids. Following this, the *S* was asked to imagine the sound of tapping. He reported that he had a similar but less intense experience, auditory images with tenseness as from ocular movement. Instructed to imagine the sound of a motor-car on the Midway, he later reported that he had seen the car, with no auditory image, but with slight ocular tension. He was instructed to imagine the sound of a horn in the distance, but replied that he failed to image it. Upon the direction to imagine a running faucet, he reported the image of a groaning sound with a feeling of tension in the ear-drum, as well as a visual image of a faucet with a faint tension in the eyeball. Requested to imagine the voice of a certain university instructor, he replied that he heard the pecular sound of the voice and saw the class room, but reported no tension. After again imagining the sound of that instructor's voice, he corrected his previous report, saying that there was slight tension in the eyes.

At the eleventh period, during attempted relaxation, the *S* still held his head a little stiffly. To make him aware of this, his head was pushed to the side, and he was urged to let it go a little more. After this had been done, he was requested in turn to imagine the following things: a motor car passing; a man passing; 13 times 14. In each case he reported visual images with tension in the muscles of the eyeballs. Next he was requested to think of the relation of interest to knowledge. He reported visual images of the terms and feelings of tenseness in the eyes, but no thought of the relationship. The questions were asked, "What is mean deviation?" "What is the relation of idealism to monism?" "What is probability?" Reports on these complex experiences were evidently beyond the capacity of this *S*. He waited long before replying, and evidently could not report further than to say, "Tension in the vocal apparatus". He was instructed to relax completely, including the vocal apparatus. At the twelfth period his neck was found still a little stiff. The director moved it, and P. W. was instructed to practise letting it go completely. He was asked whether the eyes relaxed completely? He replied that they did so occasionally. He was then instructed to "let go till there is not the faintest trace of tension."

At the thirteenth period the *S*'s arms appeared extremely limp. Without any leading question he was asked, "What happens when you relax your eyes completely?" He reported that relaxation seems general and complete for a brief period during which no thought at all is present; that 'thoughts occasionally slip by' during the intervals when he is not so relaxed; that with complete relaxation of the vocal apparatus there is no inner speech; while with complete relaxation of the eyes, along with general relaxation, there is no attention to sound. In short, with the approach of general complete relaxation, all imagery ceases.

[illegible] perfect his technique, letting go further and further, and increasing if possible the period of complete relaxation.

At the fourteenth period or later he reported that the times during which no imagery was present were increasing in length. He failed to go to sleep during the periods. At the final meeting, the director noted that

he appeared well relaxed, but not to the utmost. The eyelids did not have the perfectly "ironed out" appearance that distinguishes complete relaxation in the most successful *S*s.

S B., graduate in psychology, was present at about 20 periods, ending March 7, 1922. From the outset as a rule tenseness was recorded +1 S. Likewise at each stage her relaxation was relatively excellent. She practised considerably at home, but was somewhat late in learning to distinguish between strain and tenseness. When the arms were extended forward, contracting the pectoral group, she stated that she felt tenseness behind and strain in front. The method of correcting this error is to have the *S* relax, while the director pulls the limp arm forward, thus putting strain in the interscapular area. In the present instance, the *S* then spontaneously noted that this sensation was not the same as tenseness. Apparently her localizing sense was developing, for upon shrugging the shoulders she localized the tenseness +1 S, and similarly with movements of the eyes, open and closed.

Her observations on visual imagination were clear-cut. Upon staring at the finger in imagination, she reported, "A slight feeling of contraction, not so much as when open, along with a visual image of the finger." Requested to imagine a man moving from right to left, she spoke of a visual image, along with a sensation from ocular movement left to right. Imagining 'a motor car passing;' 'a boat passing on the lake;' 'a street-car moving;' 'a man standing still,' brought reports of visual images, with sensations as from contractions of the ocular muscles. Her first account of imagining the Eiffel Tower in Paris was of a visual image, "but no contraction, for I can see it so far off." "A man very far off" brought the report, "I hardly can describe it. There is not quite the same contraction as before; so slight that I can scarcely think of it as contracting." Repetitions with Eiffel Tower brought the report she felt sure she did not note contraction in this case.

When a *S* fails to note tenseness, which the director suspects, the latter must carefully avoid prejudicing the *S*'s judgment, for observation should be spontaneous. After such failure, it is generally better to have a period of general relaxation, and then to return to the exercises. Accordingly the *S* relaxed at this point, and reported that she almost slept. At first there were many visual images, but they gradually went away as she became quieter. She would feel a contraction and would try to get rid of it. Contraction of the eyes and the frown often left together. She added that on the previous occasion she had found that visual imagery interfered with relaxation, for "the eyes shot around towards the things that were thought of". Imagining 'a ball falling' brought the report, "Downward motion of the eyes, with visual image of the ball." Imagining 'a blade of grass' brought a similar account. However, upon imagining 'a green ball in the distance,' she noted a visual image with practically no contraction.

While the *S* was an excellent observer, she still remained doubtful whether tenseness is present in imagining *distant* visual objects. Accordingly she was requested to imagine the green ball again in the distance, but off to the left. A visual image was noted, but this time along with a sensation of slight contraction; similarly when off to the right. Upon imagining it in the center of the field, she observed the image of the ball with a very faint contraction. Images of Eiffel Tower off to the right, and then to the left, seemed to be accompanied with slight contraction, as in the eyes. She said that the contraction seemed less than in the case of the ball, for the tower was further off. When the tower was imaged in the center of the field, she found it difficult to say whether tenseness was present. At the next period she reported that during successful relaxation, visual imagery disappeared. She said that she seemed more likely to go to sleep when the eyes relaxed. During the period she slept. Review was given of looking

from finger to finger with eyes open, and later again in imagination. Further practice was had in imagining a rubber ball about 10 ft. away, and a building in the distance. In each case she noted a visual image, with a very slight contraction. Later she was requested to imagine London Bridge and reported a visual image, but practically no contraction, or at least so little that she remained in doubt. At this stage she was not sure but that there was some contraction, which she believed she ought to call the faintest contraction that she could distinguish. She was now requested to imagine London Bridge, but at the same time to relax completely and to see if 'the visual image' remained or left. She found that under these conditions, with advancing relaxation, the visual image disappeared. The same took place with the image of Eiffel Tower. She finally observed very slight muscular tensions present upon imagining even very distant objects.[16]

The period of Feb. 15 was given to involuntary visual images. The instruction was, "Do not seek any visual images, but if any should come, report!" She observed as follows: "First an image of the director, accompanied by a slight contraction of the ocular muscles. Second, a vague visual image of dropping water with very slight contraction of the eye muscles. Third, an image of a scene out of the window, along with slight contraction. Fourth, the lake at Jackson Park, with almost no contraction. Fifth, a certain motor-car, with very slight contraction." She said that at the time the director came into the room there were no visual images, and her eyes seemed perfectly relaxed. Periods from Feb. 15 to 21 were given largely to the face, lips, tongue and speech, with success usually +1 S. On Feb. 21 the question was asked, "What takes place when you relax fully?" She replied, "I lose visual and vocal imagery." "What do you have left?" "Nothing. As a rule I go to sleep."

The period of March 7 was devoted to observation with eyes open. She was instructed to relax her eyes, and to imagine a man walking by. She reported that the eyes did not stay relaxed! They tended to move in the direction the man was walking. Instructed to imagine a motionless motor-car, she reported a very slight downward tendency of eye-movements. 'The motor-car was seen out there' (indicating). Imagining a tree in the distance brought the report, "There was no perceptible motion. The eyes seemed, however, as if held still, with a slight tenseness." The next task was to report when anything occurred spontaneously to the visual imagination. Her statement was that when her attention went to any visualized object, the eyes seemed to tend to focus there. She added that as a rule, when her eyes are open, she does not have visual images spontaneously. Requested to imagine a certain graduate student, she reported a slight tendency towards convergence along with a visual image of the individual in front of her. When she imagined a person walking upstairs she noted a slight tendency to move the eyes up. She visualized a man walking by, whistling, and reported with it a slight tendency to move her eyes towards the side in which he was going. She summarized her observations by saying that when visual images appeared, there was a slight tendency away from relaxation and towards convergence. This was more noticeable when she was instructed to have a visual image than when the image came spontaneously. She found the tenseness also more noticeable if the visual image was projected than if it was not.

In summary, this *S* learned to relax excellently. She found that imagery, visual and vocal, both voluntary and involuntary, was accompanied with sensations of tenseness. This was true of visual imagery with

[16]In distant vision, as is well known, the ocular muscles are relaxed, in the ordinary sense of the word. If this observer is correct in her report that slight tenseness was present during visual imagination of distant objects, this may perhaps be due to slight shifting of focus during that act, or to the tendency to visualize the objects out of the central line of vision.

the eyes both closed and open. Most difficult of all to note was the faint tenseness present upon visually imagining an object in the distance. Visual and other images disappeared upon progressive relaxation of the corresponding tension.

Discussion

The above records are, for the interests of the present discussion, typical of all the other *S*s or patients who learned to relax very well. Four *S*s are not considered further, since one failed to attend, another did not seem earnest in following instructions, the third was evidently in too excited a condition to make the necessary progress in the required time, while the fourth failed to attain the control of muscular relaxation and the finer skill in observation that was desired. This leaves 11 laboratory *S*s and 10 highly experienced patients who attained the requisite skill and who agreed in their observations in connection with imagery.[17]

The earliest observations of each *S*, of course, may not be counted. *It is repetition that makes the sensation of muscular contraction easy to observe.* At first the individual who contracts his biceps may scarcely be confident of the sensation he gets from this large muscle; but with experience it becomes easy. A more advanced task is to recognize the muscular sensation from a slight movement or from slight convergence of the eyes. Here again practice improves, until the experienced *S* reports the tenseness every time. When the stage of visual imagery is reached, the *S* may here also at first fail to report the experience of muscular tenseness.[18] However, with repetition of the imagery, every *S* finally reported the tenseness in a spontaneous way.

Our conclusions must be subject to certain reservations. (1) The agreement of our *O*s that the experience of muscular contraction accompanies imagery does not permit us to infer that physiological contraction takes place, unless there is additional evidence by objective methods. Further investigations from the objective point of view are under way. (2) There is a possibility of error on the part of our *O*s. For they may

[17]Seven of the laboratory *S*s seemed to have clear visual imagery and were of the visual-motor type. Four, including P. W., were less clear in their visual images, inclining more to the motor type. One of these at first seemed lacking in visual imagery, but later reported some very faint forms.

[18]It must be remembered that the director, according to the methods of the experiment, does not ask the direct question, "Did you note tenseness?" but only requires the *S* to report what happened. There seem to be four sources of omission with the inexperienced *S*. (1) Omission through lack of training. The *S* admits that he does not know how to recognize the sensation of tenseness. (2) The visual image is observed, but the attendant fainter tenseness is ignored. (3) The *S* wrongly identifies tenseness with movement, and fails to report tensions in convergence. (4) Tenseness is observed, but not reported; error of the incomplete report.

conceivably have developed a prejudice or habit of reporting tenseness. However, it does not seem likely that there was such error. (*a*) Our *O*s were mostly of a critical type of mind. (*b*) The investigator took precautions to avoid leading questions or hints, and the *O* did not know what matter was under investigation. (*c*) The *S*s who have shown the greatest skill in relaxation and in their reports on the locality of contractions do not require the same repetition as the others. Upon the first occasion, they generally report tenseness during imagination. (*d*) *S*s whose technique is still a little lacking in progressive relaxation of slight eye-movements, as externally manifested, do not report that imagery has vanished. (*e*) Every skilled *S* was requested to relax progressively, but at the same time to have visual images. Some reported failure with chagrin, and tried it over again. Every one found the combination impossible. (*f*) We attempted to rule out the error in question by using a relatively large number of *O*s, independent of each other, covering a period of years. Their observations agreed.

There seem to be additional grounds for believing that the experience of contraction accompanies imagery. The profoundly relaxed individual has a characteristic appearance, as described below; does not look or act like an imaging or thinking person; later reports that he was not mentally active; gives no evidence of the results of thought-processes; and often goes to sleep.

The relaxation of the residual tension of the small muscles of the sense-organs is a delicate matter that requires practice. All *O*s agreed that, when this was done, mental imagery dwindles or ceases in a corresponding way. The face of the individual who is so relaxed makes a lasting impression on the beholder; the eyes, if open, are vacant in appearance; they seem to be "not looking." The whole face is expressionless. One student, watching the director illustrate such relaxation, stated that it looked "as if the individual could not possibly be thinking." The thesis that progressive relaxation brings with it absence of thinking is apparent, literally, on the face of it.

This thesis also harmonizes with the experience of all the *S*s and patients who considered that it was impossible both to be relaxed extremely and to have images at the same time. With the advent of the one condition, the other invariably ceased.

We find, therefore, the experience of muscular tenseness a *sine qua non* of imagery, attention and thought-process. This [illegible] that, when a sense-organ, for instance, the eyes, are active, the muscles that control them also are active, and the sensation from the controlling muscles evidently plays a useful role in vision. In a corresponding way, it seems a matter of course that when

the original visual experience is repeated, taking the form of imagery, the muscular experience that goes with it also is repeated. In this way the individual doubtless at least in some measure controls his images and thought-processes. The cultivation of this factor has been used in clinical work for the control of undesirable mental activity or overactivity.[19]

Résumé and Conclusions

(1) A method is described to relax the striated musculature in a progressive manner.

(2) The *S* is trained to continue the process of relaxation, or *negative* of contraction, to an extreme degree.

(3) The muscles of the ocular regions and of the speech apparatus receive special, prolonged practice.

(4) The director judges the relaxation of the *S* by certain objective signs.

(5) The *S* judges the relaxation by the absence of the sense of muscular tenseness. Particular training is given with the chief muscle-groups of the entire body in order. Observation of this sense is gradually cultivated, being most difficult to observe in the small muscles of the sense-organs, especially in their slight and fleeting variations.

(6) The *S*s make their observations independently and unknown to each other, in order to avoid prejudice.

(7) For the same reason, the director carefully avoids suggesting to the *S* what he is to look for. The *S* did not know in advance that we were investigating whether imagery diminishes with advancing relaxation of muscles. The only instruction given was to relax certain muscles and to report.

(8) All the *S*s and patients who attained high skill in progressive relaxation spontaneously arrived at, and agreed in, their conclusions regarding psychological activities. With visual imagery there is a sense as from tenseness in the muscles of the ocular region. Without such faint tenseness, the image fails to appear. With complete ocular relaxation, the image disappears. This may be done by individuals of greatest skill and experience not alone lying down, but also sitting up with eyes open.

Motor or kinaesthetic imagery likewise may be relaxed away. "Inner speech," for instance, ceases with progressive relaxation of the muscles of the lips, tongue, larynx and throat.

Auditory imagery also is attended by a sense of tenseness, sometimes perhaps felt in the auditory apparatus, but character-

[19]The writer is not arguing in favor of a motor theory of consciousness, and particularly not in favor of Münsterberg's highly speculative views. This paper is an attempt to present our observations and our results.

istically in the ocular muscles. The individual tends to look toward the imaged source of sound. With the relaxation of such looking or other tension, the auditory image is absent.

Progressive relaxation is not as a rule perfect or complete save perhaps for brief periods of time. It is during such brief periods that imagery seems altogether absent. However, when the relaxation of the muscles of the sense-organs seems to approach completeness, there takes place the diminution of image-processes. It appears that natural sleep ensues after the imageless state is maintained for a relatively prolonged time.

With progressive muscular relaxation, not alone imagery, but also attention, recollection, thought-processes, and emotion gradually diminish.

Relaxation apparently is a natural physiological function which every individual possesses and uses, for muscles that contract must also relax. It is shown in the present work that this function may be cultivated to the point of a fine control. In our experience it may be cultivated to some extent at least with every normal individual, just as every normal individual may learn to walk; but persons vary greatly in the time required for learning. In general, the person who is highly suggestible may be slower to learn than one who is independent and observing.[18]

[18]A future paper with a different approach will present further evidence. I am indebted to Professors H. A. Carr and A. J. Carlson for critical reading of the text.

A COMPARISON OF SOPHISTICATED AND NAIVE SUBJECTS BY THE ASSOCIATION-REACTION METHOD[1]

By Dorritt Stumberg

I. Statement of Problem and Method

The purpose of the work here reported is to compare the result of the association method of detecting suppressed ideas, when used with subjects who are thoroughly familiar with the method both in theory and in practice, with the result of the same method when applied to persons who are unfamiliar with the method. In most of the work reported in the literature the subjects are either entirely naive so far as the knowledge of the method is concerned, or they have heard discussions in the classroom and have perhaps acted as subjects once or twice before the principal experiment. In the present study the subjects served again and again and thus had opportunity to improve their technique of preventing detection.

The *S*s were three students of Harvard University and two students of Radcliffe College, all of whom were familiar with the method before the present experiment started. These *S*s are designated as the sophisticated group. To make sure they had the method fresh in mind they were asked to read pp. 112-116 in "An Elementary Laboratory Course in Psychology" by Langfeld and Allport, in which there is a brief description of the method and a list of factors upon which may be based judgments of guilt or innocence. Furthermore, the *E* pointed out to them certain methods by which a guilty person might appear innocent: (1) deliberately lengthening the reaction-time to non-crucial words (in order to equalize the averages of the reaction-times to crucial and non-crucial words, and to prevent the longest reaction from occurring with a crucial word); (2) anticipating test-words and thinking up noncommittal replies, so that when the test is given there may be no lengthening of the time due to a consciousness of guilt; and (3) feigning surprise at unusual words with which they must be familiar if they were "guilty."

There was a preliminary series in which the *S* was given two stories, one of which he was instructed to read. Immediately after he had read the story, he was given a series of 25 stimulus-words of which half came from one story and half from the other story. The words from the story he had read

[1] From the Harvard Psychological Laboratory. Submitted by H. S. Langfeld.

were "crucial" words to him, and the words from the other story served as non-crucial words. Both the responses and the reaction-times, taken with a stop-watch, were recorded.

After the preliminary series, in which three trials were made, the *S* was again given two stories, but the list used in making this test contained 50 words, 25 pertaining to one story and the other 25 to the other story. The *S* was instructed to make the *E* think he had read the story which actually he had not read. After this method had been tried twice (tests A and B), with an interval of a week between trials, the *S* was given one story and told to read it or not to read it as he chose, but in any case to make the *E* think that he had not read it. This test was tried three times with each *S* (tests C, D, and E) at intervals of about a week, making a total of 5 tests for each *S* after the preliminary trials.

Test C was repeated with four entirely naive *S*s. The method was the same as the one used with the sophisticated *S*s, except that no suggestions were given as to the nature of the test or the means of avoiding detection. Test D was repeated with one naive *S* and with one *S* who had studied the test several years before, but who did not realize the method was to be used until it was well under way. This *S* did not, therefore, have an opportunity to prepare a defense, though she did presumably have an advantage over the entirely naive *S*s. Test E was repeated with one naive *S*.

In making the list of words the *E* tried to avoid unusual words and others, such as foreign words, which she thought would result in a long reaction on the part of either a guilty or an innocent *S*.

II. Explanation of the Table

The results are presented in the Table. The first vertical column gives numbers for convenience in reference to the Table. The numbers 1 to 6 refer to different criteria used in making a judgment as to guilt or innocence. The small letters indicate that there was an attempt to increase the accuracy of judgment by setting an arbitrary standard for each criterion.

The second column gives the different criteria used as a basis for judgment of guilt or innocence, and in some cases the arbitrary limitation of these criteria.

The third column gives the results for sophisticated *S*s. The numbers show in how many cases the criterion mentioned [illegible] the criterion was a correct indication of guilt or innocence. The negative sign shows that the criterion was an incorrect indication of guilt or innocence.

The fourth column gives the results for the naive *S*s

TABLE

		Number of correct and incorrect judgments		Percentage of correct judgments			Diagnostic certainty		
		Sophisticated *Ss* 23 Experiments	Naive *Ss* 7 Experiments	Sophisticated *Ss*	Naive *Ss*	Ratio of Naive to Sophisticated	Sophisticated *Ss*	Naive *Ss*	Ratio of Naive to Sophisticated
1	Difference between average reaction-times	+14 −9	+6 −1	61	86	1.41	.22	.72	3.3
1*a*	Crucial difference .2 sec. or larger for a judgment of guilty	+ 7 −7	+5 −1	50	83	1.66	0	.66	∞
2	Difference between A.D.	+ 9 −14	+6 −1	39	86	2.20	.22*	.72	(3.3)
3	Longest reaction-time	+13 −8	+4 −3	62	57	.92	.24	.14	.58
3*a*	Two or more reaction-times in one set longer than the longest in the other set	+ 8 −5	0 0	62			.24		
3*b*	Difference between the longest reaction-time of one set and the longest of the other set is 1 sec. or more	+ 6 −6	+1 −2	50			0		
3*c*	Difference between the longest reaction-time of one set and the longest of the other set is 4 sec. or more	+ 5 −1	0 0	83			.66		
4	Associations apparently based upon the story	+ 7 −3	+1 −2	70			.40		
4*a*	Two or more associations based upon the story	+ 3 −2	+1 0	60			.20		
5	Misunderstanding of word	+10 −11	+2 −3	48	40	.83	.04*	.20*	5
5*a*	Misunderstanding of two or more words	+ 5 −6	+2 −3	45	40	.89	.10*	.20*	2
6	Apparent refusal to give association	+ 2 −3	0 −1	40			.20*		

* By reversal of criterion

The fifth column shows the percent. of correct judgments for the sophisticated *S*s. The sixth column shows the percent. of correct judgments for the naive *S*s. These values are based upon the number of judgments which can be made and not upon the total number of experiments. There are some instances in which the value of the criterion which was used was the same for both series. Therefore no judgment could be made. These instances were omitted from the number of cases upon which the percentage was calculated. If they had been included, the value would be smaller in every case.

The seventh column gives the ratio of the correct judgments for the naive *S*s to the correct judgments for the sophisticated *S*s.

In the last section of the Table (columns 8, 9 and 10), the diagnostic certainty is presented. 100% correct judgments may be considered certainty. 50% is mere chance. Then 0% is also certainty if one reverses the criterion. The results were therefore treated so that 50% of correct judgments, in column 5, is represented as 0 in column 8; and 100% or 0% in column 5 is represented as 100% in column 8. The cases where the criteria have been reversed are marked with an asterisk. The last column gives the ratio of the diagnostic certainty for naive *S*s to the diagnostic certainty for sophisticated *S*s.

Omissions in the Table occur in those instances where there are too few cases for the results to be of value.

III. Discussion of Results

1. *Crucial Differences Between Average Reaction-Times*

If we consider the longer average reaction-time to crucial words an indication of guilt, as has frequently been done in the association test, we shall judge correctly 14 times and incorrectly 9 times, on the basis of this criterion alone. The percent. of judgments which are correct is 61, which gives a diagnostic certainty of .22. Turning to the naive *S*s we find that we can safely base our judgment on the crucial difference 6 out of 7 times, or in 86% of the cases. This gives a diagnostic certainty of .72[2]. This record was never exceeded throughout

[2] An attempt was made to increase the accuracy of judgments by raising the standards. Two-tenths of a sec. was arbitrarily chosen as the necessary difference between average reaction-times before a judgment was made in tests A and B. In tests C, D, and E, if the average reaction-time to crucial words was .2 sec. (or more) longer than the average reaction-time to non crucial words, the *S* was considered guilty; if the average re- [illegible] reaction-time to crucial words, or if the difference between the averages of the reaction-times of the two sets was less than .2 sec, the *S* was considered [illegible] 7 incorrect judgments. The greatest difference which occurs is +.54, [illegible]

the entire experiment with any criterion. It is equalled only when we use the A.D. as a criterion to judge naive *S*s. This conclusion is in accord with the results of previous experimenters, who report that the reaction-time is a more reliable indication of guilt or innocence than a qualitative analysis of the reaction-words.[3]

2. *Average Deviation*

If we assume that, when the A.D. is greater for crucial words than for non-crucial words, the *S* is guilty, we shall make 9 correct judgments and 14 incorrect ones. Thirty-nine percent. of the judgments are correct. We may reverse the criterion, saying that the shorter A.D. is an indication of guilt, and thus increase our percentage of correct judgments to 61, which gives a diagnostic value of .22. But in practical application one cannot tell when the criterion should be reversed. When we study the results of the naive *S*s we see that the criterion should not be reversed, for the longer A.D. is a correct indication 6 times and an incorrect indication only once, making the percent. of correct judgments 86 and the diagnostic value .72.

In only one of all the cases is the A.D. a correct indication when the crucial difference between the average reaction-times is not. Here the difference in the A.D. is +.04, and the crucial difference between the average reaction-times is −.13. With a value as small as +.04 a judgment could not be made with any assurance. The A.D. is an incorrect indication 6 times when the crucial difference is correct. These cases occur only for the sophisticated *S*s. Among the naive *S*s the A.D. and crucial differences between reaction-times gives similar results.

3. *Longest Reaction-Times to Individual Words*

The current interpretation of long reactions is that they indicate guilt when occurring with crucial words. From the Table we see that in 13 out of the 23 experiments with sophis-

of judgments by considering the size of the difference between the average reaction-times.

In considering the longest reaction-time, an attempt to increase the accuracy of judgment is made (*a*) by considering only those cases where one set of words has at least two reactions longer than any reaction-time in the other set; (*b*) by considering only those cases where the difference between the longest reaction-time of one set and the longest reaction-time of the other set is at least 1 sec.; (*c*) by considering only those cases where the difference between the longest reaction-time of one set and the longest reaction-time of the other set is at least 4 sec. See Table, 3*a*, 3*b*, and 3*c*. The results of attempting to increase the accuracy when using other criteria are shown in 4*a* and 5*a* of the Table. These results have a negative value similar to the results discussed in the foregoing paragraph.

[3] H. S. Langfeld, Psychophysical Symptoms of Deception, *Jour. Abn. Psych.*, 15, 325. H. M. Leach and M. F. Washburn, Some Tests by the Association Method of Mental Diagnosis, this JOURNAL, 21, 1910, 162.

ticated *S*s the longest reaction-time is a correct indication. Eight times we are wrong when we judge on this basis, and twice we cannot judge because the longest reaction-times in the two sets are equal. The percentage of correct judgments is 62 and the diagnostic certainty is .24. With the naive *S*s the longest single reaction is not as good a criterion as the A.D. or crucial difference. It is a correct indication 4 times and an incorrect indication 3 times. Fifty-seven percent. of the judgments are correct and the diagnostic certainty is .14.

4. *Qualitative Indications*

Seven of the sophisticated *S*s were correctly judged guilty by the fact that the response words were closely related to the story. Three times associations which apparently came from the story misled us, and an incorrect judgment was made. Seventy percent. of the judgments were correct, which gives a diagnostic certainty of .40. In each of the three cases where the judgment was incorrect there were more than one misleading association; in one case there were as many as eight. In this last case the *S* wished the *E* to think she had read the story which she had not. She guessed at the setting by the words given, and gave associations seemingly from the story (sometimes giving as a response a word which had previously been used as a test-word). With some care it would be possible to make a list of test-words where this device could not be used. When the *S* wishes to appear innocent the danger of this method being used is not great.

In the 7 cases where the *S* was correctly judged guilty, there were two instances where 5 associations were based upon the story, and one instance where there were 4 associations of this nature. In contrast to these results, 4 of the naive *S*s gave no crucial associations and two gave only one. In the other test (where the *S* was not guilty) there was one response which the *E* felt was of possible significance, but which must have been due to chance. The explanation may be that the naive *S*s guessed at once that the nature of the association was important, but did not realize that reaction-time would count heavily. Therefore they slowed up on test-words (as the Table shows) and succeeded in repressing a tell-tale word. The sophisticated *S*s on the contrary realized the importance of the reaction-time and strove to keep it low, a procedure which sometimes resulted in an incriminating association

[illegible]

conditions and made correct judgments 52 out of 53 times, considered that of the qualitative indications the misunderstanding or misinterpretation of a test-word was very [illegible]

[illegible]

portant in indicating innocence. For example, when the crime consisted in the *S* looking into a box containing a bottle of red ink, the test-word "write" was given. The subject responded with the word "wrong." The authors argue that the *S* was innocent because if she had seen the ink, she would certainly have recognized the word as w-r-i-t-e.

Without intention on our part, the test-words gave many opportunities for a double interpretation on the part of the *S*. If we base our judgment upon this criterion we obtain 9 correct judgments and 10 incorrect ones with the sophisticated *S*s and 2 correct and 3 incorrect ones with the naive *S*s. The percentage of correct judgments for the sophisticated *S*s is 48 and the diagnostic value (if the criterion is reversed) is .04. For the naive *S*s the percentage of correct judgments is 40. Reversing the criterion gives us a diagnostic value of .20.

Apparent refusal to give associations at all, that is to say, failure to follow directions, occurred 5 times among the sophisticated *S*s, but led to a correct judgment only twice. The percentage of correct judgments is 40 and the diagnostic value is .10, provided the criterion is reversed. In the naive group there was only one such instance and the *S* in question was not guilty.

5. *Combined Criteria*

Since each criterion alone led to many incorrect judgments among the sophisticated *S*s and to one or more incorrect judgments even among the naive *S*s, it seemed advisable to test the reliability of a combination of criteria. The *E* therefore considered all criteria in every case and refused to make a decision when the evidence was contradictory.

Among the sophisticated *S*s there was one case where all the criteria were present and were of positive value. In addition there were 6 cases where the criteria which were present were positive or of very doubtful negative value. We may say, then, that we have 7 fairly clear-cut cases for correct judgment by the use of a combination of criteria. Four of these occur in test A, that is to say, early in the series. There were 2 cases where every indication which was present was incorrect, and which therefore led to 2 incorrect decisions. Among the naive *S*s there were 4 cases where every indication except one (of very doubtful negative value) was positive. There is one case where every indication was negative.

IV. Methods Used as Reported by *S*s

The following are methods which the *S*s reported.

(1) Lengthening the reaction-time to certain non-crucial words. (2) Lengthening all reactions so much that there would be plenty of time to think of reactions to crucial words

and yet not cause a large crucial difference. (3) Associating of non-crucial words with others in the same set (done in the case where two stories were given to the *S*s). (4) Thinking, before the test, of associations to the test-words which would probably be used. (5) Noting test-words which might be misunderstood by an innocent *S* and preparing associations (Chess—Nut; Pale—Bucket). (6) Sparring for time by asking, "What was that?" (7) Using a definite mental set, such as giving definitions, no matter what word appeared.

V. Conclusion

The above results show that under the conditions of this experiment (in which no emotional feature was introduced) sophisticated *S*s can prevent detection much of the time. If all criteria are considered, we find that out of 23 tests with 5 sophisticated *S*s we can make 7 correct and 2 incorrect judgments, leaving 12 on which we can make no decision. If we take our most reliable single criterion (longer average reaction-time) we can make 14 correct and 9 incorrect judgments. If we take the A.D. as a criterion we can make 9 correct and 14 incorrect judgments, exactly reversing the results of our most reliable criterion. It seems clear that the method is unreliable when the *S*s are sophisticated.

The naive *S*s, on the other hand, were not so successful in preventing detection. The average reaction-time and the A.D. were correct indications 6 out of 7 times. Combined criteria led to a correct judgment 4 out of 7 times. But there is one case (in which the *S* was not guilty) where a wrong decision was based upon a number of apparent indications of guilt.

This Study suggests several problems for further investigation. It would be interesting to see how much the introduction of an emotional feature, such as is present in the case of a real crime, would affect results. Could the sophisticated *S*s maintain their record? If they were detected the first time, could they learn to cope successfully with a series of new situations?

The question also arises as to the success of sophisticated *S*s compared with naive *S*s when the *E* did not know to which of these classes each *S* belonged. Could sophisticated *S*s not only prevent detection of their crime, but also prevent detection of their sophistication?

AN EXPERIMENTAL STUDY OF FEAR

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In the vast literature dealing with emotion, one searches in vain for any attempt to make an experimental analysis and description of an emotion from a psychological point of view. One reason for this lack is the frequent assertion that an emotion can not be brought under experimental laboratory control.[1] The physiological changes characteristic of emotion have received their full share of attention.[2] Work in this field has, however, left the psychological aspects of emotion just where they were. One can not go from quantitative changes in the secretion of adrenalin, for instance, to the qualitatively different emotions.

The present investigation was undertaken to determine experimentally to what extent an emotion can be made the direct object of observation and description, and to work out so far as possible the description of a particular emotion. We chose the emotion 'fear' because the concept of it seemed to us to be the most restricted and clean-cut of the emotional concepts. In the absence of any physiological symptom which unequivocally indicates the presence of fear, we had recourse simply to *O*'s report that he felt fear. If, by emotion, we mean a mental experience, this is the ultimate proof of its existence.

Procedure I.

The experiment was carried on in a dimly lighted room with dark grey walls. *O* sat in an easy chair facing a dark corner. *E* always remained behind him. *O* read the following instructions at the beginning of every observation period: "I am going to present to you a stimulus. Let it have its full normal effect and respond to it naturally. After I say 'Now' describe your experience as completely as you can. After the 'Ready' signal, close your eyes, and keep them closed until after I have said 'Now'. You may then open them if you wish." We found it necessary in the course of training to caution the *O*s further: "Don't try to tell what the object is, but describe *your* experience in the situation. Be 'subjective' and personal rather than objective. Deal with yourself and do not try to understand what is going on." *O* was always asked to sit with his arms on the arms of the chair and his hands spread out. About 10 stimuli

[1] In his *Textbook of Psychology*, 472 ff., Titchener points out this lack of psychological investigation in the field of emotion and maintains that emotions are susceptible of laboratory study and analysis.

[2] W. B. Cannon, *Bodily Changes in Pain, Hunger, Fear, and Rage*, 1920.

were presented at every period, and an interval of 3 to 4 min. was allowed to elapse between presentations. When the stimulus was visual, the light was thrown on the object, and *O* was told to open his eyes. The *O*s were three undergraduate girl students and observed over a period of 18 weeks. Every *O* gave a total of 350 reports.

Seventeen different stimulus objects were employed, including things that would give rise to only moderately affective experiences and to pleasant emotions as well as to fears. The presentation of a large number of situations that would arouse other than fear experiences was designed, first, to prevent the *O*s from becoming too sophisticated with regard to the experiment, and secondly to avoid making it predominantly disagreeable. The several stimuli in the order in which they were introduced into the experiment were: odors (pl. and unpl.), sandpaper, warm velvet, loud noise (bang), feather, tuning fork touched to the face while in motion, file on tin, whistle, blast of air, rubber glove filled with water, preserved brain, preserved frog, soaked macaroni, wet rubber snake, cold metal, pictures of snakes.

Results

Emotional Characterizations. Only those reports in which *O* included an emotional characterization indicate situations which are adequate for the arousal of fear. Typical expressions of this sort are: "I was frightened", "It felt horrible", "It was simply awful". The numbers of such reports of emotion were for the 3 *O*s respectively 50, 29, 22.

Analytical Descriptions. In addition to giving the emotional characterizations, the *O*s also analysed the experiences and reported their sensory, imaginal, and affective components. The affective element showed the greatest constancy, in as much as every experience that was characterized as 'fear' also was described as unpleasant..

The sensory components of the emotional experiences split into two types; first, those that were localized externally and were referred to the stimulus object; secondly, those that were localized internally and perceived as 'subjective', *i.e.*, organic. The factors that remained constantly present in all the descriptions that we obtained were the sensations directly set up by the stimulus, and the affection of unpleasantness. Neither organic sensation nor imaginal supplementation was invariably present.

O	% emot. char.	% local. in stim.	% unpl.	% organ.	organ. image	% image	total % [illegible]	total % [illegible]
S	50	100	100	54	30	16	84	46
M	22	100	100	50	23	23	[illegible]	[illegible]
D	29	100	100	55	20	25	75	45

The Table of the reports shows that organic sensations in some cases formed the greater part of the vivid and reportable experience; that frequently, however, these sensations were further supplemented by images; and that many times the images themselves made up the whole of the focal experience, and any organic sensations that may conceivably have been present were marginal, and hence neither reportable nor a part of the emotion as experienced.

Samples of Reports: Organic Sensations Focal

S. "That frightened me, and made me jump, it felt so horrible, so wet and cold and clammy and unpleasant. I felt my hand draw away from it and something inside me 'caught', *i.e.*, there was a pulling and swelling pressure in the region of my stomach, when it first touched me. I didn't even stop to think what it was."—Stimulus, rubber glove.

M. "Oh, that was awful! It made me feel 'all mixed up' inside. It was so wet and cold and horribly unpleasant. I felt myself squirm."—Stimulus, rubber snake.

P. "That was unpleasant. It produced a sinking, sick feeling in my stomach. It was horrible, and made me want to get away from it."—Stimulus, human brain.

Organic Sensations and Images Focal

S. "That is a violent reaction. It feels awful. It makes me shiver and shake inside and my stomach feels 'all mixed up.' It is so ugly and awful. It is the most unpleasant feeling I have ever had. I can't control myself. I want to wipe it off my hand. I think (visual image) it is a bunch of worms and I have always had a horror of worms."—Stimulus, soaked macaroni.

M. "That scared me and made me feel shaky. It was an unpleasant sound. It made me think (in visual images) of an explosion."—Stimulus, loud noise.

P. "That was horrible,—very unpleasant. It was a wet slimy feeling. It made me feel creepy all over and sort of sinking inside. It felt like a lot of wet angle-worms (visual and verbal images)."—Stimulus, soaked macaroni.

Images Focal

S. "That felt cold at first; then I thought that it was the glove (visual and verbal images) and became frightened and wanted to draw my hand away. It was unpleasant."—Stimulus, cool metal.

M. "That always scares me, and is unpleasant. I think of a huge hammer (visual image) striking near me and I don't want to be struck."—Stimulus, loud noise.

P. "A cold wet pressure at first; then I got to thinking that it was that frog (visual and verbal images) and it became very unpleasant and repulsive, and I wished that you would take it away (kinaesthetic images)."—Stimulus, cold metal.

Procedure II.

The rôle of images led us to attempt to control and to vary the imaginal setting of the stimuli, and to record the accompanying changes, if any, in the emotional aspect of the experiences. This aim was accomplished in the following manner. A certain stimulus, the rubber glove, had constantly and invariably evoked a violent fear on the part of one *O*. The mode of presentation was varied in two ways. Instead of having the

stimulus placed unexpectedly on his hand, *O* was told to open his eyes and grasp the glove. The experience under this presentation was unpleasant, but never gave rise to an emotion, though organic sensations were often reported. In as much as *O* had seen the stimulus and now could visualize it or represent it to himself in other imagery, he was told, "This is the rubber glove", and was then stimulated. The result was sometimes an emotion and sometimes not. The organic sensations were frequently present; but whether an emotion was experienced or not was determined by the absence or presence of images of the rubber glove. A vivid representation of the stimulus as the rubber glove *O* had seen resulted in no emotion even in the presence of strong organic sensations.

Samples of Reports

S. "That felt cold and clammy and nasty, and my stomach felt contracted; but I was not frightened as I used to be."—Stimulus, seen and touched.

"That was rather unpleasant and made my stomach start to feel queer; but when I realized that it was the rubber glove (visual and verbal images), it stopped, and that was all. I was not frightened."—Stimulus with knowledge.

"That frightened me (*O* screamed). I feel completely out of breath and my stomach caught inside. I couldn't help but yell. Shivers went over me. I didn't think it was the glove. As soon as I did realize that it was the glove (visual and verbal images) I was no longer frightened, though I still feel queer inside and am out of breath."—Stimulus without knowledge.

We next attempted to produce an emotional experience by the addition of an imaginal setting. In what to *E* was a preceding situation, we presented a stimulus that was likely to carry over in imagery to the succeeding stimulation. For instance, a picture of snakes was shown, and then after a shorter interval than usual cold soaked macaroni or a thin rubber tube filled with wet sand was placed on *O*'s hand. The result was a strong emotion which included always images of the picture and sometimes organic sensations. Another arrangement of stimuli that produced similar results consisted in placing on *O*'s hand a piece of cold metal and at the same time bringing near his nose some of the preserving fluid in which the brain and frog were kept. Whenever the odor called up the image of one of these stimuli, *O* experienced an emotion.

Samples of Reports

P. "A cold, wet, clammy feeling. It was horrible, repulsive and slimy, and felt like the snakes or worms I had just seen in the picture."—Stimulus, soaked macaroni.

"Oh, that was awful! It was wet and slimy. It made me feel creepy all over. I saw that picture of crawling worms. It was horrible and unpleasant."—Stimulus, cold soaked macaroni.

"I thought you were putting that frog on me (visual images) and I had the queer feeling in my stomach and was frightened."—Stimulus, [illegible]

"A cold wet clammy pressure and the smell of that frog (visual and verbal images) which made the experience horrible and repulsive. It gave me the creeps all over, for I thought you had put the frog on my hand (visual and verbal images)."—Stimulus, preserving fluid and cold metal.

It appears from the foregoing examples that the presence of fear depends not upon any particular group of processes such as organic sensations, but rather upon some perceptual meaning of the object for *O*, a meaning which is carried by the particular sensations, images, or both, that are called out by the stimulus. A re-examination of the reports brings out the general nature of this meaning. The emotional characterizations of the experiences were accompanied in most cases by a further, explanatory statement of the nature of the experience. This explanatory statement always indicated that the experience signified a negative motor response. "I wanted to withdraw from it and wipe it off my hand;" "It is something I don't want to touch;" "I didn't want to look at that;" "I wanted to get away from it." Sometimes the response was actually carried out; at others it was merely 'felt', *i.e.*, imaged kinaesthetically, while often it was represented in other than kinaesthetic terms. But the meaning was always that of movement away from the stimulus.

Conclusions

Our investigation has demonstrated, first, that emotions may be set up under laboratory conditions and be subjected to psychological description. Also, it has enabled us to work out the gross structure of the emotive pattern in the case of fear. This pattern is akin to the pattern of perception,—core and context. It is related, likewise, to the pattern of the action consciousness. The core of an emotion consists, as in the case of perception, of those sensory elements that are referred to the stimulus. In addition, there is a context which consists of images or sensations or both. An as integral part of both core and context, there is always an affective element,—in the case of fear, unpleasantness. In the cases of intense emotions and of certain individuals, the sensations of the context, which are principally organic, probably play the dominant rôle; but their presence does not guarantee that the experience shall be an emotion, nor does their absence preclude emotion. A definition of emotion that includes only those emotions in the contexts of which organic sensations predominate makes an unwarranted restriction of the term and excludes experiences that are felt as emotions.

The emotive pattern resembles the fore-period of the action consciousness in as much as it carries the meanings both of a movement on the part of the *O* and of the result of that movement. The fact that the truly emotional aspect of an experience is the meaning of a particular type of response to the situation

throws light on the wide differences one finds among the various classifications of the emotions. Such classifications, obviously, are not based on the mental components or the patterns of the emotions, but rather upon the action-meanings which the particular patterns of the particular mental elements carry for the experiencing individual. They are, therefore, not psychological but logical, and are determined by the classifiers' tendencies to explicate or to generalize and by their evaluation of the responses.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF McGILL UNIVERSITY

Communicated by WILLIAM D. TAIT

I. AN EXPERIMENTAL INVESTIGATION OF THE INFLUENCE OF CERTAIN WEATHER CONDITIONS UPON SHORT PERIODS OF MENTAL WORK

By J. P. BETHEL

Introduction

The direct and qualitative relationships existing between weather conditions and mental processes furnish a field of experiment and research which has attracted surprisingly little notice in psychological circles. While it is true that a great many articles and reports make reference to the subject of weather conditions and their effects on mental work, such references are merely suggestive, theoretical and entirely incidental to the reports in which they occur. Many and varied experiments have been carried on, and valuable data have been accumulated in consequence, concerning the existence and nature of certain definite curves of periodicity—diurnal and seasonal—of mental work; yet Robinson affirms that, from the facts now available, it is impossible to arrive at any statement of the diurnal course of efficiency which has a very wide applicability. In spite, furthermore, of these numerous investigations, the experimenter in the field of weather conditions alone will find that there are very few previous data (excepting, of course, mere theorizing) which he can verify or contradict, with the exception, perhaps, of findings in the sphere of seasonal periodicity. There are, however, several important facts which can be gleaned from the previous records—hints as to methods employable, and pitfalls to be guarded against.

Historical Survey

Before proceeding with a report of my own experiment, it will be advisable to give some brief summary of the work of previous investigators, which affects, even indirectly, the special problem before us.

(1) As early as 1859 the physiological value of the weather began to be observed by E. Smith, who showed in his "Analysis of Respiration" that breathing is at a maximum in spring and at a minimum towards the end of summer and beginning of autumn. Finsen next discovered a seasonal variation in the amount of hæmoglobin in the blood, and Lehmann showed experimentally that there is a corresponding periodicity in the strength of the heart-beat. Malling-Hansen discovered that the height and weight of growing boys seemed to be subject to periodic variations which recurred year after year in the same manner. Besides such general physiological periodicity, more specialized topics began to attract attention, and the results of various investigators are summarized by Peaks in his work "Periodic Variations in Efficiency[1]."

(2) Schuyten: Periodicity of Attention.—School children were tested once a month, the test being repeated 4 times each test-day. Each child sat with his Flemish Reader open before him: at a given, unchanged signal they all began reading silently. The *E* observed those who were not attentive, those who stopped reading, those who raised their eyes, and those who were continuously active during 5 min. The investigation was begun

[1] For the purpose of our present study we may neglect the various experiments (of Schuyten, Lehmann, Pedersen, Marsh and Peaks, etc.) dealing with periodicity of muscle strength or physical abilities alone.

in March, 1893 and continued for 10 months (August and September being holiday months). From his general results for the year Schuyten notes the greatest difference between March and July, and upon these results bases the conclusion that the power of attention in children is inversely proportional to the atmospheric temperature.

In order to satisfy himself that the results were valid, Schuyten made a second series of experiments, using the same general method of study as before, with one exception—he began in April instead of March. From these results he concludes that (*a*) the second test verifies the results of the first; (*b*) rest has a good influence on attention; (*c*) intermissions are more beneficial in summer, more beneficial to lower classes, more beneficial to boys.

The faults of this experiment are: (*a*) the neglect of quantitative measurements; (*b*) the neglect of the obvious fact that the assigned readings would not interest all the children equally; and (*c*) the unwarranted conclusions drawn from the same test 4 times a day every month. Furthermore, Peaks does not agree with Schuyten that the results of the second experiment verify those of the first.

(3) Lobsien sought to supplement and verify the work of Schuyten in a similar investigation. In gathering the data, experiments were made on the boys and girls in the public schools of Kiel, from 8 to 14 years of age, on the 15th day of each month from September 1901 to June 1902 (July and August being holiday months). Lobsien's work is limited to the study of primary memory for lists of 10 words involving both visual and auditory imagery, *e.g.*

List A

Visual	*Acoustic*
Sonnenlicht	Gesang
Fenstercheibe	Gebell
Wandteller	Summen
Handspiegel	Sausen
Himmelblau	Rasseln

The word-lists were spoken plainly, after which each pupil was asked to write the words in a list at once. Lobsien tabulated the results separately for girls and boys, and for each age. The curves for boys and girls are much alike: in general they show a high period or crest around December and January and a trough or depression in February and April. The visual tests show a curve which is higher in the first than in the last part, a fact which Lobsien considers the more remarkable because pupils should improve with practice.

Peaks thus criticizes Lobsien's work: "While very carefully worked over," it "seems to be somewhat questionable both in the materials and in the procedure. While the method was new, the word materials are so much alike in form that the memory-images undoubtedly interfered with one another . . . Then again, there are so many accidental factors which enter into mental abilities, due to time of day, temperature, individual daily results, etc., that it is doubtful in the extreme that the results from tests made but once a month are a true index of any but accidental factors, and that the seasonal rhythms can be discovered from tests taken [illegible] were tested cannot overcome this source of error in the procedure.

(4) In 1906, Marsh began a series of experiments at Columbia University dealing with the daily variations in mental, physical, and psycho-physical efficiency. Mental tests were made in association, [illegible] addition, all of which showed that 12 m. to 2 p.m. was the most favourable, and 9 to [illegible] p.m. the least favourable time of the day [illegible]

(5) Gates made two elaborate series of investigations on the diurnal course of efficiency; a few of his conclusions are of interest in the present investigation. There is, he says, a positive correlation shown between visual and auditory memory, on the average. The correlation of these two functions in one case runs up to +.93 and is only slightly lower in several others. The general average is considerably reduced by a single negative correlation which appears in one small group of boys. All the functions tested show an improvement, due to practice, in amounts varying from 7% to 33.3%.

(6) Gates' curves for diurnal efficiency, which show (*a*) a steady diurnal increase for the speed, accuracy and cancellation tests, and (*b*) a steady rise till 12 m., a drop from 12 m. to 1 p.m., and a slight rise till 3 p.m. for the addition, multiplication, memory, recognition and completion tests, are not borne out by the cancellation-test experiment conducted by Muscio on a group of factory workers. Muscio finds that efficiency for the tests gets gradually lower during the day, notwithstanding the fact that the only work done on the test-days was the actual work of the tests—not more than 7 min. per half-hour. The test-results from 4.15 p.m. onwards are definitely poorer than any other test-results during the day except the initial one.

(7) With regard to the correlation between initial ability and improvement, Thorndike makes the assertion that it is by no means perfect, fairly large differences in the latter being found between *S*s with equal initial scores.

(8) Peaks has conducted an elaborate experiment which will be described here in some detail. His investigations, which concerned periodicity as shown in both mental and physical[2] tests, were conducted during the school year October 1910 to June 1911. The tests were made on two groups of students in the First Year of the Manual Training High School of Washington University, St. Louis, Mo. The tests were all made under Peaks' personal direction at hours ranging from 9 a.m. to 3 p.m., at a time when classes came to his room for a study period. Many students took the tests for a part of the time, but only 10 (the B Division) took the tests daily throughout the entire period. Another group of 22 (A Division) took the test every Friday. Owing to the amount of time consumed by the tests it was not possible to give more than one physical and one mental test daily.

Periodicity as shown in Primary Memory.—(*a*) The purpose of the tests was to discover any periodic variations in mental ability during the school year, and to see how these variations compared with those found by other investigators of mental ability and with his own physical tests. (*b*) Method and procedure were as follows. A series of 4 numbers, as 84, 26, 47, 32, was read at the rate of 1 per sec. At the close of the series the class was allowed 15 sec. to reproduce the series in order of presentation on a record blank provided for this purpose. The blank bore particulars as to the character of the day and the pupils' score in the physical test.[3] After the 15 sec. of reproduction a second series of 5 numbers was read: then a final series of 6 numbers. The pupils were also asked to introspect and record any factors which helped or hindered their reproduction of the series. Marks were awarded as follows: 1 for every correct whole number in right order; ½ for every correct whole number in wrong order; ½ for a number with one digit correct. (*c*) A series of preliminary tests was given on 3 days, and the pupils were told the purpose of the test. (*d*) Results:

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[2]The physical test is here omitted.

[3] Since the pupils knew the purpose of the tests, Peaks may not have been altogether wise in allowing them to see their scores for the physical test just before doing the mental test.

in this investigation the averages for each day, each week and each month were computed for the individuals, for all ages and for each group as a whole. The monthly variations for primary memory for both the A and B Divisions are shown by the following Table.

Div.	No.	Oct.	Nov.	Dec.	Jan.	Feb.	Mar.	Apr.	May	June	Avge.	High	Low	Grtst. Var.
A	22	10.2	10.8	10.6	10.7	10.5	10.7	10.7	11.1	10.7	10.7	11.1	10.2	0.9
B	10	9.1	10.0	10.4	10.6	10.2	10.2	10.2	11.2	11.0	10.4	11.2	9.1	2.1

The Table for the B Division shows a characteristic curve similar to the curve for muscular growth; that for the A Division does not show the same characteristics. The only striking similarity is found in the depression for February, a depression which is found in the curves for both mental and physical tests. "If," says Peaks, "we eliminate these special cases" (*i.e.*, older or brighter boys whose scores were considerably above the average) "and the practice-effects, we find that the curves for the B Division Memory Tests become more nearly level, the one outstanding feature being a slight drop some time in January and February. We must conclude, therefore, that the curve for primary memory shows a rising tendency with favourable influences from October to January, a period of depression for January and February and the first part of March, and a rising tendency with favourable influences from March to June."

In the mental tests Peaks affirms that the effects of practice and growth are very small. In no case in either group was a continuous monthly gain made throughout the year. With the exception of 5 individuals, every boy had made his highest record of the entire set of tests by the first of January. The following paragraph is of particular interest: "Among the 10 boys in the B Division who took the tests daily, 5 could usually be depended upon to make good scores. The other 5 were very nervous and easily became confused, making often very low records. I have taken the individual records of each boy, and by comparing them with the records of temperature, humidity, etc., sought to discover causes. In most cases the results of such comparisons are contradictory or baffling." Individual results vary so much from day to day, owing to accidental causes, that it is often difficult to see depressions in the weekly averages; but the individual monthly averages bring out the depression referred to above, in almost every instance.

(9) Of even more interest to us, however, than this carefully planned and executed work of Peaks' are his remarks on the influence of weather-conditions.

The first American to investigate the field of weather influences to any extent was Dexter. He collected statistics of truancy, delinquency, etc. In each case the relationship to weather conditions is purely statistical, based on the probability or expectancy of occurrence. Temperature seems to be most influential of all the weather-factors. Dexter finds that normal work goes on when temperature is between 69° and 73°F. Hines finds that 68°F. gives the best results. Lehmann and Pedersen find that rapidity of addition varies inversely as the temperature, the variations in rapidity beginning only after variation in temperature has continued a short time. They conclude that a study of weather influences may have practical as well as theoretical value.

The effect of humidity is in general the same as that of temperature. Kuhnes found that 75% to 85% relative humidity was most favourable to best work, and when rel. hum. fell below 50% there was a marked drop in efficiency.

There is much difference of opinion as to the effects of barometric changes. The general idea seems to be that of Dexter, that the effect of a low barometer and high altitudes is invigorating. On the other hand,

however, Kuhnes found that a barometric pressure of about .10 above normal seemed to be most favourable; and Lehmann and Pedersen also found an increased efficiency with high barometric pressure.

While Peaks' own data of weather-influences are concerned chiefly with the character (*i.e.*, cloudiness or sunniness) of the day, they also show that while a moderately high humidity is favourable for temperatures of from 40° to 80°F., it is unfavourable when the temperature is below that point. On the whole Peaks seems to think that the larger yearly variations in abilities are due to the combined influence of temperature and light-strength.

New Experiments

(1) My own experiment was conducted in the McGill Psychological Laboratory during the greater part of the session 1923-24. The *S*s were 5 men and one woman from the sophomore, and one woman from the junior year in Arts. These students volunteered to act as *S*s and took the tests seriously. Each student came once a week to the Laboratory, except on holidays.

The tests were of two kinds, a visual memory test, and an auditory memory test. The method of administration of the tests was as follows. (*a*) The *S* was given a list of 15 nonsense syllables to study for 3½ min. At the end of that time 1½ min. was allowed for the reproduction of as many of the syllables as could be remembered. (*b*) Two lists of numbers were read successively in the following manner: a list of 4 two-digit numbers was read at the rate of 1/sec., and then reproduced by the *S*; a list of 5 numbers was read and reproduced; a list of 6 numbers. The same procedure was carried out for a second similar series of numbers. (*c*) The nonsense syllables were again reproduced. The method of scoring was as follows. (*a*) Syllables: each correctly reproduced syllable received one mark, irrespective of whether or not it was in its correct order. For the purposes of my experiment this method was preferable to Lyon's complicated method of scoring (counting marks for individual letters and for correct order—which latter would unnecessarily increase the tax upon the *S*'s attention). (*b*) Numbers: the method used was the same as that adopted by Peaks: one mark for every correct whole number in right order; ½ mark for every correct whole number in wrong order; ½ mark for every correct digit in correct order (total 15 marks). I subjoin specimen lists.

(1) BAZ
HEK
MIB
WOD
JUX
KAK
GEB
RIN
BON
ZUL
YAB
HEX
SOF
DUJ
KIZ

(2)
(*A*) 43, 75, 91, 26.
82, 19, 65, 37, 41.
54, 69, 27, 13, 81, 46.

(*B*) 19, 74, 53, 26.
87, 35, 42, 16, 93.
12, 24, 36, 78, 59, 81.

A series of preliminary tests (varying in number from 3 to 5) was first administered to enable the *S*s to become acquainted with the nature of the tests, and to obviate, as far as possible, the practice-effects.

The barometric records are those of the McGill Observatory (the altitude of which is approximately the same as that of the Laboratory).

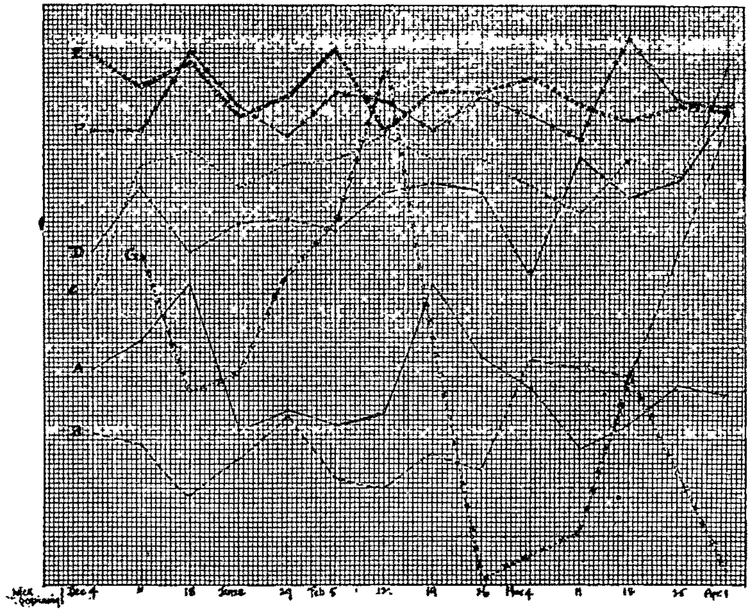
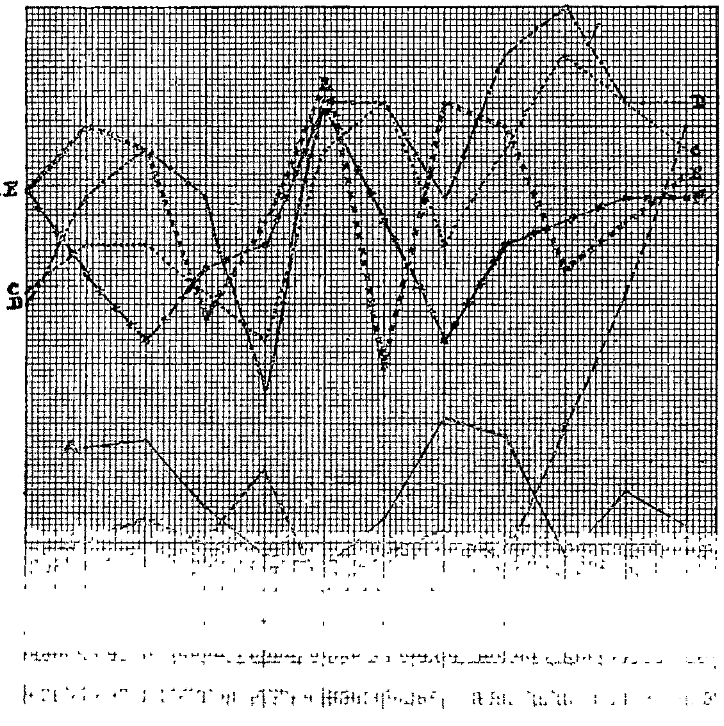


FIG. I. TABLE OF WEEKLY AVERAGES



(2) Sources of error are easily found. (*a*) Morgan has pointed out that irrelevant noises leave permanent effects on the memory. Absolute quiet for my experiments was not always attainable, and the test of 2 of the *S*s on March 11 was seriously disturbed by extraneous noises. (*b*) Entire absence of meaning in the syllables is almost impossible: *e.g.*, in the specimen list given above 2 of the *S*s were able to fix the last two syllables, because, pronounced in a certain way, they formed a friend's name. (*c*) The personal equation, *i.e.*, the individual variation in each *S* from day to day, in general health, fatigue, mood, interest and lack of interest, is an element which cannot be controlled experimentally. A passing mood or a state of fatigue at the time of learning or of testing will entirely alter the usual result.[4]

(3) Results have been drawn up graphically both for the individual *S*s and for the weekly averages of the group as a whole; but, for convenience, only one individual record, selected at random, will be used.

In general, the results are baffling. When the individual records were examined, it was seen that in 3 cases there is little or no relation shown between the "immediate-recall" syllables and the relative humidity curves; in one case there is an almost perfect direct variation; in 2 other cases there is in the general curve a direct variation; and in only one case (Fig. II) is there any evidence of inverse variation even in the general curve. The "weekly-average" curves, however, show an almost perfect inverse variation between these two curves. In this latter there are only three places where direct variation occurs: (*a*) Jan. 29, where no reason is apparent; (*b*) March 11, where the curve is altered from inverse to direct variation by the poor score of one *S*, who both changed his method of learning the syllables and had to cope with particularly noticeable outside noises; and (*c*) April 1, where a possible cause of the improvement in score is to be found in the fact that the *S*s knew it was their last test for the session.

Besides these contradictory evidences of the individual and average results, confusion is to be found in the average results themselves: *e.g.*, the relations of the "immediate-" and "delayed-recall" syllables to the relative humidity curve are contradictory, the former varying inversely, and the latter having a general tendency to direct variation.

The various relationships may be briefly summarized as follows.

A. Syllables: (1) *Relative Humidity*

(*a*) The relation of "immediate-recall" syllables has been discussed above. (*b*) The relation of the "delayed-recall" syllables is as follows. In general the "average" curve tends towards a direct variation with the humidity, and 3 individual records show the same tendency. On the other hand, one individual record shows an inverse variation; and the "average" direct variation contradicts the results of the "immediate-recall" syllables of (*a*) above (Tables II & VI).

(2) *Barometer*

(*a*) The "immediate-recall" syllables show a vague direct variation in the "average" curve and very variable results in the individual curves, from which absolutely no inference as to a relationship can be drawn. (*b*) The "delayed-recall" syllables in the "average" curve show a marked tendency to direct variation with the barometric pressure, while once again the individual records fluctuate so freely that no conclusions can be drawn from them (Tables II & VII).

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[4] In this connection I neglected to obtain any introspective records from the *S*s; so that I have been unable by this means to facilitate the interpretation of my results. Facts such as changes in the hour of rising may have had their influence; for one occasion was brought to my notice on which one of the *S*s made the time of my experiment the time of her first appearance.

B. Numbers

If the visual-memory test, however, is barren of results, the auditory test is, in general, fairly fertile. As stated above, two similar series of numbers were read for the test. An examination of the results shows that No. II yields fairly clear-cut results, while the results of No. I are not so definite. From this and other factors (*e.g.*, speed of reproduction, etc.) it may be concluded that No. I is a necessary preliminary to ensure good results from No. II. It obviates to a great extent practice-effects, and allows the *S*s to accommodate themselves to the *E*'s voice, rate of reading, etc.

(1) *Relative Humidity*

(*a*) No. I. Three individual records show general inverse variation: but results from one individual and the "average" curve have the opposite tendency. (*b*) No. II. Five individual records show marked inverse variation and the tendency in the "average" curve is still more definite. These results are very clear-cut.

(2) *Barometer*

(*a*) No. I. In neither the individual nor the "average" curve is there any marked degree of correlation. (*b*) No. II. Four individual curves show an inverse variation and in the "average" curve inverse variation occurs quite noticeably.

(4) In addition to the influence of the humidity and pressure I have considered the relation of the scores on the "average" chart to the temperature curve (Table III & Fig. I). No. II again furnishes the most marked correlation—the general tendency being again toward an inverse variation. This inverse variation is almost as well shown by No. I. The curves for the syllables do not show any marked correlation except a direct variation in the case of the "delayed-recall" curve for the month of February.

The findings in my graphical records tend to contradict rather than to substantiate Peaks' opinion that temperature is the most influential weather-factor; for an examination of Fig. I reveals the fact that, while the variations of temperature (10° range) are far more sudden and greater than those of pressure, the memory-curves show a gradual rise or fall, more in consonance with the pressure- than the temperature-curve.

(5) Finally, a comparison of my results with those of other investigators of seasonal periodicity yields the following results. While others find a period of decreasing efficiency from December to the end of February followed by a rising period till May, my results (Tables IV & V) show very little "average" monthly variation, and where variation does occur it usually points to February as the month of increased efficiency.

Practice-effects may have had some influence in this inconsistency: but another factor may be held accountable to a greater degree—the difference in the number of experiments held each month. Peaks states that a full month's total of experiments is necessary to illustrate the seasonal variations. Since my data comprise only two (or three) in December, two in January, and one in April, any definite conclusion from them as to the nature of seasonal periodicity is unwarranted.

(6) The following general conclusions may be drawn from my experiments. (*a*) When the number-series is used for tests of this kind a complete set should be read and reproduced merely as a preliminary, to allow for accommodation and to obviate, partially, the effects of practice. (*b*) Since subjective "feelings" are fairly constant during so short an experiment (each day) it would [illegible] auditory more than visual memory. Since the visual test was one of memorizing this inference may not be strictly accurate: but the results [illegible] (*c*) There [illegible] no quantitatively-measurable influence of weather conditions on visual [illegible] in my tests. (*d*) Auditory memory [illegible]

Table I

Week of:	Dec. 4	11	18	Jan. 22	29	Feb. 5	12	19	26	Mar. 4	11	18	25	Apr. 1
Barom.	29.01	30.01	30.25	29.65	29.71	29.66	29.71	30.25	29.93	29.81	29.55	29.66	29.83	29.79
Rel. Hum.	40.83	39.57	39.2	38.14	42.57	36.	35.	38.66	37.16	48.33	47.57	46.66	59.57	74.62
Sylls, "Imm-Rec."	7.57	10.71	11.2	10.28	10.86	11.	11.66	11.	11.	10.33	9.57	11.	10.57	13.5
Sylls, "Del-Rec."	8.66	10.28	8.6	9.28	9.43	9.16	10.16	10.33	10.16	8.	11.	10.	10.43	12.25
No. I	13 6/8	12 7/8	13 4/8	12½/8	12 5/8	13 7/8	11 6/8	12 6/8	12 6/8	13 1/8	12 3/8	12.	12 3/8	12 2/8
No. II	11 6/8	11 6/8	13 5/8	12 3/8	11 5/8	12 6/8	12 4/8	11 6/8	12 6/8	12 1/8	11 4/8	14 1/8	12 4/8	12 3/8

Table II

Correlations for Average Weekly Results

I Imm.-Recall	(a) Rel. Hum.	inverse in. in. direct in. in. in.—in. d. in. in. d.	(inverse)
	(b) Barom.	d. d. d. d. in. d. in. d. d. d. d. in. in.	
II Delayed-Recall	(a) Rel. Hum.	in. d. d. d. d. in. d. d. in. in. d. d. d.	(apparent dir. var.; conflicts with I (a))
	(b) Barom.	d. in. in. d. d. d. d. d. d. in. in. d. in.	
III No. I	(a) Rel. Hum.	d. in. in. d. in. d. d. — d. d. d. d. in.	
	(b) Barom.	in. d. d. d. in. in. d. — in. d. in. d. d.	
IV No. II	(a) Rel. Hum.	*— in. in. in. in. d. in. in. in. d. in. in. in.	(inverse variation)
	(b) Barom.	*— d. d. in. in. in. in. in. in. d. d. d. in. d.	

* Due to poor score of 1 subject

Table III

Temperature Readings

Week of	Dec. 11	18	Jan. 22	29	Feb. 5	12	19	26	Mar. 4	11	18	25	Apr. 1
	71.6	69.	69.4	71.4	72.5	75.7	70.	65.1	65.6	66.1	69.3	67.3	65.3
Variation with No. II		inv.	inv.	inv.	dir.	inv.	dir.	inv.	inv.	inv.	dir.	dir.	inv.

Table IV

Individual Seasonal Variations

	Sylls. Immediate-Recall					*Sylls. Delayed-Recall*					*No. I*					*No. II*				
	Dec.	Jan.	Feb.	Mar.	Apr.	Dec.	Jan.	Feb.	Mar.	Apr.	Dec.	Jan.	Feb.	Mar.	Apr.	Dec.	Jan.	Feb.	Mar.	Apr.
Hewetson	10.5	9.5	10.25	8.	*11*	7.	8.	8.25	7.5	*11*	11.5	13.75	12.6	12.25	*8*	10.75	12.25	10.	10.25	*9*
McLeod	11.3	12.	12.5	13.	*15*	11.3	12.5	12.75	13.	*15*	14.5	14.75	13.4	13.	*14.5*	15.	15.	14.4	14.5	*15*
Hyde	7.3	8.6	10.25	9.	*15*	5.7	7.	7.5	7.7	*13*	13.3	11.75	13.1	12.25	*13.5*	12.3	11.	12.25	11.6	*11.5*
Johnson	9.7	13.	10.	10.5	*12*	6.6	9.	5.7	7.25	*9*	13.1	14.25	13.1	14.	*14*	14.3	11.25	14.	13.9	*13.5*
Novick	14.5	*13*	12.5	11.4	*15*	12.5	*11*	12.	9.8	*14*	13.25	*9*	13.	12.5	*10.5*	13.5	*14.5*	12.75	13.1	*12.5*
Cossman	10.3	10.	11.5	10.5	*13*	11.3	8.5	10.7	11.	*12*	12.1	12.25	12.8	12.25	*12.5*	11.1	12.25	11.7	12.25	*12.5*
Sommer	10.	8.5	11.7	13.5	*12*	11.5	9.	12.7	14.	*13*	12.25	9.5	11.6	10.	*11.5*	8.75	9.75	10.4	10.25	*11.*

Italics = only 1 score

Table V

"Average" Seasonal Variations

	Immediate-Recall	*Delayed-Recall*	*No. I*	*No. II*
Dec.	10.6	9.4	12.8	12.2
Jan.	10.3 (or) 10.6*	9. (or) 9.3*	12.7 (or) 12.2*	12.3 (or) 11.9*
Feb.	11.2	9.9	12.8	12.2
Mar.	10.8	10.	12.3	12.26

* = with Novick's score (only 1 this month) included

Table VI

Relation to Relative Humidity

Subject	*Syllables "Immediate-Recall"*	*Syllables "Delayed-Recall"*	*No. I*	*No. II*
Hewetson	——	——	inverse variation	inverse variation
McLeod	direct variation	direct variation	——	——
Hyde	——	" "	general inverse var.	general inverse var.
Johnson	general direct var.	general direct var.	general direct var.	" " "
Novick	" " "	——	——	——
Cossman	——	——	general inverse var.	inverse variation
Sommer	general inverse var.	general inverse var.	——	general inverse var

—— = no correlation in variation

Table VII

Relation to Barometric Pressure

Subject	*Syllables* "Immediate-Recall"	*Syllables* "Delayed-Recall"	*No. I*	*No. II*
Hewetson	——	——	generalinversevar.	general inverse var.
McLeod	general inverse var.	vague general inver. variation	——	——
Hyde	——	——	——	general inverse var.
Johnson	——	——	——	" " "
Novick	——	vague general direct variation	——	——
Cossman	——	——	——	——
Sommer	——	——	——	general inverse var.

—— = no correlation in variation

influences to a marked extent. Relative humidity, barometric pressure and temperature appear to affect efficiency in auditory memory inversely, *i.e.*, as humidity, pressure and temperature rise, the scores for auditory memory become lower. (*e*) My results do not substantiate Peaks' opinion that temperature is the most influential weather-factor: they point rather to a closer relationship between barometric pressure and mental work. (*f*) In general a low barometer is favourable to mental work. (*g*) Initial ability and rate or amount of improvement do not appear to be quantitatively related. This result substantiates Thorndike's findings. (*h*) No really successful and definite conclusions can be drawn from an experiment of this character unless something is known of the *S*'s affective state at the time of the test. To supply this information introspection by [illegible]

While much work may be done along lines similar to the present experiment, an absolutely quantitative relationship between the weather influences and mental work cannot be established until the influence of fatigue, the personal equation, and other minor influences can also be measured to some quantitative extent.

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II. THE REACTION OF MEMORY TO AFFECTIVE STATES

BY A. RENDLE STONE

Purpose of Experiments and Introductory Remarks

In the following series of 7 experiments, I have tried to discover if there is a relation, and, if so, what, between memory and affective states. The last 6 experiments have a common method, and a much closer relation to each other than to the first one. The ground covered by these 6 seems to have been so untouched that I have been unable to compare their results with any others.

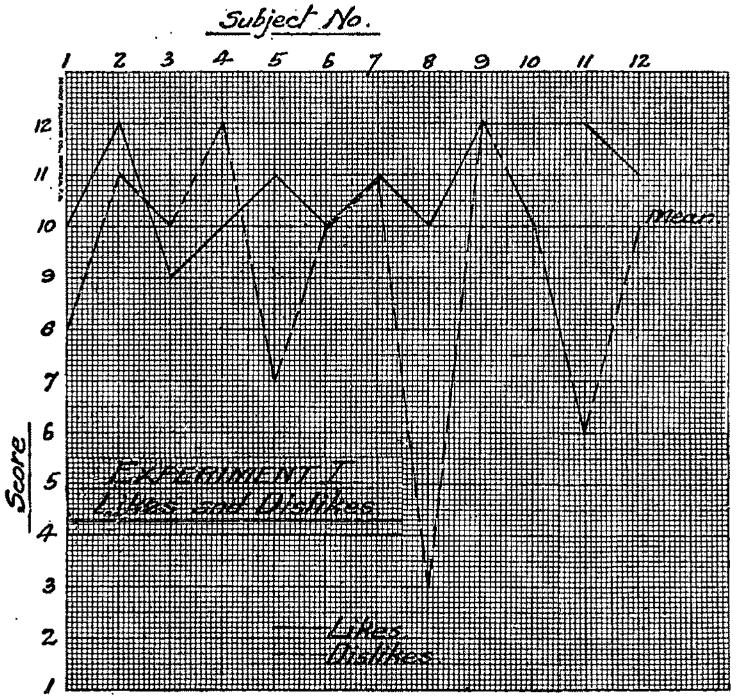
The same thing cannot be said about the first experiment, which, being along the hedonic lines of recall of likes and dislikes, is by no means the first of its kind. However, several of the experimenters have arrived at different results, and as my method seems to be a new one, it may have its place.

History of Previous Experiments.

W. W. Smith[7] has given a summary of earlier experiments. For convenience, I will briefly mention some of them.

The first experiment on the recall of pleasant and unpleasant experiences was made in 1898, but the results were valueless. K. Gordon, the next investigator, found no difference in their facility of recall. Kowalewski draws some conclusions, but he is acting on an assumption that pleasant and unpleasant experiences are equal in number; so they cannot be accepted. W. W. Smith's conclusion is that memory is influenced by affective tone; the intensely toned experiences are better remembered or more easily forgotten. His inference seems justified. W. D. Tait concludes that the pleasant experiences are remembered better than the unpleasant, but adds that such a conclusion must be taken with care, as the unpleasant experiences may be remembered potentially as well as the pleasant, but may then be suppressed. A. Wohlgemuth found a slightly greater tendency for the forgetting of pleasant experiences than for the forgetting of unpleasant ones, but ends up by saying: "There is no difference whatever in the effect of each tone, as the variation is so slight as to be accounted for by individual differences." C. Fox, experimenting on

the influence of subjective preference on memory, decides that in the memorizing of two things a preference for one causes that one to be remembered better. It seems to me, however, that the way in which he draws his conclusions is questionable. His method was that of giving his *S*s sonnets to learn. *After* they were learnt, the question was put which one the *S* preferred. But it seems to me that facility in learning would



be a cause for preference, just as difficulty in learning might prejudice a *S* against a sonnet. Baxter, Yamada and Washburn in 1917, and Morgan, Mull and Washburn in 1919 undertook two experiments along similar lines. Although the evidence was not conclusive, it showed a tendency for the temperament or disposition of the *S* to have an influence on the matter remembered: the cheerful person remembering pleasant, the pessimistic, unpleasant impressions the better.

New Experiments: Method

I now come to my own experiments. I will take up the first [illegible] itself and then turn to the other six. In general, it may be said that the experiments were carried out on 12 *S*s—10 men and 2 women—all students in the second year of Arts, McGill University, and all but one from the elementary course in psychology. The experiments took place in the McGill psychological laboratory, the *S*s being taken in pairs at the same

hours weekly. There were comparatively no distractions. Gates[16] has shown that at different times of the day ability to remember varies. Memory also seems to vary with meteorological changes. However, as each experiment was completed in half-an-hour, and as the results were only compared in a relative way that will be seen when they are examined, the above mentioned influences should not affect the evidence.

Experiment 1

The particular method of Experiment 1 was as follows. Each *S* was asked to write down a list of 24 names, 12 being of people he liked and the other 12 of people he disliked. The likes and dislikes were to be in separate columns, and, if possible, composed of 6 of one and 6 of the other sex. The sex was not insisted on, however, but it was emphasized that the likes or dislikes must be really felt. About a month later the *S* was shown a series of photographs of persons he had never seen before. The photographs were, for the most part, of identical background, being cuts from the same periodical. Each picture was intended to correspond to one of the names the *S* had submitted; and while the *S* was being allowed to examine the picture for a moment, he was told the corresponding name. (This method should have been made more systematic by accurate timing of the pictures' exposures; but I think they were fairly regular.) After a brief pause, the pictures were again shown, one at a time as before, and the *S* was asked to remember the name allotted to each. The above procedure was gone through for each series separately, not both together. To compensate for any possible slight error due to learning, in half of the experiment the likes were shown first, and the rest of the time the dislikes alternated. One point was counted in the *S*'s score for every right name for the right picture. The results are shown in the accompanying graph.

My conclusions are as follows. (1) Only on two occasions does the dotted line top the continuous. It equals it in 3 more places, while the continuous is higher in 7 places. This makes the ratio 3.5/1. Therefore pleasant impressions are remembered better than unpleasant. (2) There are great individual differences in memory for dislikes (perhaps this is where temperament comes in), while there is not nearly so great a fluctuation for likes.

Remarks on Method Common to Experiments 2-7

After explaining the method in general for the remaining 6 experiments, I will take them up in detail. As a criterion of memory, the learning of nonsense syllables was used. They were unassociated and in lists of 15. I consider such material much superior to any other kind for the purpose desired—certainly better than poetry, etc. The *S* was allowed to concentrate on a list for 2 3/4 min., which, by experiment, was found to be the most satisfactory period. The times were taken with a stop-watch. When the 2 3/4 min. were up, the lists were removed, and the *S* was instructed to try to make his mind as blank as possible and try to think of nothing. After 2 min. of such passivity, the *S* wrote down on a slip of paper all the syllables he could remember. As soon as he had finished (all the time desired was allowed, but this usually amounted to not more than 2 min.) his blood-pressure was taken. The palpatory method was used, and the systolic pressure noted. A few minutes' rest interval was here interposed, after which another list of syllables was submitted, the time being the same as before. As soon as the time was up, a stimulus (the stimulus was different for each experiment, so will be mentioned sepa-

rately) was given, lasting the same time as the corresponding period o passivity. Again the *S* was asked to write down what he remembered, and the final systolic pressure was noted. Lastly, the *S* was asked to give any introspection that might bear on the result, with particular attention as to whether or not emotion was produced. Precaution was taken against any irregular fluctuation in the score of syllables remembered by submitting the *S*s to a long period—about two months—of training at memorizing the syllables. This should have eliminated any learning curves. Time was also spent in experimenting on the right length of lists, comparative values of associated or non-associated syllables, the best length of time to submit them for, the most advisable interval to allow to elapse before recall, whether the recall should be visual or auditory, and so on. I found much that was helpful in other experiments on memory. In particular, I learnt from W. Brown[10] that the number of syllables forgotten in attempted recall is fairly constant for some time after the first brief interval, in which most of the forgetting occurs. This was just what I noticed myself; so it was important to have a slight interval after the first learning to compensate for the time taken in introducing the stimulus after the second learning. In calculating a *S*'s score, order of words was not considered, but each syllable perfectly recalled was marked up as one point; half points were allowed if two of the letters were correct. The stimulus was given to arouse some type of emotion, and, as an index to the intensity in which the emotion was produced, the blood-pressure was examined. The evidence of the blood-pressure was pretty generally supported throughout by the introspection; it does not seem necessary to include all the introspection in detail in this report, however, so I will only refer to it where it is of special service. Graphs alone have been used in recording the results, as they seemed to serve all purposes most conveniently.

Experiment 2

The stimulus used in this case was an easy test in Psychology. The interval after learning was lengthened to 4 min., giving 1 min. each to answer 4 simple questions. A sentence or two for each question was all the answers called for. The *S*s were told that, although observations on the examination were to be taken for experimental purposes, the marks given for their answers would count in their final standing at the end of the year in their psychology course. Although a few were slightly suspicious, they could not be sure; and, not wanting to take any chances, all accepted it as a *bona fide* test.

The accompanying graph must now be referred to to follow the conclusions, which are as follows. (1) In this and the following graphs, when a line (either blood-pressure or score) approaches one of the lines which mark every tenth space on the graph, it denotes merely individual difference. (2) In this and the following graphs, when a line (either b.-p. or score) approaches one of the lines which mark every 5 divisions on the graph, this denotes the relation of b.-p. to score. (3) Whenever (and this holds for the following graphs) there is a noticeable jump in the blood-pressure line from a 10-division line to a 5-division line, the presence of emotion is inferred. (4) The practically uniform result may be noted that, the greater the emotion aroused, the greater is the variation in score, either positive or negative. (5) Analysis of pressure and score relations. Under the A column the cases of lowered pressure and lowered score and, *vice versa*, increased pressure and increased score, are noted. Under B are the cases of lowered pressure with increased score and vice versa.

S	A	B	Comment
1	1		
2		1	But not much change in blood pressure
3	1		
4		1	" " " " " " "
5	1		
6	1		" " " " " " "
7	1		" " " " " " "
8		1	Great difference in *S*'s scores. His low score lowest of all, so may be ruled out
9	1		But not much change in blood pressure
10		1	

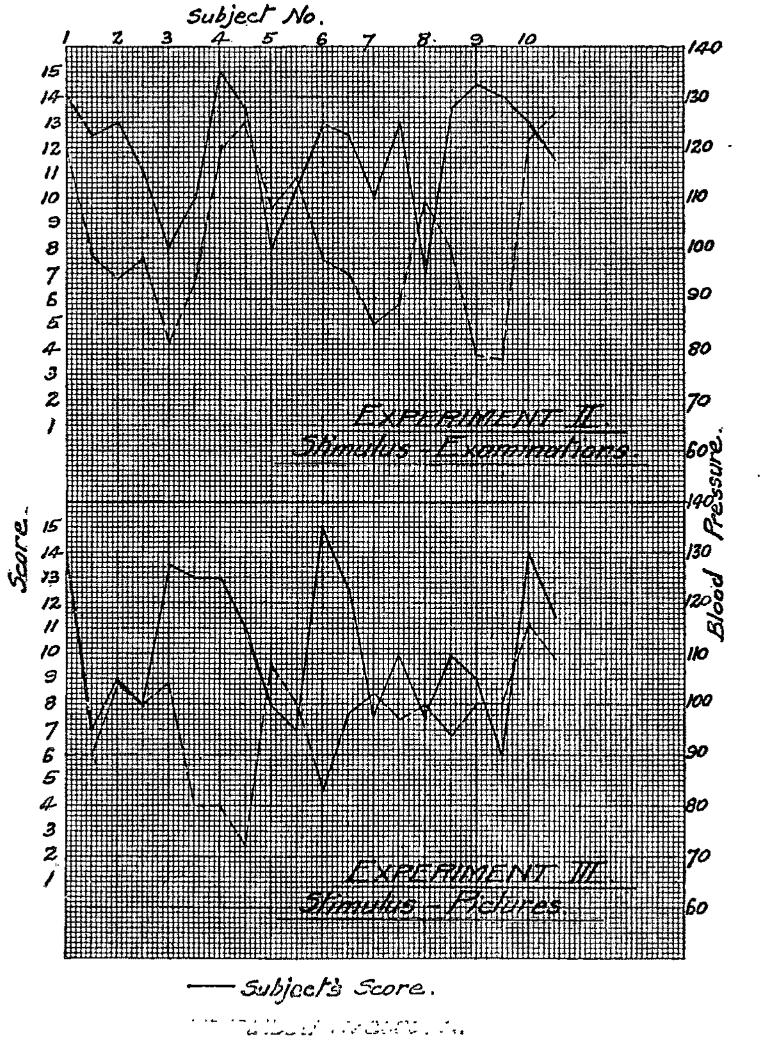
We have three good instances to one against the proposition (and its reverse) that with increase of b.-p. memory gets better. (6) In most cases the b.-p. changes for an increase.

Summary. The conclusion I would draw from this experiment is that an examination seems to stimulate quite a decided emotional excitement, as indicated by a considerable change—either increase or decrease—in blood-pressure. This emotion affects the memory of the *S* to quite an appreciable extent; the effect may be either positive or negative, *i.e.*, the memory is either increased or impaired quite largely in efficiency, depending on the individual. *The likelihood is that the blood-pressure will increase, and the memory become better.*

I suggest the following as a possible explanation. "All parts of the cortex, when electrically stimulated, produce alterations both of respiration and circulation . . . The brain itself is . . . a sponge full of blood" (W. James). Mosso's observations on the brain-pulse showed an immediate afflux of blood whenever mental activity was quickened by any cause whatever, intellectual or emotional. The emotional excitement produced by the examination, then, would affect the cortex both directly and indirectly: directly, by the stimulation of the emotion itself; indirectly, through increasing the blood-supply, and thereby making conditions more favourable to cortical activity. Sometimes we saw that the blood-pressure was lowered by the examination, decreasing the efficiency of the memory. In this case, the converse of the above would be an explanation. Of course, it is unlikely that these are the sole influences at play. Glandular activity is also much increased. A number of urinal analyses made in the United States a short time ago disclosed the presence of sugar in the urine of a great majority of a large number of students going up for examination. Under normal conditions, this would be a pathological state; so its occurrence here is illuminating. The adrenal glands are also probably active, and the increase of adrenalin in the blood should be an aid to memory.

Another interesting aspect of memory fluctuation caused by the emotional excitement of an examination is "the ups and downs" of efficiency in the same *S* during the same examination. While not approaching it on an experimental basis, I have an interesting personal experience in this connection. On the occasion of an examination in philosophy a year ago (taking place 9 to 12 a.m.) I found, after reading the paper, that everything was a complete blank. By 11 o'clock I had only one question out of 6 answered, and thought myself due for a failure. I felt greatly disappointed, as it was the last of a strenuous series of examinations, and I was convinced that I had passed all the rest successfully, and had expected to do better in this one. By a strange accident, I had set my watch an hour fast that morning, before coming into the examination. The bell rang, my watch read 12 o'clock, and I thought the time was up. I walked to the front of the room to hand in my paper; but, as no one else moved, I inquired the time from the proctor. Somehow the news that I still had another hour worked an enormous change in me. I returned to my seat

and worked without a pause till my time was up. I do not think I ever wrote faster. I nearly finished the paper, and made a second class pass, which is somewhere between 65 and 75%. This seems a good instance of such memory fluctuation.



Experiment 3

The emotion which it was attempted to induce in this experiment was horror. The stimuli were about a dozen 10 by 12 in. pictures of Russian

famine victims, particularly children, in a state of extreme emaciation. They were obtained from the dominion government at Ottawa, and were very clear, and excellent for the purpose.

The following are the conclusions from the graph. (1) The first thing noticeable is that, with two exceptions (*S*s 7 and 8), the effect of the stimulus was a decrease in the memory score. In most instances the decrease was quite decided, and in one instance (*S* 1) quite remarkable. These two exceptions are significant as occurring during the same hour and under identical conditions. It will be noticed that the *S*s made exactly the same score in both cases, and there is also only one point of difference in the decrease of b.-p. In reply to the question whether any emotion was aroused or not, their introspection was: (*S* 7) "Not moved much;" (*S* 8) "Moved to some extent, perhaps." The blood-pressure here supports this introspection, and indicates that both were moved to only a small extent, the latter a little more than the former. I imagine that all introspection in cases like this, where the emotional excitement is slight, would err in underestimating the effect. There is one possible explanation for the contradiction of these two results to the others. These two *S*s had not had the benefit of the long practice in memorizing the syllables that the others had, as they were taken into the experiments late in the session. Thus the increased score might be due to learning. If this was not the case, these two results are directly contrary to all the others. (2) The blood-pressure also decreases (with 3 exceptions, in 2 of which there was no change whatever). This seems to show that emotion was excited; but the change is not so great as in the preceding experiment. (3) There does not, however, appear to be such a clean-cut relation between score and blood-pressure. In many cases (*e.g.*, *S*s 1 and 3), where the one line indicates a great change, the other hardly fluctuates. There are 3 exceptions (6, 7, 8) to the statement that decrease in memory is accompanied by decrease in blood-pressure, or *vice versa*, and in 2 other cases (1, 9) the pressure remains stationary during a big decrease in score.

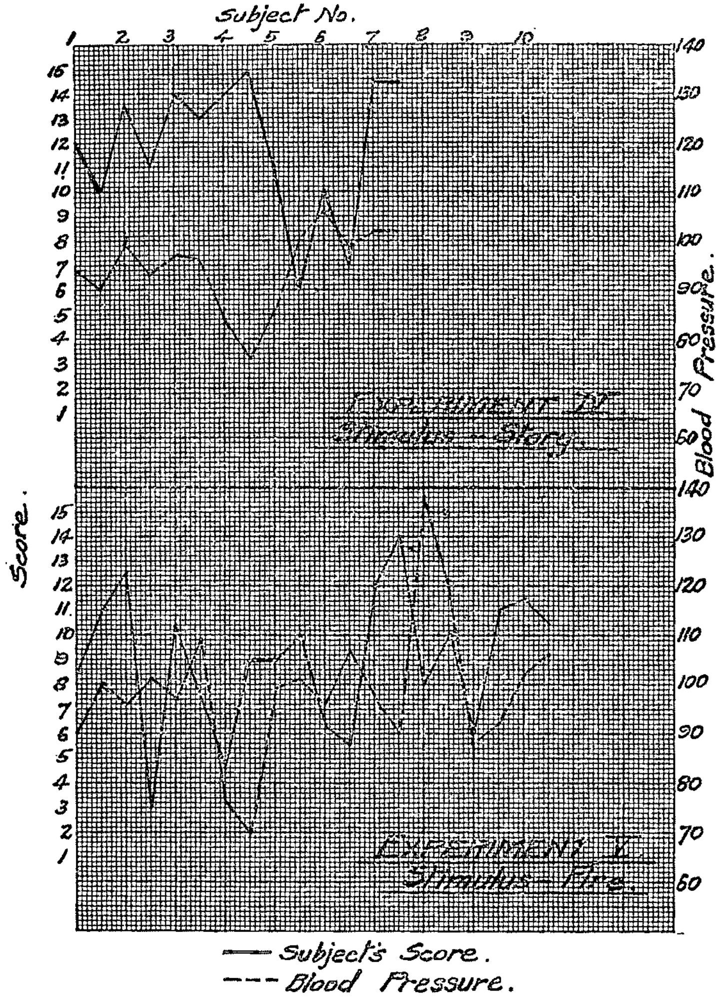
Summary. The emotion of horror reacts on memory with a negative effect.

Experiment 4

The stimulus in this experiment was the reading of a selection from Francis Parkman's "Jesuits in North America." The passage dealt with the torture by the Indians of Jesuit priests, and was full of revolting cruelties.

The conclusions from the graph are as follows. (1) This experiment has one result of particular interest to this series, *viz.*, that of *S* 7. Here the noticeable fact is that there was change in neither the blood-pressure nor the score. Looking up the *S*'s introspection, I find that he considered that no emotion whatever had been aroused in him. This is exactly in accordance with the theory on which I have been working—that, other conditions remaining invariable, all changes in memory will be due to the induced change of emotion (with blood-pressure as its index). Here the affective state is absent, so the memory stays constant. (2) The stimulus of this experiment should, from the results of Experiment 3, as the emotion produced is much the same, have a negative result on remembering the syllables. This, we see, is the case, with the exception above noted and explained, and the exception of *S* 4. A possible explanation for the other exception is this: the *S*'s introspection in this instance disclosed no emotion, therefore the fall of 8 points b.-p. was probably due to some other cause. The *S* was a little late in arriving that morning and had probably been hurrying, in which case this fall might be expected. The increase in score is only of one point, and so might easily occur without significance for the general result. (3) In most cases the hearing of the story caused the

pressure to fall. In one case (*S* 5) it rose. From the somewhat mixed nature of the emotion aroused by this stimulus, however, I do not see that the direction of the fluctuation matters much, so long as there *is* a fluctuation. It may easily be realized that excitement as to the result to the



[illegible]
them, etc., might be incited instead of impersonal horror at the tortures themselves. In this case a rise in pressure might be expected.

Summary. The affective state produced by such a stimulus as was used in this experiment, which we may call a combination of horror, dread, fear, suspense, perhaps some indignation, reacts on memory with a negative effect

Experiment 5

The stimulus for this experiment was brought about as follows. The waste-paper basket (wire) was filled with paper before the two *S*s came into the laboratory. The *S*s were seated with the basket at their backs about a yard away. While they were studying the second list of syllables, I lit my pipe and dropped the lighted match into the basket as if thinking it was out. The basket was timed to burst into flame the moment their time was up. It made quite a blaze, and was dangerously close to a desk on which were some useful papers and books. It created quite a surprise, filling the laboratory with smoke and ashes.

The reaction was usually varied. In most cases a startled exclamation was uttered. Sometimes the *S*s moved the basket away from the desk; sometimes they sat and looked at it, in which event I moved it. Sometimes they tried to put it out; sometimes they let it burn. In all cases the fire was taken for a natural accident—at least at the first, although a few began to be suspicious after a while, when they remembered that they were in a psychological laboratory.

Conclusions from the graphs are as follows. (1) The score curve for the experiment, compared with the preceding ones, seems to be most marked in its great fluctuations. Evidently the stimulus had quite a big effect, for we see (to pick out the 3 most apparent) a change from 12.5 to 3 (*S* 2); from 4.5 to 9 (*S* 4); from 6 to 11 (*S* 9), etc. (2) In 6 cases the above change was positive, in 4 negative; while the intensity was about the same for both positive and negative. (3) The changes in b.-p. are not correspondingly great. In the instance (*S* 8) of the highest pressure, 138, and the greatest change, it was ascertained that the *S* had been playing basket-ball immediately preceding the experiment, and so this change may be discounted. (4) The proportion of rises to falls in b.-p. is 7 to 3. This seems natural from the nature of the stimulus; however, the direction is not important, as the specific affective states may vary somewhat. It is only the change and the extent of the change that should matter. (5) The ideal result would be to see a perfect ratio, either direct or inverse, between the two curves. On the surface, however, this does not seem to be the case. *E.g.*, with *S* 2 an enormous drop in the score curve is accompanied by only a slight deviation in the b.-p. curve. But it seems to me that this is just one more instance where it behooves the *E* to beware of extreme results. In an experiment like this, where it is practically impossible to discover every *vera causa*, the *E* is justified in assuming at times that some influence may be at work of which he is ignorant, and so may ignore certain results which are seeming exceptions, and which he cannot explain. This, of course, only where he believes that, if explained, they would be in accordance with his conclusions.

Since writing the above, I discovered a further note on the experiment, which bears out this opinion completely. *S* 2 said he was suddenly fatigued after learning the first list, and got completely "fed up" with it; *ergo* his final score.

In this experiment I would call attention to the results of *S*s 1, 3, 5, 6, 7, 8 (with the allowance noted) and 10. Here are 7 good results with the ratios quite close to the desired.

Summary. The affective state produced by stimuli of a similar nature to the one used in this experiment has a marked reaction on memory, which it affects either positively or negatively, in direct proportion to its intensity.

Experiment 6

A list of association words was used to stimulate the emotion of "love" for this experiment. The list was intended to be suggestive of a couple "spooning" on the beach in summer, accompanied by a sympathetic moon. The *S* wrote down a response word to the one read, times of the long reactions being noted. Judging by the response words and reaction times, emotion was aroused.

Conclusions from the graph are as follows. (1) The results of this experiment seem particularly good with regard to a relation between amount of variation in b.-p. and amount of variation in score. In 5—one might almost say 6—out of the 7 cases the variation is gratifyingly proportional. (2) In 5 out of the 7 instances, the effect on the score is negative, and the proportion is the same for an inverse relation to the blood-pressure.

Summary. The emotion of "love" has a negative effect on memory in inverse ratio to the direction, and direct ratio to the intensity, of the concomitant change in blood-pressure. (This calls to mind the traditional absent-mindedness, sheepish forgetfulness, and stutterings of the lover.)

Experiment 7

Grief was the emotion experimented on. The *S* was asked to imagine the death of a mother or father and to work himself up as much as possible. The James-Lange theory of the emotions was also used in the sense of a partial cause. Taking the expression of the emotion as outlined in Darwin's work[1] as a model, the *S* was asked to simulate the bodily changes the emotion induces—relaxed muscles, slouched shoulders and head, drooping mouth and furrowed brow, etc.

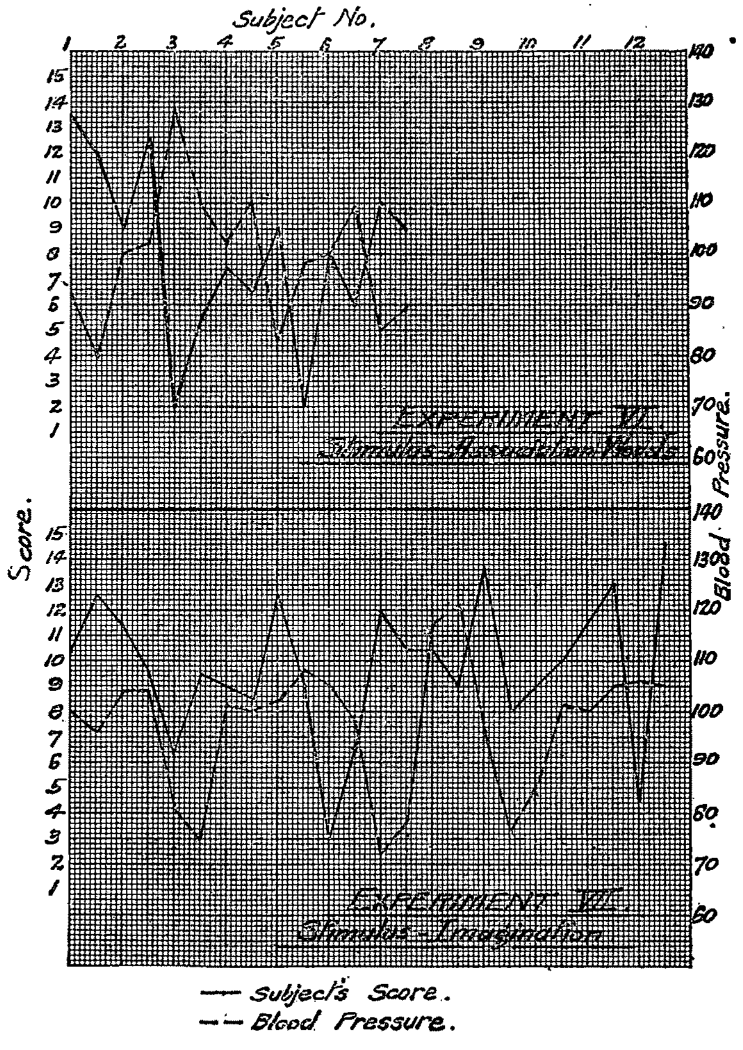
Conclusions from the graph are as follows.

(1) *S* No.		1	2	3	4	5	6	7	8	9	10	11	12	Total
Score	Up	1	—	1	—	—	1	—	—	—	1	1	1	6
	Down	—	1	—	1	1	—	1	1	1	—	—	—	6
B.-P.	Up	—	—	—	—	*	—	*	*	—	1	1	—	5
	Down	*	—	*	1	—	*	—	—	1	—	—	*	6

The asterisk means an inverse ratio.

(2) The result of *S* 2 will have to be ruled out, as it is a solitary instance of no change in b.-p. (3) The result of *S* 12 should be ruled out as: (*a*) it is a freak result in score; (*b*) the *S*'s introspection disclosed no emotion; (*c*) it is impossible that the exceedingly small change in b.-p. (explainable on the ground of natural systemic fluctuation) should indicate the reason for such a big jump in score. (4) All other results indicate a disturbing influence of this affective state on memory. (5) From the above Table (Conclusion 1) it is seen that the score may be either increased or decreased, and that the blood-pressure may either rise or fall, without any probability in favour of either direction. There seems to be some tendency for [illegible] should hardly be justified in setting this down definitely as a conclusion; in [illegible] an excellent exception.

Summary. The emotion of grief has a decided influence on memory. This influence may be either positive or negative. The concomitant change in blood-pressure may be either a rise or a fall.



General Conclusion from Experiments 2-7

Memory is greatly influenced by the *S*'s affective state at the time of the attempted recall. Some states help, some hinder; others may have either result, depending on the individual.

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MINOR STUDIES FROM THE PSYCHOLOGICAL DEPARTMENT OF NORTHWESTERN UNIVERSITY

Communicated by ROBERT H. GAULT

IV. A SURVEY OF THE INTELLIGENCE AND ENVIRONMENT OF SCHOOL CHILDREN

By TONAN FUKUDA

I. *The Nature of the Study*

The present Study comprises the results obtained in the survey of the school children of the Washington School, Evanston, Ill., during the months Feb. to May, 1923, to ascertain the intelligence norms and their relations to nationality, school training, and environment, factors commonly regarded as having a direct bearing upon the intelligence of the children. The first part of the paper is devoted to the study of the intelligence of the children and its relation to such factors as the teachers' estimate of their intelligence or school success, while the latter part is taken up with the study of the environmental conditions of the children and the correlation between their environment and intelligence.

For the study of the intelligence of these children the Terman Revised Binet-Simon Individual Test is used. Fortunately, in 1920, Mr. Walker, the Assistant Superintendent, tested some of the children with the Kingsbury Group Intelligence Test, and the results are still more or less available for us in checking up some of our own results. Moreover, they afford us an opportunity, though incidental to our main purpose, for the comparative study of the two tests, which will be taken up later. For the study of the environment of the children we rely chiefly upon the Whittier Scales for Grading Home and Neighborhood Conditions with our modifications.

As the city of Evanston is so situated as to connect with the northern part of Chicago, the race movement started in the maelstrom of the latter city is immediately felt by her. This fact makes our Study the more interesting, as Washington School is located in the southern part of the city, where the foreign speaking people are increasing in number. To bring out the situation we give here the names of the countries which are represented by the parents of the children here discussed, together with the number of the children studied: United States (Whites), 78; United States (Colored), 6; Canada, 2; England, 19; Scotland, 3; Ireland, 1; Sweden, 63; Poland, 27: Germany, 25; Norway, 11; Greece, 6: Bohemia, 4; Russia, 3; Russian-Jew, 3; Italy, 2; Denmark, 2; Holland, 1; Mexico, 1.

Most of these foreign-born people speak their own native languages at least at home. They are mostly laborers of various kinds. The district is usually classified as "not so favored" as the other districts of the city. The following Table will give a general idea with regard to this point.

Type of Occupation	*Number*
Business, Office work	29
Transportation work	43
Small store	37
Skilled labor	59
Unskilled labor	52
Miscellaneous	37

The Washington School is a public elementary school. The children are divided into 8 grades with 3 divisions, C, B, and A, in each grade. Although there is a kindergarten attached to this school, a study of the children in this department is omitted from our survey. The number of the children studied totals 257. The total enrollment in the school is numbered as 265, but owing to the various unavoidable causes, such as language dif-

ficulties and the impossibility of even ascertaining the date of birth, 8 children are neglected in the present Study. The following Table shows the classification of the children, according to their chronological ages.

Age	*Number*
5 to 6	1
6 to 7	15
7 to 8	34
8 to 9	23
9 to 10	33
10 to 11	29
11 to 12	29
12 to 13	34
13 to 14	27
14 to 15	18
15 to 16	12
16 to 17	2
Total	257

II. Intelligence and Correlated Factors

Naturally, children who hear a foreign language at home are handicapped in a test that is based largely on words.[1] This handicap however, falls especially upon such vocabulary tests as Terman's revision of the Binet-Simon Test. Therefore, in the present Study, we took care to substitute, whenever possible, alternative tests of non-vocabulary nature for the vocabulary tests and tests of similar nature. Of course such care does not remove the handicap entirely; but we have reason to believe that if care is taken whenever possible, such a handicap is not great even in the cases of older children with whom some use of the vocabulary tests is inevitable. As evidence we present here the location of mental age and vocabulary score of the total 148 children of rooms 5, 6, 7 and 8, to show the close relation obtained between the mental age and the vocabulary score.

TABLE I

Location of Mental Age and Vocabulary Score of the Total 148 Children of V, VI, VII, and VIII Rooms

Mental Age	*Vocabulary Score* 10–14	15–19	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–64	65–69
8-5			1									
9-0		4	5	2								
9-5	1	1	4	3	6				1			
10-0		2	4	2	16		1					
10-5			1	1	6		1					
11-0			1	2	12	4	6					
11-5					2	1	6	1	1			
12-0					2	1	6	1	2	1		
12-5							6	2			1	
13-0							4	2	3			
13-5								1	3	1	1	
14-0									2	2		
14-5							1		3	1	1	
15-0										1		1
15-5										1		
16-0												
16-5											1	

[1] See R. Pintner and R. Keller, Intelligence Tests of Foreign Children, *Journ. Educ. Psych.*, 13, 1922, 214–222. Also S. S. Colvin and R. D. Allen, Mental Tests and Linguistic Ability, *ibid.*, 14, 1923, 1-20.

Aside from the care taken in connection with the linguistic difficulties, the procedure in giving the test departs from the standard given by Terman in the tests of fables and the test of the similarities of three things. Instead of following the usual individual method a group testing is adopted in these two tests. It is, however, used for the rather older children of rooms 6, 7 and 8. The purpose was to save time.[2] For this task Mr. Phipps, a graduate student in Northwestern University, was engaged; but the scoring was, of course, done entirely by the writer. In giving the tests in this way the time element was neglected. The failure was carefully checked up, and was individually investigated. Of the 49 cases of failure in the test of fables none was successful after the testing without outside assistance. Of the 41 cases of failure in that of the similarities of three things only three children reported success afterwards; it was not certain whether or not they had received any outside help. Moreover, in such cases of investigation, a considerably long period of time after the testing must be seriously considered. On the whole we have seen no serious drawback in this group-method of testing as included in the present Study. The following two Tables show the cases and relation of success and failure in these two tests.

TABLE II

Distribution of Scores of the Fable Test of 101 Cases of 6, 7 and 8 Rooms

Score	*Mental Age*								
	9-0	10-0	11-0	12-0	13-0	14-0	15-0	16-0	Total
Above 8				2	3	5	1	1	12
4 to 8			10	13	10	6	1		40
Below 4	5	21	16	5	2				49

TABLE III

Distribution of Scores of the Similarities-of-Three-Things Test of 101 Cases of the 6, 7 and 8 Rooms

Score	*Mental Age*								
	9-0	10-0	11-0	12-0	13-0	14-0	15-0	16-0	Total
Plus	1	6	12	15	13	10	2	1	60
Minus	5	15	14	6	2	1			41

Results.—The following Table shows the distribution of the intelligence quotients of practically the entire school and also the median and average scores, according to the grade.

TABLE IV

Distribution of I. Q. and Median and Average Scores of 257 Unselected Children

I.Q.	*Grade*								
	I	II	III	IV	V	VI	VII	VIII	Total
130		1							1
120				2					2
110	5		3	5		2	1	4	20
100	3	9	10	4	5	7	2	6	46
90	14	21	10	14	9	9	11	15	103
80	5	6	7	8	6	7	9	7	55
70	2	1	1	5	4	3	2	3	21
60	2		1	1	1	2		1	8
50									0
40	1								1
Total	32	38	32	39	25	30	25	36	257
Median	93	95	97	94	91	91	91	93	93
Average	91	95	95	95	90	91	90	94	93

[2] Cf. E. A. Lincoln, Time Saving in the Stanford-Binet Test, *ibid.*, 13, 1922, 94 97.

The same distribution of the entire school is presented graphically in Fig. 1.

From this we see the median score of 93 and average score of 93, with maximum score of 130 and minimum of 43. These scores may seem rather low. The explanation, however, of this fact may come from our study of their relations to nationality, environment, and other factors.

The teachers made their own estimates of the intelligence of the children in their rooms. The coefficient of correlation between these estimates and the intelligence quotients is not low, being .55. The following Table shows these coefficients of correlation according to the rooms.

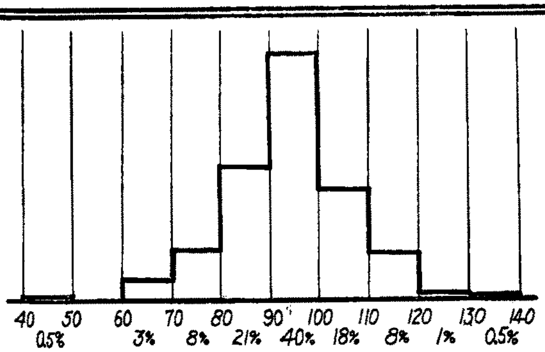

Fig. 1—Distribution of I.Q.s of 257 unselected Children, 5-16 Years of Age

TABLE V

Coefficient of Correlation Between I.Q. and Teachers' Estimate of Intelligence

Room

	1	2	3	4	5	6	7	8
C. C.	.75	.38	.22	.58	.64	.72	.51	.62

The correlation between I.Q. and the school success of the children is, on the average, .48. The same according to the rooms may be seen in the following Table.

TABLE VI

Coefficient of Correlation Between I.Q. and School Success

Room

	1	2	3	4	5	6	7	8
C. C.	.76	.42	.24	.58	.57	.65	−.03	.64

In this connection it is interesting to note the distribution of the children in the grades, with regard to their mental ages. Figg. 2 and 3 show this location of mental ages in the grades, Fig. 2 showing only a more detailed classification than Fig. 3.

MENTAL AGE	GRADE I			II			III			IV			V			VI			VII			VIII		
	C	B	A	C	B	A	C	B	A	C	B	A	C	B	A	C	B	A	C	B	A	C	B	A
3-10 to 4-1		1																						
4-2 to 4-5																								
4-6 to 4-9		2																						
4-10 to 5-1		1	1																					
5-2 to 5-5		2																						
5-6 to 5-9		1																						
5-10 to 6-1		1		1																				
6-2 to 6-5		5		1																				
6-6 to 6-9		4	3			1																		
6-10 to 7-1	1		1	1	4	1																		
7-2 to 7-5			4	4	2	1																		
7-6 to 7-9		1	3	3	1	2		1																
7-10 to 8-1				1	3	3	2	2																
8-2 to 8-5			1	1	1	3	2	2																
8-6 to 8-9					2	1	1		1			1												
8-10 to 9-1							2		4	1	1													
9-2 to 9-5							2	3	3	3	1	5	1	1	1	1								
9-6 to 9-9					1			1	2	1		4		1				2						
9-10 to 10-1									2	1	5	4		5				1				1		
10-2 to 10-5									1		2	3	1	1	6	1	3		1		1			
10-6 to 10-9									1								2	1	1	1	1			
10-10 to 11-1											1	2			1		2	2	4	2				
11-2 to 11-5										1	1	2		1	4	2		2		1				
11-6 to 11-9															1			5			1	2		
11-10 to 12-1																1			1			1	2	1
12-2 to 12-5															1			1	2	1	1	1	1	
12-6 to 12-9																				1	2	2	1	1
12-10 to 13-1																1		2	1			1		2
13-2 to 13-5																		1			1		1	1
13-6 to 13-9																						1	1	3
13-10 to 14-1																					2		1	1
14-2 to 14-5																						1		1
14-6 to 14-9																						1	1	4
14-10 to 15-1																								
15-2 to 15-5																								1
15-6 to 15-9																							1	
15-10 to 16-1																								
16-2 to 16-5																								
16-6 to 16-9																								1
16-10 to 17-1																								

FIG. 2. Age-Grade Location of 257 Unselected Children: Washington School

MENTAL AGE	GRADE I	II	III	IV	V	VI	VII	VIII
3 -6 to 4 -5	1							
4 -6 to 5 -5	6							
5 -6 to 6 -5	7	2						
6 -6 to 7 -5	13	14						
7 -6 to 8 -5	5	18	9					
8 -6 to 9 -5		3	16	12	3	1		
9 -6 to 10-5		1	6	20	14	7	2	1
10-6 to 11-5			1	7	6	11	10	
11-6 to 12-5					2	7	6	8
12-6 to 13-5						4	5	9
13-6 to 14-5							2	9
14-6 to 15-5								7
15-6 to 16-5								1
16-6 to 17-5								1

FIG. 3. Age-Grade Location of 257 Unselected Children: Washington School

III. Intelligence and Nationality

The problems of the nationality and the environment of a child are perhaps those most absorbing for a social psychologist in connection with his consideration of the problems of intelligence. We now take up the first of these, to see what light the Study may throw upon the relation of intelligence and nationality.

In Table VII we give the norms of intelligence in terms of the median and average scores of the intelligence quotients which the children scored and according to the nationalities which the parents of the respective children represent.

TABLE VII

Norms of Intelligence of Nationalities

Name of Country	*Number of Cases*	*Median I.Q.*	*Average I.Q.*
United States (Whites)	78	98	97
United States (Colored)	6	88	86
England	19	93	93
Scotland	3	91	97
Canada	2	84	84
Ireland	1	96	
Total, English Speaking	109	96	95
Sweden	63	95	94
Poland	27	83	86
Germany	25	96	93
Norway	11	91	89
Greece	6	71	73
Bohemia	4	90	91
Russia	3	100	97
Russian-Jew	3	82	87
Italy	2	81	81
Denmark	2	107	107
Holland	1	105	
Mexico	1	95	
Total, Non-English Speaking	148	92	91
Grand Total	257	93	93

The results can not, needless to say, be taken seriously for those nations which are represented but by a few cases. Taking those, however, which are represented rather well, we see the distribution of I.Q. with regard to the comparative study of the English speaking class and Non-English speaking class in the following Table.

TABLE VIII

Distribution of I.Q. of Nationalities

I.Q.	*U.S. (Whites)*	*English speaking*	*Sweden*	*Poland*	*Germany*	*Non-Eng. speaking*	*Total*
130	1	1					1
120	1	1		1		1	2
110	9	11	7		2	9	20
100	17	20	13	4	4	26	46
90	32	47	26	4	11	56	103
80	12	21	10	11	5	34	55
70	4	6	5	6	2	15	21
60	2	2	2	1	1	6	8
50							
40						1	1
Total	78	109	63	27	25	148	257

The same distributions are graphically presented in Figg. 4 to 9. In Fig. 10 we give a graphic presentation of the comparison of the distributions of I.Q.s of the total cases between the class of the English Speaking and that of the Non-English Speaking. From these we see that high I.Q.s are scored by more children among the English Speaking than those among the Non-English Speaking. The reason for this fact may be attributed to the hereditary factors. We may get even more light upon this problem from the study of the environment of the children, to which we now turn our attention.

IV. Intelligence and Environment

The environmental survey in the present Study was made possible by the Whittier Scale for Grading Home Conditions and the same Scale for Grading Neighborhoods.[3] Ten items of 5 points each, making a total number of 50 points for the maximum scores, is the result from the combination of both scales for the grading of environment, and proves very convenient. Although both of these Whittier Scales, taken separately, are perhaps nearly perfect in the grading of their respective fields, yet the combined form can hardly be said to be perfect. It neglects to take into account some of the most important elements in a child's home environment, and its equal emphasis on both the home and the neighborhood makes the scale unduly over-balanced on the side of the neighborhood conditions. A child is usually moulded by direct contact with parents, relatives, and other intimate associates at home, more strongly than by the rather remote neighborhood influences, which undoubtedly play a more important part as the child grows older. In this connection it is especially important to know how the child lives at home, in what way his habits are formed, and how his time at home is spent; for it is on such factors of the home life that the development of life and character and, consequently, intellectual manifestations, are conditioned. The Whittier Scale for the Grading of Homes lacks a serious consideration of these matters; it takes two items out of the total items of five for the parental influence, Parental Conditions for Item 4 and Parental Supervision for Item 5, but none for the influences from relatives, friends and other associates nor for the mode of life and manner of the child's forming habit and character. The last mentioned factor may be intimately connected with the supervision of the parents, yet it is important enough to claim separate consideration. In saying this, the writer does not consider two items on the parental influences as over-balanced nor does he consider three other items more directly connected with the material side of the home too preponderant; these items are essential, and can never be omitted. Then, to make a combined form for grading environment consisting of 10 items and, at the same time, to include these important additional items, we turn to the consideration of the items for grading the neighborhood conditions. Here we have seen that this part, if put in the combined form on equal standing with that of homes, will result in an unduly over-balanced scale on the side of the neighborhood conditions. Among 5 items listed in the original Whittier Scale we may, then, safely group together Item 2, Recreational Facilities and Item 3, Institutions and Establishments into one item; and Item 4, Social Status of Residents and Item 5, Average Quality of Homes also into one group, thus leaving room for two of the most important items, the child's associates and his home life itself, which have been neglected.

Then the scale for the grading of the environmental conditions of the child will ultimately take the following form. *Item I.* Necessities, corresponding to Item I of the Whittier Scale for Grading Home Conditions. *Item II.* Neatness of the Home, corresponding to Item II of the same.

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[3] W. W. Clark and J. H. Williams, A Guide to the Grading of Homes, *Publications of Whittier State School, Department of Research*, Bulletin No. 7. A Guide to the Grading of Neighborhoods, *ibid.*, Bulletin No. 8.

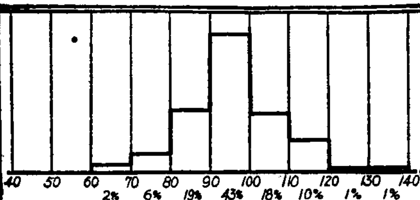

Fig. 4 ~ 109 unselected Children of English Speaking Parents ~

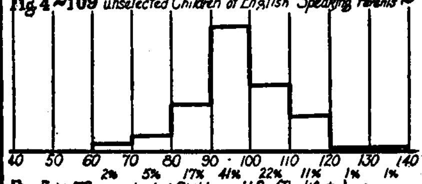

Fig. 5 ~ 78 unselected Children, U.S. (The Whites) ~

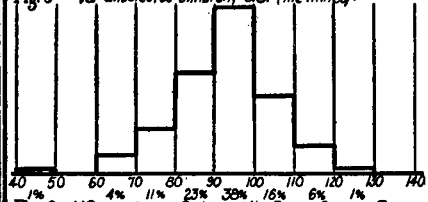

Fig. 6 ~ 148 unselected Children of Non-English Speaking Parents ~

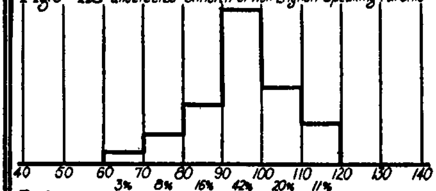

Fig. 7 ~ 63 unselected Children, Sweden ~

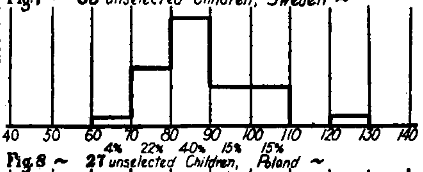

Fig. 8 ~ 27 unselected Children, Poland ~

Fig. 9 ~ 25 unselected Children, Germany ~

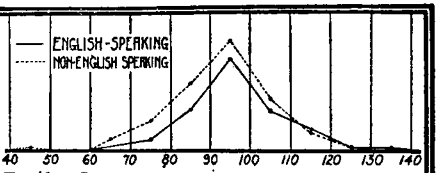

Fig. 10 ~ Comparison of the Distribution of the I.Q.s of English- and Non-English-Speaking Classes (257 Cases) ~

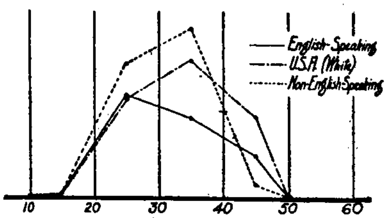

Fig. 11 ~ Comparison of the Distribution of Environment Scores of English-Speaking, United States (the Whites), and Non-English-Speaking (200 Children) ~

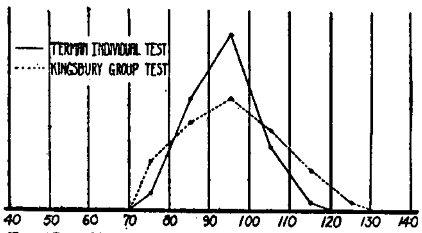

Fig. 12 ~ Distributions of Kingsbury and Terman I.Q.s. ~

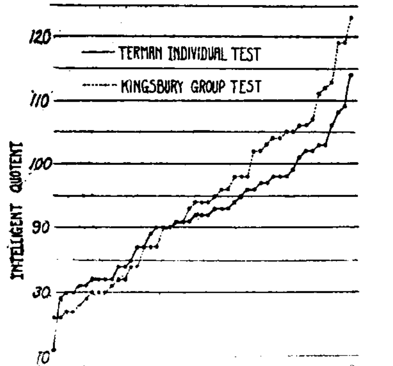

Fig. 13 ~ Comparison of the Distributions (47 Cases)

Item III. Size of the Home, corresponding to Item III of the same. *Item IV*. Parental Conditions, corresponding to Item IV of the same. *Item V*. Parental Supervision, corresponding to Item V of the same. *Item VI*. Relatives, Friends, and Associates; a new item. *Item VII*. Child's Home Life; a new item. *Item VIII*. Neatness, Sanitation, Improvements of the Neighborhood, corresponding to Item I of the Whittier Scale for Grading Neighborhood Conditions. *Item IX*. Recreational Facilities, Institutions, and Establishments, corresponding to Items II and III of the same. *Item X*. Social status of the Residents and Average Quality of Homes, corresponding to Items IV and V of the same.

The maximum score of each of these items is 5 points, making a total of 50 points as the highest score. Of course there will sometimes be an exceptionally higher score; for, as the Guide to the Use of the Whittier Scale for Grading Home Conditions admits, "if an item is found which is entitled to a higher score than 5 points by comparison with the standard samples there is no reason why it should not be accorded 6 points or even 7 points, if in the judgment of the observer it is sufficiently superior to merit such marking."[4]

Eight items of the present scale for grading the environmental conditions, thus presented, are sufficiently explained in the Guides to the Grading of Homes and Neighborhood, referred to. We now, therefore, try to explain briefly the remaining two items which are in fact new, and to propose a tentative scale for them.

"Associates," explains J. H. Williams, "are persons with whom one comes in contact at home, in school, at work, on the street, in church, etc., and those with whom leisure time is spent. The associations may have arisen through accidental circumstances, or may be due to desired cultivation. It is generally conceded that character and development are closely related to associates."[5] A careful observation should be made of the relatives, friends, and other associates and members of the family, with whom the child comes into contact. The following 5 points may be mentioned as claiming special consideration in this connection: (1) character of the associates; (2) relations: activities of the individual in association with these persons; (3) attitude taken and interest shown by these persons; (4) examples given by these persons; (5) manner of spending leisure time.

We will next offer our tentative scale for grading this item of Associates, based on our actual study of 200 cases.

Samples Grading 5 points: (*a*) Three uncles and two aunts live near-by. They have average success as various kinds of skilled laborers, or as the wives of such. They keep their homes separately and seem enjoying a fine home life. Their relation to the *S*'s family is very cordial and intimate. They love the *S* and try earnestly to help in training him and building up his character. There are about 5 intimate friends. They are of good homes and possess fine character. They spend their leisure time together in playing, hiking, etc. Some of them are seen in the library in company with the *S*. So far as is known, there is no associate of questionable character. (*b*) Grandfather, a retired business man, still active, has a keen intellect, perspective and a well balanced character. He cares much for the welfare of the child; is accustomed to tell stories to *S* in the evening. The *S* loves him ardently. Several uncles and aunts live in near-by cities. They seem to enjoy fine home lives and are on good terms with the *S*'s family. Other associates of the *S* are made up mostly of his school-mates.

Samples Grading 4 points: (*a*) Grandmother, a gentle lady of normal intelligence, is very fond of the child. Several uncles live near-by, and are on good terms with the *S*'s family. They are of apparently normal intelligence and seem to have normal success in various occupations.

[4] *Op. cit.*, 11.

[5] Individual Case History Outline, *Jour. of Delinquency*, 5, 1920, 77 f.

(*b*) Grandfather and grandmother, apparently of rather keen intelligence and of good character. There is one aunt living in the family. She is a normally intelligent person, working in a down-town store. They are in good harmony. The *S*'s friends and other associates are schoolmates. None of them is known to have a bad reputation.

Samples Grading 3 points: (*a*) Three uncles live near-by. They are too busily engaged in their occupations to come to see the *S* often, though the *S* goes to see them. They are of apparently normal intelligence. School-mates make up the *S*'s other associates. (*b*) Grandfather and grandmother, rather ill-natured, but apparently on good terms with the rest of the family. One uncle lives near-by. He is a successful small store-keeper of a fine character. The *S* sees him often. Other relatives live in places rather remote, and do not come intimately into the life of the *S*. (*c*) An aunt, a divorcee, lives with the *S*'s family. She is rather vain and frequents theatres, but loves the *S* well. She is apparently on normal terms with the rest of the family. The friends of the *S* are recruited from among school-mates.

Samples Grading 2 points: (*a*) One uncle, a gambler and cheat, is living in the *S*'s home. Several relatives live in places more or less distant, and their relation with the family of the *S* can not be said to be intimate. (*b*) Although there are several uncles and aunts of the *S* living in the same town and adjoining towns, they are not on intimate terms with the family of the *S*; and the *S* does not even remember them. The other associates of the *S* are comprised of children living near-by. They organize a sort of gang, and are often said to be the authors of mischievous stunts.

By Home Life we mean the way and manner of the child's daily home life in which the character and future life of the child are moulded. Together with the school life it claims the foreground in influencing the child's development in various lines. We may especially note the following 5 points in this connection: (1) orderliness and punctuality; (2) cleanliness, fostering the habit of a clean thinking and living; (3) industriousness; (4) interest in studying and reading; (5) indulgence in amusements, the kind of amusements indulged in.

Samples Grading 5 points: (*a*) (13-year boy) Gets up at 7 a.m., goes to bed at 9 p.m. After school in the afternoon, runs errands or reads books. In the evening, plays ball and reads books. On Saturday, work. On Sunday, attends Sunday school. Home life is well regulated and kept clean. Sees moving pictures about once a month; likes those of cowboys. Likes school entertainments; loves reading books. The family keeps a library of about 300 books. Likes and plays indoor baseball, football and golf. Also plays violin. (*b*) (11-year girl) Gets up at 6:30 a.m., goes to bed at 8 p.m. The family speaks English. After school in the afternoon, plays and practises music lessons. In the evening, reads books and magazines. On Saturday, takes music lessons, plays, and "helps mother." On Sunday, goes to Sunday school in the morning, and plays or goes out with all the family in the afternoon. Sees moving pictures about once every other week; likes comic pictures, "not sad ones." Likes and plays baseball, basketball and skating. (*c*) (7-year boy) Gets up at 7 a.m., goes to bed at 8:30 p.m. Speaks English at home. After school in the afternoon, plays. In the evening, plays games and reads. On Saturday, plays and "helps Daddy and Grandpa." On Sunday, goes to Sunday school in the morning and plays in the afternoon. Sees moving pictures probably once or twice a year. Likes school entertainments. Also likes to play baseball and marbles, and to listen to victrola. Is well provided with educational as well as recreational materials. (*d*) (9-year boy) Gets up at 7 a.m., goes to bed at 8 p.m. Speaks English at home. After school in the afternoon, takes music lessons, does errands and plays. In the evening, reads books, and goes to "where mother tells me to." On Satur-

day, takes music lessons, works and plays and reads. On Sunday, goes to Sunday school and remains in the church during the morning service, and reads books and magazines or plays in the afternoon.

Samples Grading 4 points: (*a*) (14-year boy) Gets up at 7 a.m., goes to bed at 9 p.m. Speaks English at home. After school in the afternoon, plays ball. In the evening, plays and reads. On Saturday, works; and on Sunday, goes to Sunday school and plays. Sees moving pictures once a week. Likes music. Likes and plays baseball, basketball, etc. (*b*) (12-year girl) Gets up at 6:30 a.m., goes to bed at 8:30 p.m. After school in the afternoon, plays and practises music lessons; in the evening, plays. On Saturday, takes music lessons and plays. On Sunday, goes to Sunday school. Sees moving pictures about once a week. Likes minstrels and vaudeville. Plays basketball, skating, etc. (*c*) (7-year boy) Gets up at 7:30 a.m., goes to bed at 9 p.m. Speaks English at home. After school in the afternoon and in the evening, plays. On Saturday, plays. On Sunday, goes to church. Does not see moving pictures often. Likes marbles and skating.

Samples Grading 3 points: (*a*) (13-year boy) Gets up at 7 a.m., goes to bed at 9 p.m. Speaks English at home. After school in the afternoon, plays ball or does chores. In the evening, reads books or goes to theatre. On Saturday, plays. On Sunday, goes to church, and plays. Sees moving pictures about twice a week; likes pictures of "love and adventure and 'Diamond Dick.'" Hardly ever attends other entertainments. (*b*) (10-year girl) Gets up at 7 a.m., goes to bed at 8:30 p.m. Parents speak Polish at home. The home life is not well regulated, but runs about as follows: after school in the afternoon, plays, and in the evening, washes dishes; on Saturday, works; and on Sunday, plays or goes to see moving pictures. Sees moving pictures about once or twice a week. Hardly ever attends other entertainments. Likes kite.

Samples Grading 2 points: (*a*) (14-year boy) Parents speak Swedish at home. The home life is not regulated, but runs about as follows: gets up at 7 a.m., and goes to bed at 9 p.m. After school in the afternoon, plays or does chores. In the evening, plays. On Saturday, does chores, plays and goes to theatre. On Sunday, plays and goes to theatre. Goes to see moving pictures very often; likes love, adventure and comic pictures. (*b*) (13-year girl) Parents speak Swedish at home. The home is not regulated nor clean. The course of the home life runs about as follows. Gets up at 7:30 a.m., goes to bed at 8 p.m. After school in the afternoon, works, and in the evening, takes rest. On Saturday, works. On Sunday, goes out. Attends theatres and goes out frequently. Likes pictures of love.

Samples Grading 1 point: (*a*) (13-year boy) Non-English speaking parents; home life is not at all regulated, and the habit of the *S* is untidy and his manners and appearance are dirty. The home life runs about as follows, though not at all definite. Gets up at 7:30 a.m., goes to bed at 9:30 p.m. After school in the afternoon, plays. In the evening, plays or attends theatre. On Saturday and on Sunday, plays and wastes time. See moving pictures very often. (*b*) (9-year boy) Non-English speaking parents. Untidy habits. The *S* is, though young, obliged to help the parents by working; but the home life as such is not at all regulated nor clean, and only results in forming undesirable habits. Attends theatre very often with older associates.

We could study and grade 200 cases with this scale of the grading of environmental conditions. Now let us see the results thus obtained. The following Table shows the distribution of the environmental scores and their median and average scores, obtained through the use of the Grading Scale referred to.

TABLE IX

Distribution of Environment Scores, Median and Average

Score	*Number*
Above 40	18
30 to 39	94
20 to 29	87
10 to 19	1
Total	200
Median score	30
Average score	31

The coefficient of correlation between intelligence quotients and environment scores, thus obtained respectively by the Terman Revised Binet Test and the Scale for Grading Environment referred to, is not low, being .53.

The relation between intelligence and occupation may be studied in the following Table.

TABLE X

Relation of I.Q. and Occupations

Kind of Occupation	*Number of Cases*	*Median*	*Average*
I Business, office work			
Office work	19	99	101
Business	5	98	100
Salesman	5	106	106
Total	29	101	101
II Transportation work			
Driver	17	93	93
Milkman	9	99	100
Teamster	7	95	94
Chauffeur	4	103	100
Mail carrier	3	102	111
Motorman	2	97	97
Conductor	1	102	
Total	43	98	97
III Small Store Keeper and Helper			
Laundry	13	86	86
Tailor	9	91	90
Grocery	5	104	104
Butcher	4	95	101
Bakery	4	86	89
Photography	1	117	
Greenhouse	1	102	
Total	37	92	93
IV Skilled Labor			
Mechanic	20	94	94
Carpenter	15	94	94
Mason	11	90	91
Painter	7	96	97
Blacksmith	3	103	100
Plumber	2	79	79
Lather	1	76	
Total	59	93	93
V Unskilled Labor	52	89	88

VI	Miscellaneous			
	Janitor	17	90	88
	Peddler	4	64	61
	Gardener	3	82	87
	Total	24	85	84
VII	Widow			
	Housework	9	93	94
	Outside work (cook, maid)	4	100	100
	Total	13	98	96

From these figures we see that a decidedly high I.Q. is gained by the children whose parents are engaged in business or office work, and that a low I.Q. is for the children of those engaged in unskilled labor or peddling work. There are about 5 points of difference in I.Q. between skilled labor and unskilled labor, favoring the former.

V. Nationality and Environment

We have seen from the results thus far presented that the children of the English Speaking class stand higher in I.Q. than those of the Non-English Speaking class; especially, the children of the White United States citizens earned decidedly higher scores than any of other nationalities. Further, we have noticed that the higher I.Q. is attributed to the children whose parents are engaged in so-called brain or skilled work rather than to those whose parents are of other occupations. What, then, will be the relation of these three factors: Intelligence, Nationality and Environment? To get some light in answering this question we now turn our attention to the relation of nationality and environment.

The first point of which we have to take note in this connection is the relation of nationality and occupation. The Table presents this relation by showing the distribution of the number of persons with regard to these two points.

TABLE XI

Distribution of Nationality in Occupation

	Occupation	*U. S. White*	*U. S. Colored*	*Others*	*English Speaking*	*Non-English Speaking*	*Total*
I	Business, office work						
	Office work	14	..	3	17	2	19
	Business	4	..	..	4	1	5
	Salesman	5	..	..	5	..	5
	Total	23	0	3	26	3	29
II	Transportation						
	Driver	6	..	2	8	9	17
	Milkman	2	..	3	5	4	9
	Teamster	1	..	..	1	6	7
	Chauffeur	1	..	..	1	3	4
	Mail carrier	2	..	..	2	1	3
	Motorman	2	..	..	2	..	2
	Conductor	1	..	..	1	..	1
	Total	15	0	5	20	23	43

III	Small Store						
	Laundry	..	..	..	..	13	13
	Tailor	..	..	..	..	9	9
	Grocery	4	..	..	4	1	5
	Butcher	1	..	..	1	3	4
	Bakery	1	..	..	1	3	4
	Photography	1	..	..	1	..	1
	Greenhouse	..	..	..	..	1	1
	Total	7	0	0	7	30	37
IV	Skilled labor						
	Mechanic	6	..	6	12	8	20
	Carpenter	7	..	4	11	4	15
	Mason	1	..	..	1	10	11
	Painter	..	..	3	3	4	7
	Blacksmith	..	..	..	..	3	3
	Plumber	1	1	..	2	..	2
	Lather	1	..	..	1	..	1
	Total	16	1	13	30	29	59
V	Unskilled labor	11	3	4	18	34	52
VI	Miscellaneous						
	Janitor	3	2	..	5	12	[illegible]
	Peddler	..	..	..	..	4	4
	Gardener	1	..	..	1	2	3
	Total	4	2	0	6	18	24
VII	Widow						
	Housework	2	..	..	2	7	9
	Outside work	..	..	..	..	4	4
	Total	2	0	0	2	11	13

From these figures we see that the more favored occupations are taken by the English Speaking class, especially by the White citizens of the United States; and that, on the other hand, the Non-English Speaking class is mostly left for the less favored occupation. Whether this is because of the inherent difference of intelligence or the environmental difference of opportunity we can not so readily tell. We may here give one more Table, showing the distribution of the environment scores in the main different groups of nationalities.

TABLE XII

Distribution of Environment Scores

Nationality	*Number of Cases*	*Median*	*Average*
United States	45	33	33
Other English Speaking	26	29	29
Total English Speaking	71	32	32
Total Non-English Speaking	129	30	30
Grand Total	200	30	31

The same relation from a somewhat different angle may be seen in Fig. 11, which presents the comparative study of the distributions of environment scores of 200 unselected children between the English Speaking and Non-English Speaking classes and the White citizens of the United States. We see that the United States White citizens enjoy the most favored environment, and that the Non-English Speaking class is less favored on this point.

VI. *Conclusion*

In concluding the present Study, we may especially note the following points.

(1) Both the median and the average scores of the intelligence quotients of the total school children (257) is 93.

(2) The English Speaking class scores higher than the Non-English Speaking class, with a difference of 4 points in both the median and average scores.

(3) Those whose parents are engaged in so-called brain and skilled work score higher than those whose parents are of other occupations. To put this relation in terms of the average I.Q., we say that there is a difference of 13 points between business and office work and unskilled labor, and that there is also a difference of 5 points between the class of skilled labor and that of unskilled labor.

(4) The English Speaking people are engaged more in the occupations of "brain and skilled" work and less in unskilled labor than the Non-English Speaking class.

(5) The English Speaking class gets higher environment scores than the Non-English Speaking class, the difference being 2 points.

(6) The coefficient of correlation between the intelligence quotients and the environment scores is not low, being .53.

(7) From these facts we may conclude that there exists a rather close relationship between intelligence, nationality and environment. High I.Q., high environment score, and brain work go with the people of the English Speaking class, and the reverse is true with those of the Non-English Speaking class.

Appendix I: Kingsbury Test and Terman Revised Binet Test in our Study

The Kingsbury Group Intelligence Test was used by Mr. Walker, the Assistant Superintendent, in testing the intelligence of the children of the Washington School in May and Dec., 1920. Some of the results are found still useful in checking up the present results which we get by the use of the Terman Revised Binet-Simon Individual Test. Here we take up the correlation of the Kingsbury Test and the Binet Test. As but a few of the results obtained in May, 1920, are still applicable, we take here only those of the Dec. test.

The following Table shows the distribution of I.Q.s, median and average scores of 47 children, tested by the Kingsbury test.

TABLE XIII

Distribution of I.Q.

I.Q.	*Number*
120	1
110	5
100	10
90	14
80	11
70	6
Total	47
Median	94
Average	94

The coefficient of correlation between the I.Q. of the above cases tested by the Kingsbury test and those obtained by the Binet test is .47.

The same relation may be studied in Figg. 12 and 13, which present graphically the comparison of the distribution of intelligence quotients of 47 children between those obtained by the Kingsbury test and those of the Binet test, Fig. 13 being the study of individual case comparison.

In this connection we may look at the different scores according to the main divisions of nationality although the study made by this Kingsbury test does not include a sufficient number of cases to give any authoritative result.

TABLE XIV

Mean and Average Scores of 47 Children Made by the Kingsbury Test, According to the Main Divisions of Nationality

Division	*Number of Cases*	*Median*	*Average*
English Speaking	15	94	96
Non-English Speaking	32	95	94
Total	47	94	94

Appendix II: The Problem of Sex

Of the total number of 257 children of our study, 145 are boys and 112 are girls. The following Table shows the distribution of intelligence quotients among the two sexes.

TABLE XV

Distribution of I.Q.s Between Sexes

		Number	
I.Q.	*Boys*	*Girls*	*Total*
130		1	1
120	1	1	2
110	11	9	20
100	26	20	46
90	62	41	103
80	25	30	55
70	13	8	21
60	6	2	8
50			
40	1		1
Total	145	112	257
Median I.Q.	94	93	93
Average I.Q.	92	93	93

From these figures we do not see any significant difference to be mentioned between the two sexes.

DISCUSSION

C. SPEARMAN, *The Nature of 'Intelligence' and the Principles of Cognition*, 1923.

The author of this volume is the well-known psychologist of the University of London whose work in the field of mental measurements has led him to the interpretation of intelligence in terms of a general and purely quantitative factor to which specific and variable factors are added. The rationale of testing is accordingly based upon the effect of pooling specific tests, thereby bringing out the general factor which underlies them.

In this volume Spearman attempts to lay the foundations of a scientific psychology which shall determine the principles upon which intelligence operates and thus discover the nature of intelligence itself.

Part I, embracing two chapters, deals with the historical and critical aspects of the problem, attention being chiefly given to the views of Binet and those who followed him in perfecting the technique of testing. After first reducing intelligence to *judgment*, Binet proceeded to add tests of memory, language, and sensory intelligence, and finally modified his original view by admitting four criteria of intelligence—*comprehension*, *invention*, *direction*, and *censorship*—all of which are pooled in the tests which now bear his name.

Recent definitions of intelligence in terms of the tests are even more wanting in agreement than Binet's own divergent views. This fact was instanced in the symposium on the subject printed in the *Journal of Educational Psychology*, 7, 1921, where the definitions of various authorities are vaguely stated in such terms as "adaptability to new situations," "acting effectively," making "good responses," etc. Spearman also refers to Bobertag's view that the technique of testing does not require any definite answer to the question, what it is we are testing; an opinion with which Spearman finds himself in complete disagreement. The book at hand may therefore be regarded as an attempt to lay the foundation for a definition of intelligence in terms of a psychology of cognition.

The defect of modern psychology, according to Spearman, is that its experimentation has not led to significant generalizations; such generalizations as it has employed having remained empty for want of experimental substantiation. The only general law which can be evoked by psychology is the law of association, and this has suffered many diverse interpretations. Spearman proposes to remedy this defect by "clear thinking, ultimate laws, and an appropriate experimental procedure." Beginning with sensory experience, which in the original conscious process is merely "lived" or "undergone," the ordinary percept includes far more than sensory stimulation as a mere state of sentience. Even sentience is attained only after a transition which, in the case of vision, involves (1) the material thing; (2) the ether waves; (3) the chemical processes of the eye; (4) a current in the perceptual nerves; (5) the chemical processes of the sensorium; and (6) the passage from matter to mind—all occurring before sentience itself can be "lived" or "undergone." Furthermore, Spearman remarks that stages (3), (4) and (5) are each qualitatively independent of the stages which precede them, and hence "the eventual effect of the material thing on consciousness is barred three times over from any likeness to that thing itself."

Granted sentience, however, what are the means whereby we pass on to the percept? This question is answered by the statement that "any lived experience tends to evoke immediately a knowing of its characters and experiences." Here we have Spearman's first principle of the *apprehension of experience*. The "form" which experience thus takes implies "some sort or other of what may be called 'material,' " but whereas the

"form" is only the aggregate of those attributes of the occurrence which remain in all instances similar, the "material" is composed of such attributes as may vary from one instance to another.

The analysis of "material" includes: (1) sentience with its four "characters" of quality, intensity, spatiality, and temporality; (2) affection; (3) cognition; (4) conation; and (5) the direct experience of the ego; all of which are experienced by direct "evocation of knowing, and so on." "Form" in its turn involves the characteristics of *propositional import*, *belief* and *insight*, *coherence*, and *unit-processes*.

These, then, are the characters of experience, material and formal, which are immediately evoked, and which, when taken together, constitute the consciousness of experiencer, experiencing, and experienced.

Two comments are here suggested. (1) Cognition and conation seem out of place as contents of consciousness, and without a reference to content the precise significance of "materials" is equivocal. (2) The formal aspects of experience in terms of the four categories named are logically unconvincing, because "coherence" and "unit-processes" are so like, whereas "propositional import" and "belief and insight" are so different.

Passing on, however, to Spearman's second principle, the *eduction of relations*, we find this principle stated in the following terms: "The mentally presenting of any two or more characters (simple or complex) tends to evoke immediately a knowing of relation between them." As a corollary it is stated that relations once apprehended may thereafter act as fundaments for new relations. Relations as such possess "character" just as the original data of apprehension possess "character." Relations are then divided into two classes: (1) *real* relations, which include *attribution*, *identity*, *time*, *space*, *cause*, *objectivity*, and *constitution;* and (2) *ideal* relations, such as *likeness*, *evidence*, *conjunction*, and *intermixture*.

It would appear that, in addition to our powers of apprehension, we possess a peculiar mental power of *eduction* which furnishes the mind, not only with the relations of one thing and another, but also with new "materials" and new "forms" of apprehension. Eduction is thus opposed to apprehension as a distinct type of awareness.

In a paper entitled "Mental Activity and Conscious Content," published several years ago,[1] and also in *An Introduction to General Psychology*[2], a similar interpretation of cognition has been advanced by the reviewer. The distinction I then drew between the immediate eduction of relations as they subsist between contents or "characters," and the abstraction of relations as new "materials," called *notions*, does not appear to have been essentially different from Spearman's provision for new fundaments, supplied by relations once apprehended.[3]

The classification of relations then proposed by me followed a suggestion of E. Dürr in his revision of Ebbinghaus' *Psychology;*[4] namely, two main classes—one of *particularity*, in which the object or character is apprehended in a dichotomous relation with the rest of experience, the other embracing relations of *likeness* and *difference*, *equality* and *identity*, in which at least two objects or characters are related to one another.

The scheme was frankly logical, though I now believe it possible to indicate psychological data from which these two fundamental principles of logical classification can be derived; thus, *particularization* is given in the apprehension of a "figure" upon the "ground" of experience,[5] whereas relations of likeness and difference are phenomenally determined by the characteristic dimensions or aspects of the figure apprehended. Particularization is therefore a more primitive type of *eduction*—to employ

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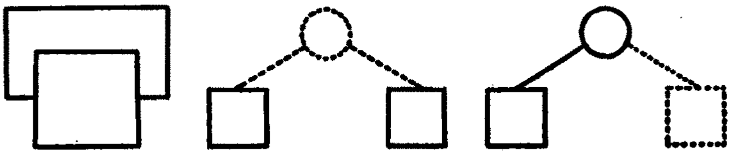
[1] *Philosophical Essays in honor of James Edwin Creighton*, 1917, 290 ff.
[2] 1914. [3] Cf., especially, *General Psychology*, 93 f. [4] 2, 1911, 279.
[5] Cf. E. Rubin, *Visuell wahrgenommene Figuren*, 1921.

Spearman's term—than is the comparison of characters in pairs, since the latter becomes possible only when the experiencer is able, as it were, to hold two things in mind at once.

While I readily admit that my own attempt to interpret cognition rested more heavily upon the logic of classification than upon the psychology of cognitive experience, Spearman's principles do not appear to be any less free of contamination. Consequently his list of relations suggests neither rigorous logic nor psychological observation. Instead, the list leaves us without knowledge as to the method of selection, so that it might conceivably be more or less extended than it is. Furthermore, logic has its virtues, despite the danger of confusing it with psychology, the chief among them being the classification of concepts so as to define each term distinctly from every other term. Logic also demands that, taken together, these concepts shall exhaust the possibilities within the limits which the class has set. One can not be sure that Spearman's *real* and *ideal* relations satisfy these conditions, whereas *contradictories, likenesses,* and *differences* do.

Passing on to Spearman's third principle—*the eduction of correlates*—we note the statement that "the presenting of any character together with any relation tends to evoke immediately a knowing of the correlative character." Here we have the final stage of eduction. Viewed together, the three principles are graphically represented by the following figures:

I II III



Though Spearman makes surprisingly little use of genetic modes of interpretation, it is fairly obvious that his third principle follows his second, since a known relation is necessary to the evocation of an unknown correlate. In other words, the relation now serves as a category under which the correlative of the given term makes its appearance.

Other figures might be devised which would bring these facts of logic more truly in touch with the psychological data of observation. Regarding a datum of observation genetically—both with respect to its origin in the earliest stages of development, and also (with certain modifications) as it appears for the first time in any mind—its initial stage might be represented as the appearance of a vague figure (A) upon the ground of consciousness (not-A): not-A A not-A. This ill-defined object of experience is the "lived" or "undergone" with a degree of articulateness sufficient, at least, to provide an accent lacking in the less articulate ground from which it emerges. Furthermore, this emerging datum of experience is not merely the counterpart of the stimulus of response, for it also embraces the response itself in so far as this is "lived" or "undergone." At its lowest level, apprehension must therefore be something more than a static presentation in sensory terms, since it is also a dynamic process with a temporal course. The analysis of such a vaguely defined phenomenon of consciousness can obviously be undertaken only at a much higher level of cognition than is implied at this low stage of development. But can we not assume for it the qualities of urgency which as adults we feel in moments of vague desire in the course taken to satisfy this desire? The direction of behavior,

with its all-or-none characteristic of persistency until the desired end is achieved, enables us to underwrite such an experience in psychological terms of sentience and affection. It also enables us to apply the logic of dichotomy to the A of our scheme which emerges over against the level of inarticulate sentience and affection that we have called the ground of consciousness, or not-A.

The next stage in this procedure would be the definition of A's contour—a definition which is conditioned both by the inherent nature of A and likewise by the nature of the receiving and behaving organism. But object and organism must not be too sharply differentiated, because at this level we are dealing with a total response; consequently the apprehension of A is part and parcel of the state of receptivity and response. It is important, however, that the varying susceptibilities of the objective and subjective aspects of apprehension should be noted; for although a clearly defined circular object in the field of view might be overlooked, such an object would nevertheless be more readily apprehended than a vaguely circular glow in the darkness.

The definition of contours is conditioned both by an appropriate objective form and also by an appropriate subjective readiness: and the likeness of these two is so intimate that one is led to question Spearman's previously noted statement that the chemical process of the eye, the current in the perceptual nerves, and the chemical processes of the sensorium constitute three stages, each qualitatively independent of the material thing and of its medium of transmission to the eye—a statement which leads him to conclude that "the effect of the material thing on consciousness is barred three times from any likeness to that thing itself." In terms of Spearman's analysis, his conclusion is inevitable; but this analysis is not the only one possible, since it is conceivable that the material thing itself, as well as the physiological stages which intervene between it and consciousness, each and all possess *formal* characteristics of an identical order. In other words, if the material object, the physiological configuration of nervous response, and the perception, or seen object, were *formally identical*, a definition of contour would be jointly conditioned by the structure of the object perceived, by the maturation of previously established conditions of readiness in the sensorium, and by the general attitude of the whole psychophysical organism at the time of apprehension.

What this definition of contour *does* to the object which was not done at the first and less articulate stage of emergence is contingent upon observable data within the configuration of A, toward which more or less discrete and differentiated modes of behavior are directed. Thus, to return to our scheme, we have not only A and not-A, as the terms of our configuration, but also the a^1 and a^2 dimensions of A, which, being the "characters" of the contour, supply the basis for all comparison and all discrimination. The relation which Spearman *educes* is therefore a feature of the configuration. Likewise the eduction of correlates is but the completion of a figure by a supplementation of missing members: $\underline{\text{not-A}}\,\boxed{a^1\;\mathbf{A}\;a^2}\,\underline{\text{not-A}}$.

I submit this scheme as a substitute for the one advanced by Spearman in the belief that it meets the conditions imposed by logic without using the terms of logic as if they were themselves psychological data.

After presenting his three principles of apprehension and the eduction of relations and correlates, Spearman advances five quantitative principles which govern the general factor of intelligence.

I. With respect to mental energy, "every mind tends to keep its total simultaneous cognitive output constant in quantity, however varying in quality."

II. With respect to retentivity, "the occurrence of any cognitive event produces a tendency for it to occur afterwards;" in connection with which it appears that "cognitive events cease more gradually than their (apparent) causes."

III. With respect to fatigue, "the occurrence of any cognitive event produces a tendency opposed to its occurring afterwards."

IV. With respect to conative control, "the intensity of cognition can be controlled by conation."

V. With respect to primordial potencies, "every manifestation of the preceding four quantitative principles is superposed upon, as its ultimate basis, certain primordial but variable individual potencies."

These quantitative principles, Spearman goes on to say, bring about the three processes of *reproduction, disparition,* and *clearness-variation,* which are " 'anoegenetic' in the sense that they neither have the nature of self-evident propositions nor generate any new items in the cognitive field."

The discussion then leads to a definition of clearness in terms of *intensity* and *determinateness,* in the course of which it is pointed out that the determinateness of cognition is slower than the intensity of cognition. Certain experiments by the author on visibility are cited which seem to indicate three distinct temporal curves for intensity of sentiency, intensity of cognition, and determinateness of cognition; the first of these attaining its maximum after about .2 sec., the second after .5 sec., and the third after 1.0 sec. The correlation of these with their physical counterparts is indicated by the following scheme:

Mental

Conation (including habit and set)	Cognitive intensity	Determinateness (together with any differentiation)
↓	↑	↑

Physical Direction of energy → Cortical tension → Cortical configuration

This scheme fits well into the general setting of our own constructive suggestions, if mere sentience is made equivalent to what we have called the undifferentiated ground of consciousness. Cognitive intensity, then, arises with an appropriate cortical tension which occasions the emphasis of A above not-A, without, however, defining A's contour. For definition, a "cortical configuration" is requisite whose counterpart is a more definitely articulated figure of consciouness, the dimensions or characters of which are differentiated. In a word, *determinateness* is equivalent to what we have called the definition of contour.

The essential difference between Spearman's interpretation of cognition and the one here contrasted with it is the difference between the conception of an aggregate of materials and forms, which can be supplemented with new materials and new forms by *eduction,* and a conception which rejects the aggregate as an observed datum of experience, and regards cognition, and hence intelligence, as processes involving constructive membership. The apprehended unit is assumed from the start to be an organized whole, although it is one which is always capable of further organization through further definition of its members. Definition, however, is accomplished neither by the associative principle of accretion, nor by the alien and somewhat mysterious act of eduction, but solely by the integrative principle of organic membership inherent in the factual data themselves.

When Spearman emphasizes the surprising validity of Ebbinghaus' completion test as still one of the best, though it is one of the earliest single tests of intelligence, he is at the same time emphasizing the fact that intelligence strives for completeness by closing the gaps which otherwise

render experience incomprehensible. But is not comprehension itself a membered whole the contour of which is complete and definite because its internal structure is complete and definite?

What then is the nature of "intelligence?" An answer to this question would be that the nature of intelligence is revealed by the nature of the contour of experience. Whether the mode of experience be extended as a picture to the eye, or drawn out as a melodic sequence to the ear, or knit together in a continuous course of movement as a rhythmically balanced, economical response, in all cases we are defining the contour of a figure, with variations of definition which contribute degrees of intelligence. But always there is a perceptual aspect which can be more or less definitely objectified, and always there is a movement of response incapable of the same degree of determinateness as to the successive features of the movements involved.

Two questions remain. (1) Does this alternative suggestion answer Spearman's demands for "clear thinking, ultimate laws, and an appropriate experimental procedure?" Only further investigation can answer, but at least it may be said that the laws of configuration, so far as they have been derived, are a direct product of experimentation, whereas the laws of evocation which Spearman advances impress one as a somewhat belated formulation of the now obsolete psychology of "imageless thought." (2) Has Spearman's or any other attempt to define "intelligence" any significant bearing upon the improvement of the technique of mental measurements? This, too, is a question which will be answered, not from the arm-chair, but from the field of experimental testing.

R. M. Ogden

Cornell University

BOOK REVIEWS

(1) *The Education of Gifted Children.* By LULU M. STEDMAN. Yonkers-on-Hudson, The World Book Co., 1924. pp. vii+192. One of the "Measurement and Adjustment Series," edited by Lewis M. Terman. Price $1.80.

(2) *The Unstable Child: An Interpretation of Psychology as a Source of Unbalanced Behavior in Abnormal and Troublesome Children.* By FLORENCE MATEER. New York, D. Appleton & Co., 1924. pp. xii+471.

Of all literature on applied psychology the best is that which reports the results of actual trials, carefully planned and systematically carried out, or the observations of competent observers with ample material for study. The two books above mentioned belong to this excellent type.

(1) Miss Stedman, the organizer, and training teacher in charge, of the "Opportunity Room" of the training school of the Southern Branch of the University of California, reports the results of an experiment running through five or six years in giving exceptional opportunities to exceptionally able children in a special class in which the lock-step of the ordinary school system was discarded and the children worked with great freedom and largely on their own initiative. The group was small of course and their teacher no doubt exceptionally qualified. They were of chronological ages from 9 to 13, with high I. Q.'s (mostly over 140), and were doing work corresponding to the upper grammar school grades or the junior high school. Seventeen cases are described in some detail, five or six of them with a chapter each, and make interesting reading.

The course in the "Opportunity Room" was planned, not with a view to rushing the children at the rapid pace, which they were easily able to take, through the ordinary curriculum, but rather with a view to holding them within a reasonable distance of their fellows of like chronological age, while their powers were fully engaged and their interest held by an enriched curriculum. The fears of critics that segregation would lead to conceit and that a fuller course would mean over-work both proved happily groundless. The children liked it and accomplished marvels, as the samples of their literary work and musical compositions abundantly testify. No one having to do with the teaching of superior children will want to miss the reading of Miss Stedman's account.

(2) The 21 chapters of Dr. Mateer's book are divided into two parts, of which the first treats of the psychological clinic and the fine art of conducting it successfully, and the second gives the results of case studies, 350 or more in number, chiefly of young delinquents referred for study by the Ohio courts. The central point of the first part is the absolute necessity of getting away from the notion that a bare I. Q. is an adequate basis for the diagnosis of the case of a problem-child and of substituting for it the conviction that the only adequate basis is a thoroughly individualized study, making use of many methods and extending to all the aspects, both inner and outer, of the child's life and behavior. The central point of the second part is the contention, strongly supported by evidence, that the cause of delinquency in children in the vast majority of cases—Dr. Mateer would perhaps say in all—is some inadequacy in the functioning of such mental capacity as the child has, some deviation in his mental or physical organization which makes it impossible for him to respond normally; or to put the same thing more epigrammatically: "There is no such thing as a bad boy or girl. Either the child does not know any better or else he cannot help it." This deviation is what Dr. Mateer calls "psychopathy"—an intermediate condition grading out of the normal on one side and into the psychotic on the other.

For the valuable practical suggestions of Part I, and for the case records of Part II and the inferences drawn from them the reader will wish to consult the original, but one observation of the author's, to which a chapter is devoted in Part II, is worthy of special mention. Dr. Mateer has observed that children with congenital syphilis respond to the various test situations in a uniformly characteristic manner,—so characteristic, in fact, that she has been able in a number of cases to make a diagnosis of congenital syphilis by this means alone, in the absence of the ordinary physical signs and in advance of a Wassermann test. While the general reliability of such a method has yet to be confirmed on a large scale and by other observers, there seems no inherent impossibility about it in view of the characteristic mental symptoms of general paresis.

The book closes with a bibliography of 69 titles and an index. It is a work which may be commended to all who are interested in the development of clinical psychology to something nearer its full possibilities than it has yet attained.

E. C. S.

The Child: His Nature and Needs: A Survey of Present-Day Knowledge Concerning Child Nature and the Promotion of the Well-Being and Education of the Young; A Contribution of the Children's Foundation. Prepared under the editorial supervision of M. V. O'Shea. pp. ix+516.

This volume is, as its sub-title indicates, a comprehensive general statement of present-day knowledge, published and distributed by the newly organized Children's Foundation at Valparaiso, Indiana, of which Mr. L. E. Myers is the patron. The material is arranged in three major sections dealing respectively with child nature, child care, and education. The work is a cooperative one and the names of the contributors guarantee a high level of excellence.

To Part I Professor B. T. Baldwin contributes a chapter on Bridging the Gap between our Knowledge of Child Nature and the Training of Children; Professor Mary T. Whitney on The Child's Instincts and Impulses; Professor W. F. Dearborn on The Development of the Intellect in Childhood and Youth; Dr. H. Neumann on The Child's Moral Equipment and Development; Professor F. E. Bolton on Social Traits; and Professor E. A. Kirkpatrick on Language, Drawing and Music.

In Part II, Professor H. H. Goddard writes on Bridging the Gap Between our Knowledge of Child Well-Being and our Care of the Young; Dr. W. R. P. Emerson on The Relation of Nutrition to Mental Development; Dr. W. A. White on Nervous and Mental Hygiene; Professor C. E. A. Winslow on Sense Defects; Dr. W. Healy on Delinquency; Professor A. Gesell on Intellectually Inferior Children; Professor Leta Hollingworth on Intellectually Superior Children; and Dr. W. S. Hall on The Adolescent Period.

In Part III an introductory paper is furnished by Commissioner J. J. Tigert, and five chapters on modern educational tendencies and ideals by Professor M. V. O'Shea. The work closes with nearly 20 pages of select bibliographies of sources in English on matters treated in the several chapters, a section of short biographical sketches of the contributors, and a full index.

It is hardly to be expected that in a work of this character anything of great novelty should be offered—the purpose of the work is rather a compendious statement of things already known—but it should, nevertheless, prove useful as a handy means of orientation to beginners in the study of children and as a book for reference and collateral reading for college classes in education.

E. C. S.

A Magician Among the Spirits. By HARRY HOUDINI. New York, Harper and Brothers, 1924. pp. xix, 294 and 13 half-tone cuts.

Houdini writes of Spiritism in this book, as the title indicates, from the point of view of the professional magician, a thing which he by training and interest is particularly well qualified to do; for, as he tells us in the introduction, he has devoted 30 years to the study and investigation of occult phenomena. Very early in his career he identified himself with the movement, "joining the rank and file and holding séances as an independent medium to fathom the truth of it all." Since then the examination of these phenomena has been his life's avocation. Though in all his search and in all his investigation he "has not found one incident that savoured of the genuine," his interest has never flagged. He writes earnestly and sympathetically, and with an openmindedness and scientific poise that are remarkable for a man of his experiences.

The first part of the book is devoted to biographical sketches and the examination of the "claims" of the great leaders in the cult. Houdini considers in the order named the Fox sisters, the Davenport brothers, Daniel Dunglas Home, Eusapia Palladino, Ann O'Delia Diss Debar, and Henry Slade. With the exception of an account of his friendly relations with Ira Davenport, which shows that this so-called disciple of Spiritism had no belief in it, and of the publication of a letter from the medium Remigius Weiss, which contained Slade's written confession that all of his "pretended Spiritualistic manifestations were and are deceptive, performed through tricks," Houdini gives nothing new. He merely reviews in an interesting and entertaining manner what has already been published about these persons.

Though it is patent that the author could not consider all the mediums of first rank, that he had to pick and choose to keep his book within due bounds, he nevertheless, in the reviewer's opinion, should have added at least one name, Mrs. Piper's, to his list. She was a medium of a different stripe from any that he considers, her claims are as great as theirs, and her position among the ranks of the "true believers" is probably more secure.

In the chapters which follow the biographical sections, Houdini explains the technique of slate writing, of spirit raps, of levitation, and of spirit photography. A chapter is devoted to Sir Arthur Conan Dyole with whom Houdini maintains a cordial friendship, and the credulity, naïveté, and childlike faith of this author are clearly displayed. The next chapter is given to the consideration of ectoplasm; Houdini tells of his experiences with Mlle. Eva C., a virtuoso of the art of regurgitation; and of d'Albé's exposure of Kathleen Goligher of the "Goligher Circle", made famous by the writings of the physicist W. J. Crawford. A moralizing chapter is devoted to the by-products of Spiritualism, and specific instances are given of "the suffering, losses, misfortunes, crimes and atrocities of which it is the underlying cause and must bear the primary responsibility." One of the most interesting chapters of the book is that dealing with the methods employed by mediums to obtain information regarding their "clients." Houdini shows that this is not so much a matter of "lucky hits" or of "fishing," though both are taken advantage of, as it is of the systematic way in which they set about to gather it. The extent to which the "will to believe" is necessary is shown in the chapter, "What you must believe to be a Spiritualist."

Two chapters deserve particular mention: "Investigations—Wise and Otherwise," and "Magicians as Detectors of Fraud." In the first of these Houdini tells of the complications surrounding an investigation and outlines the prerequisites that investigations should have. He then illustrates his remarks by reference to specific investigations,—good and bad. His failure to refer to the *Experiments in Psychical Research at Leland Stanford, Jr., University* is surprising; for of all the investigations that have been

undertaken this work of Coover's without doubt takes first rank. In the second chapter he shows that the magician is not, by virtue of his profession alone, competent to meet the chicanery of a skilled medium, and he illustrates the magician's fallibility by citing instances in which some of the greatest performers have been "taken in." The book closes with a short chapter in which Houdini summarizes his findings, and with 8 appendices which deal with special topics.

The book is interesting throughout and it is one that the novice in Spiritism should read.

K. M. D.

Nervous and Mental Re-education. By SHEPARD IVORY FRANZ. New York, The Macmillan Co. 1923. pp. ix, 225.

Re-education, for Franz, involves the same principle as education—habit formation. There is, however, this difference: in the original habit the instinctive tendency has been used as a basis for the acquirement of the habit, but in re-education the original instinctive tendencies cannot be so utilized. The problem is, therefore, to find a new foundation for habit and to develop the new reaction upon that basis. In most cases this is not difficult, as the ambition to be socially and economically independent is usually found. The methods of re-education may be applied both to the training of bodily reactions, or the acquirement of new co-ordinations, and to habits of thinking.

The scope of the work is as follows: Part I treats of the Basis and General Concept of Re-education; Part II, of the Principles of General Re-education; and Part III, of Neurological and Mental Adjustments.

Although the programme is broad in scope, the author is concerned largely with the neuro-muscular response—the type of readjustments necessary in such diseases as anterior poliomyelitis, tabes dorsalis, the cerebral paralyses, etc. Here the problem is the building up of new motor co-ordinations. One chapter alone is devoted to the psychotic, which includes the neurasthenic, the psychasthenic and the hysteric. In this group, the patient must have insight into his own condition; he must desire to get well and must possess self-confidence in order to benefit from training. Here the attention has to be centered upon the social activities which in the organic group do not need especial emphasis.

The writer's theory of education as habit formation is applied consistently throughout, but the reviewer is impressed with the distinction between the organic and the psychotic groups. Can education, or re-education, be limited to the formation of habitual responses? Franz cites this example as illustrating the formation of habit. "Experiments with cats show very well that the same situation may result in different habits. A good example of this is with respect to the activities of different cats in a box, the door of which can be opened by moving a string which is attached to a latch. One cat learned to do this by arching its back and by rubbing against the string, thus exerting enough pressure to move the latch. A second animal acquired the habit of biting and pulling the string, and a third cat clawed and pulled the string. All these are motor habits. Each animal has learned to move in a special way when it received certain stimuli (the sight and feel of the particular box) and the special habit which is produced depends upon the instinctive reactions of the particular animal." The fact that the same stimulus does not call forth the same response [illegible] differences in the instinctive reactions of the animals. From the standpoint of Köhler's learning experiments with animals, the reviewer would think that the problem appeared differently in the course of the solution to each animal; that is, each animal adopted a different means of arriving at the desired goal. Thus the different habitual responses that the animals acquired would be attributed, not to differences

in instinctive equipment, but to differences in the individual solution of the problem. There is no evidence that would point to the fact that the animal is originally equipped to deal in a particular manner with such a puzzle box. The exercise of 'intelligence,' in the sense in which Köhler uses the term, would bring about these different achievements and would account for the habitual responses established as a result of these achievements. The solution of the educational problem would seem to point, not in the direction of the acquiring of habitual, stereotyped reactions, but in the direction of training so that the individual would be able to act intelligently —to exercise his initiative in the solution of problems.

In retraining neuro-muscular responses, the difference between the two concepts of education is not so apparent; with the psychotic, however, the difference is emphasized, as we are here dealing with disturbances of judgment, with the inability of the individual to perceive and to react as normal individuals do. In so far as re-education of the mentally disturbed is possible, the reviewer would suggest that training in 'intelligence' is requisite—training which permits the patient to seek out desired goals, the accepted ones of social life, and to modify his conduct toward these goals.

From a practical point of view, the work is very useful, in that it represents the results of long and successful work in this field by Dr. Franz. To the physicians and educators who deal with the physically and mentally handicapped, the book is heartily recommended.

Applied Psychology. By BERNARD C. EWER. New York, The Macmillan Company. 1923. pp. viii, 480.

The author of this text endeavors to steer a middle course between "elaborate theory and experimental investigation, characterized by technique which is highly refined and to some extent unintelligible except to the specialist" and "popular psychology, which presents little that is of practical value, since it lacks the scientific foundation and formulation necessary to make it reliable." He realizes that bizarre claims have been made for this study, that over-confidence and charlatanism have often characterized the field. Applied Psychology is, for the author, "the statement of psychological principles which have value with reference to the practical interests of mankind. It includes also the experimental method of solving practical problems."

Chapters II, III, IV are devoted to the critical statement of certain principles: Analytic Observation, Scientific Explanation, Mechanical Control, Experiment, Subconsciousness and Suggestion. Ewer appreciates the hazy nature of the usual concepts and cautions the reader against a too ready juggling of popular terms. To quote by way of illustration: "Freudianism seems to the writer to be a mixture of scientific observation and explanation with crude metaphysics and deceptively imposing terminology . . . . Some of the doctrines appear to be sheer fancy or the dignifying of insignificant facts by impressive nomenclature."

The topics of the Measurement of Intelligence, Fundamental Factors in Education, the Learning Process, Intellectual Efficiency, Control of Emotion and the Will, Psychotherapy and Suggestion, Vocational Selection, Industrial Psychology, Advertising and Salesmanship are treated in the remainder of the book. With the exception of the chapter on Learning, these topics are adequately treated. The psychology of learning is very fundamental for an applied psychology—in fact, one might say that it is the core around which the whole subject should be constructed. Here, however, we are given descriptions of the kinds of learning and of habits; and these do not solve the important problem: *How* does the individual

learn? In the answer to this question, and not in statistical data, interesting as they may be, is the solution of this problem and of the problem of formal training to be found.

In the opinion of the reviewer, this text is, in general, a clear, sane and common-sense presentation of the subject. In these respects it is superior to other works of the kind in English.

Cornell University SETH WAKEMAN

Evolution and Religion. By HENRY FAIRFIELD OSBORN. New York, Charles Scribner's Sons, 1923. pp. 21. Price 75 cents.

This booklet is a reply to an article by W. J. Bryan in the *New York Times* of Sunday, Feb. 26th, 1922, and appeared in the same paper on the Sunday following. What can one do with such a subject in the cramped space of a Sunday newspaper article? Little enough! But what could be done Dr. Osborn has done and has had, of course, no difficulty in showing that evolution and orthodox Christianity are entirely compatible and have been even from the time of St. Augustine.

Giving Your Child the Best Chance. By RUTH DANENHOWER WILSON. Chicago, A. C. McClurg & Co., 1924. pp. 196.

This little book has been prepared to give to the parent, face to face with the practical problem of steering his own children through their mental and moral troubles, some notion of what students of genetic psychology and psychoanalysis have found that would be helpful to him. Mrs. Wilson is evidently familiar with the best sources in English and, what is still more important, knows something at first hand about children. The material is handled simply and readably, four of the nine chapters having earlier stood the test of publication in one of the popular magazines.

E. C. S.

(1) *Herring Revision of the Binet-Simon Tests. Examination Manual: Form A*. By JOHN P. HERRING. Yonkers, World Book Co. 1922. pp. 56.

(2) *A Point Scale for Measuring Mental Ability. 1923 Revision*. By ROBERT M. YERKES and JOSEPHINE CURTIS FOSTER. Baltimore, Warwick and York. 1923. pp. 219.

(3) *Intelligence Measurement: A Psychological and Statistical Study Based upon the Block-Design Tests*. By S. C. KOHS. New York, Macmillan Co. 1923. pp. xii, 312.

(4) *Measuring Minds: An Examiner's Manual to Accompany the Myers Mental Measure*. By CAROLINE F. MYERS and GARRY C. MYERS. New York and Chicago, Newson and Co. 1921. pp. 55.

(5) *Intelligence Tests and School Reorganization*. By TERMAN, DICKSON, SUTHERLAND, FRANZEN, TUPPER and FERNALD. Yonkers, World Book Co. 1922. pp. viii, 111.

(6) *A Critical Study of Certain Measures of Mental Ability and School Performance*. By INEZ MAY NETERER. Baltimore, Warwick and York. 1923. pp. 141.

(7) *The Will-Temperament and Its Testing*. By JUNE E. DOWNEY. Yonkers, World Book Co. 1923. pp. v, 339.

Intelligence tests of one sort and another continue to appear and to demand attention. One may still scan the newer publications eagerly in the hope of finding a test or tests that rest upon a foundation firmer than the earlier tests. We have reached the point, however, where we may soon expect to discover tests which have a basis other than an empirical one.

The recent theoretical studies of Spearman and Thurston seem to point in this direction. The qualitative tests of intelligence of Lipmann and Bogen evidence the fact that quantitative tests, empirically justified, are but a stage in the development of intelligence measurement.

In the works listed above, with the exception of Downey's work upon the Will-Temperament, we are upon old and familiar ground. In the Herring Revision of the Simon-Binet Tests, we have another adaptation of the Binet material with a number of new tests added. On the whole, the material is more suitable than in the Stanford Revision, and it is more conveniently arranged. Being based upon a point system, rather than in months' credit, it would have been a distinct advance upon the Stanford Revision had it adopted a percentile score rather than a mental age score. There is nothing sacred in the concept of mental age.

A revision of the 1914 edition of the Yerkes-Bridges Point Scale is presented by Yerkes and Foster. There are also given a Point Scale for the Measurement of Intelligence in Adolescent and Adult Individuals and an Infant Point Scale. One gathers that the work has been somewhat hastily put together. The new scales are tentatively presented and are admittedly in need of further standardization and study. There is a real need of a test that should cover the earlier years of the Standard Revision; but is hardly seems that the test will be found in the same sort of verbal tests made use of in the Stanford Revision and also in this Infant Point Scale.

The Block-Design Test of Kohs is a valuable addition to the list of performance tests. It is based upon very careful statistical work and will serve as a useful supplementary test in mental examinations.

The Myers Mental Measure is a "single continuous scale consisting wholly of pictures and applicable to all ages and degrees of school experience. Each section of this test sets tasks easy and simple enough for the kindergarten child and at the same time other tasks hard enough for the student. In this respect it is unique." The authors present graphs and tables which, in their opinion, justify this claim.

The manual upon Intelligence Tests and School Reorganization, edited by Terman, is a description of a few of the experiments that are being carried on in this country with intelligence tests. The practical problems arising from the use of tests in classifying pupils in the public schools and the solutions that have been attempted are described in a very interesting and suggestive manner.

Neterer's monograph is a detailed study of the correlations existing between various intelligence tests, educational measurements and teachers' estimates and school maıks. The study was carried on with 329 pupils in Grade 4 B of eight schools in Seattle. The author found "that no one measure provided a basis for the classification of children into groups uniform enough for efficient teaching except in respect to the ability measured by that test or measurement." This is in agreement with previous studies, which have indicated that the most reliable index to be used in classifying pupils can be derived from a combination of intelligence tests, educational tests and teacher's ratings, but that no one of them alone may be used efficiently.

Downey's work with the Will-Profile, and her earlier studies in graphology, which served as a basis for her later work, are well known and hardly need comment. The problem of evaluating qualitatively the reactions of the individual and of presenting the results in terms of their integration, or temperament, is a very difficult one, and in this Downey's work is pioneer. It is to be hoped that she will continue her experimental work in this field, to which she has already made a very marked contribution.

The Defective Delinquent and Insane. By HENRY A. COTTON. Princeton, Princeton University Press. 1921. pp. xvi, 201. (The Louis Clark Vanuxem Foundation Lectures, Princeton University.)

Dr. Cotton, the medical director of the New Jersey State Hospital at Trenton, has attacked the traditional attitude of the medical profession towards the insane. He would regard state hospitals for the insane not as custodial asylums, but as institutions where the insane are given the same care and medical attention received in other hospitals; the rôle of the physician cannot be the mere classifying and diagnosing of mental disorders. The medical problem of the insane is the same as any other medical problem: the study of causation and the application of relief. The prevention of insanity, too, is a problem not to be overlooked. With this attitude, the author approaches the problem and gives in this series of lectures the results of his study of the so-called 'functional neuroses.'

The distinction between 'organic' and 'functional' mental disorders can no longer be maintained. Although in organic disturbance pathological changes in the brain have been demonstrated, the absence of pathological changes in the functional group does not signify that these disturbances are purely mental in their causation. "Anatomical investigations have repeatedly shown that conditions which have been classed among the functional diseases possess, in reality, an anatomical basis." "While psychiatrists, in the past, have held the non-biological view of the nature of the so-called functional mental disturbances, the biologists have produced evidence regarding function and structure which when applied to mental disorders will, undoubtedly, modify these traditional ideas. The biologists are definite in the assertion that there can be no function without structure. This being true it would also be true that there can be no abnormal function without a corresponding abnormal structure . . . If we could conceive of a mental state independent of the brain, and all known facts refute such a belief, then we could believe that certain forms of insanity were diseases of the mind and not diseases of the brain." Dr. Cotton expects to find, therefore, some physical cause of functional disorder.

The author regards the doctrine of 'hereditary transmission of mental diseases' as very instable from a scientific standpoint. Eugenicists and sociologists have seized upon the doctrine of the inheritance of insanity; and it is somewhat of a relief, to the reviewer, to find a more scientific attitude to this problem. Tredgold has already pointed out, as regards the inheritance of mental deficiency, that we are dealing with what he terms the 'neuropathic diathesis' and not the direct transmission of certain abnormalities. Dr. Cotton would regard the 'inherited constitution' as the "individual's constitutional resistance to various toxins, rather than to merely mental instability." He finds that hereditary taint exists in only a little more than half of the patients classed in the functional group.

Various factors usually cited as the cause of mental disorder—worry, grief, shock, mental overwork—play an important rôle in the mechanism of the psychoses. These tend to lower the patient's resistance in that loss of appetite, nutritional disturbances and loss of sleep, the result of these psychogenic factors, will cause latent infections, which may be of long duration, to crop out. The emotional reactions, on account of their intimate relation to the endocrine system, probably lower the resistance of an individual so that a latent infection may become active.

Dr. Cotton states, "Psychoses arise from a combination of many factors, some of which may be absent, but the most constant one is an intercerebral, bio-chemical, cellular disturbance arising from circulating toxins, originating in chronic focal infections situated throughout the body and probably to some extent in disturbances of the endocrine system." He would regard the functional psychoses as 'toxic' psychoses, and would include in this group the acute condition of manic-depressive insanity,

the chronic conditions characterized by brain deterioration and grouped under dementia præcox, and paranoia (chronic delusional states). The psychoneuroses—hysteria, neurasthenia and psychasthenia—also respond to toxic treatment.

The primary foci of infection are usually found in the teeth and tonsils, while the secondary foci are located in the gastro-intestinal tract and other internal organs of the body. The reviewer will not here enter into a discussion of the methods of diagnosing infection and the manner in which it responds to treatment. A considerable number of cases in which the removal of the focal infection has resulted in restoring the patients to mental stability are reported in some detail. The success of the work at Trenton may be gathered from the fact that in a few years the percentage of discharged patients to new admissions in the 'toxic' group increased from 40 to 80. During this period, the removal of toxic conditions was the only therapeutic method used. Dr. Cotton regards the psychological study of abnormal mental conditions as of considerable value; but the use of psychotherapy and psychoanalysis does not, in his opinion, produce any marked benefit with 'toxic' psychosis. It may, on the other hand, prove to be dangerous, in that the patient's attention is directed to his mental symptoms which consequently become overemphasized.

Of the three classes of mental defectives, the formative (or congenital), the functional and the traumatic types, Dr. Cotton considers that the functional type, *i.e.*, the emotionally instable and the defective with apparent absence of congenital defect, may be regarded as due to a toxic condition. If this condition be detected early enough, it would respond to treatment in the same manner as the toxic psychosis.

These lectures may be regarded as the report of work in progress. If the writer appears to be too dogmatic upon the subject of focal infections and their relations to mental disturbances usually regarded as purely functional, this may be attributed to a commendable enthusiasm for a point of view and method which have produced very remarkable results. Dr. Cotton links up his theory with Meyer's theory of integration, that is, the theory of the functions of the total person as representing an integration rather than a summation of detachable parts. The psychologist will be interested in the further investigation and elaboration of this point of view. This work represents, in the reviewer's opinion, a new chapter, and a greatly needed one, in the care and treatment of the mentally abnormal.

Cornell University SETH WAKEMAN

NOTES

DR. OBERLY ON "THE RANGE FOR VISUAL ATTENTION, COGNITION AND APPREHENSION"

In a recent number of this JOURNAL,[1] Dr. H. S. Oberly reported the results of a tachistoscopic study. He found three conscious patterns which formed the basis of his *Os*' reports, and which were described as "immediate," "grouping," and "counting." In the "immediate" type the number of dots was immediately perceived. The dots were ungrouped and of an equal and high degree of clearness. In the "grouping" type the number of dots was likewise immediately perceived, but the dots, owing to different degrees of clearness, were grouped into patterns. In the "counting" type the report of number of dots was mediated by 're-imaging' and counting.

Because his characterisations did "not correspond to the systematic categories that have appeared in the literature" Dr. Oberly "transformed

[1]35, 1924, 332-352.

his results" into attention, cognition and apprehension.[2] For every one of these categories he computed statistical thresholds. The 'transformation' is, however, both incorrect and misleading. It is misleading because the use of the term attention transcends the cognitive field in which the study was made. That the work was upon the cognitive level is attested by Dr. Oberly's instructions to his *O*s: "Immediately after the exposure you will report verbally: (1) the number of dots which you have apprehended; (2) the degree of assurance or certainty of your judgment [measured on a 5-point scale—1 being a "guess or do not know" and 5 the certainty of "a 100 to 1 bet"]; (3) the method of determining your report." To ask an *O* to report the number of dots apprehended, to estimate the degree of certainty of his answer, and to tell the method that he used in making his determinations, is to refer his report to the field of cognition.

The 'transformation' is incorrect because attention is not a cognitive term. While it is true that an attentive consciousness may functionally be cognitive,—as it may also be emotive or volitional,—it is not true, even from the point of view of the functional systems of psychology, that attention may be regarded as a level, step, or degree of cognition. Cognition, furthermore, was not a happy choice for Dr. Oberly's second category; for cognition is not a specific level on the side of awareness or knowledge-about, but is a general term synonymous or co-extensive with awareness or knowledge-about. To restrict the use of the term 'cognition' to the 'grouping' type of his *O*s' reports is to identify the whole with a part.

Preferable terms—if other characterizations than 'immediate,' 'grouping' and 'counting' are needed—would be 'field apprehension,' 'group apprehension' and 'number apprehension.' These three types, however, are not logically co-ordinate. The 'field' and 'group' apprehensions are both immediate, and are both given in a single 'flash' of consciousness; whereas the 'number' apprehensions are mediated by 'subsequent re-imaging' and 'verbal counting.' The first two are of the same kind; 'field' apprehension is merely a limiting case of 'group' apprehension. The two logically fall into one class which stands in opposition to the third. So in reality, Dr. Oberly gives us but two classes, the first of which is divided into two sub-types.

Moreover, the limens were not computed in accordance with the phi-gamma hypothesis, for the reports upon which the calculations were based were selected. "It was arbitrarily determined," Dr. Oberly says, "to use for purposes of calculation only those reports which were *correct* and which

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[2] The 'immediate' reports were 'transformed' into 'attention' because "all the dots were perceived in a single flash of consciousness with an equal and high degree of clearness," and because "this description agrees with the definition of attention as given by Titchener." Nowhere, however, does Titchener define attention as a one-level consciousness. On the contrary, attention for Titchener is a state of consciousness which is divided into the clear and the unclear, as the following quotation from *A Beginner's Psychology* (92) proves: "Whenever our experience shows the patterns of vivid centre and dim background, of bright focus and obscure margin, then we have attention before us." The unclear is as essential to the state of attention as the clear. (*Cf.* Titchener's discussion of attention in *The Psychology of Feeling and Attention*, 1908, 220 ff.; in *A Text-Book of Psychology*, 1909, 266 f.; and his reply to Britz in *Psych. Rev.*, 24, 1917, 56 f.)

The pattern of 'grouping' is 'identified' with 'cognition' because "cognition as it appears in the literature implies" [an] "immediate consciousness" in which "owing to different degrees of clearness there is an attentional grouping of the stimulus dots," and because "Dallenbach found in an experiment on sensory clearness (Cognitive *vs.* Attributive Clearness, *Jour. of Exp. Psych.*, 3, 1920, 182-230; also 4, 1921, 143-163) that two levels were possible, and......[that] the experiment dealing with two such levels is an experiment in cognition," and because Dallenbach "associates the 'lower level' of clearness (which is the same as expressed by Titchener) with cognition." Dr. Oberly, however, has misread me; for I nowhere make these statements, nor do his conclusions follow from my work. I gave my *O*s a dual instruction: I asked them to give (1) a description of process and (2) a statement of meaning. Under the first instruction the *O*s reported, for every consciousness, two levels of sensory clearness. Under the second instruction the *O*s reported various degrees of cognitive clearness. The results showed that processes at both levels of sensory clearness were cognizable, and that there was no absolute or invariable relation between sensory clearness and cognition; an impression may be attributively clear and at various degrees of cognitive clearness, or an impression may be attributively unclear and at various degrees of cognitive clearness (*op. cit.*, 255 ff.).

had an *assurance of* 5, 4, *or* 3." Unless the number of dots on an exposure card was correctly cognized, and unless "the *O* was sure enough of his report to put even money on it," the experimental result was not considered! Furthermore, the reports "I do not know" were ranked with the doubtful judgments and were placed on the scale of assurance with a "guess."

Dr. Oberly's study is a contribution, not to the psychology of attention, but to the psychology of apprehension. His limens give us merely the ranges of the confident, correct cognitions that are immediate and ungrouped, immediate and grouped, and mediated and counted.

K. M. Dallenbach

The Moscow Institute

Friends of Professor G. Tschelpanow, who remember his visit to our American laboratories in 1911, will be glad to have word of him. Professor Tschelpanow tells me that he has retired from the directorship of the Moscow Institute, and is devoting his leisure to the writing of a System of Psychology. He sends the following (officially authorised) account of the activity of the Institute since 1914.

"At the beginning of 1914 I forwarded to you the first volume of the Psychological Researches of the Moscow Psychological Institute. From 1914 to 1917 our work was carried on along almost normal lines; many researches were completed. In order to publish these we founded in 1917 the journal 'Psichologitcheskoie Obosrenie' (Psychological Review), but were able to issue only three numbers in 1917 and 1918. After that and up to now the Institute has been unable to publish its works, this with the exception of very few, some of which appeared in the form of pamphlets while others were printed in German periodicals. In 1918 the Government granted 100,000 Rbls for enlarging the mechanical workshop. The acquisition of apparatus for this sum brought up our workshop to the highest degree of accomplishment. The years 1918 and 1919 were unfavorable for scientific research work, owing to absolute lack of heating.

"In 1920 was opened the section of 'Applied Psychology and Psychology of Labor' on funds provided by the Government. The number of workers and the size of the premises were quite sufficient for the carrying out of this plan.

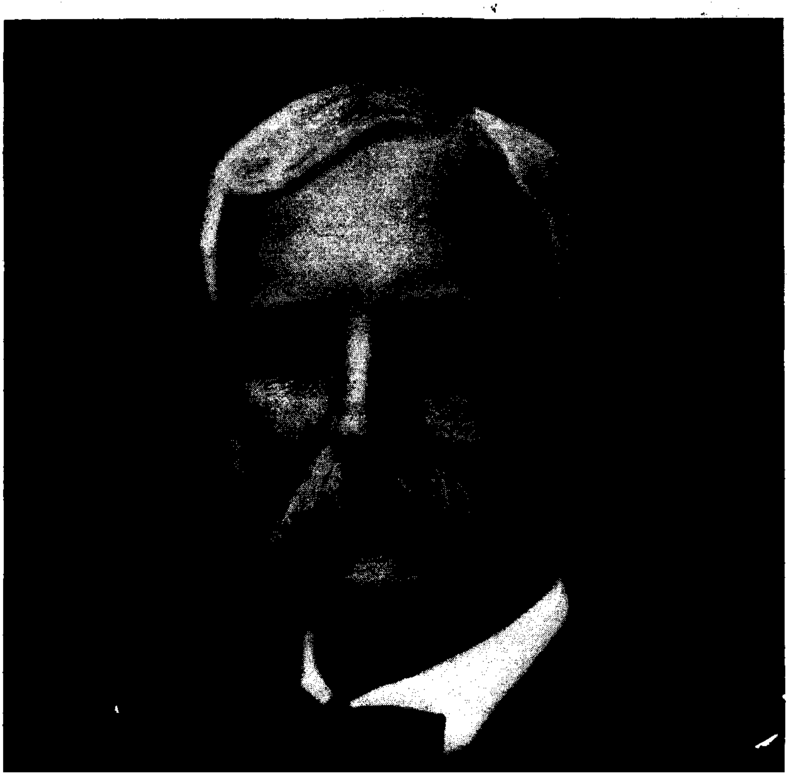
"In 1921, despite enormous technical difficulties, was completed the construction of our 'Universal Psychological Apparatus,' which can be of great interest also to American psychologists and educationalists. A detailed description of this set of apparatus will be forwarded to you in the near future.

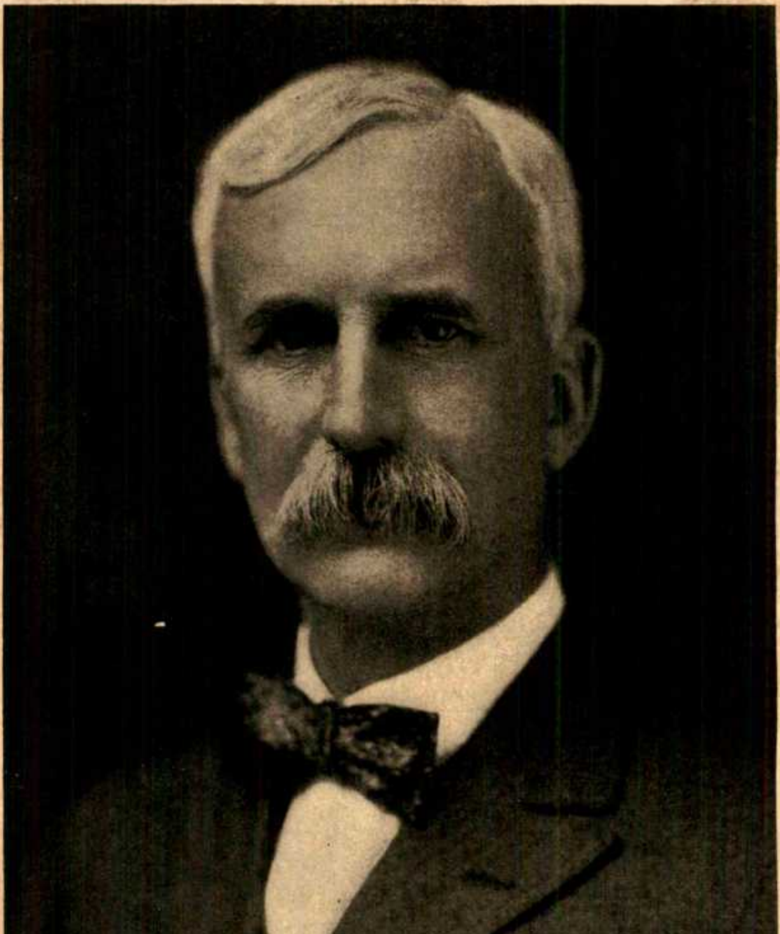
"In 1922 the Government undertook the first great reform of the Psychological Institute. This institution was reorganized into a special 'Research Institute,' which was to unite all the psychological establishments in Moscow; the number of collaborators in this organisation reached eighty.

"At present, psychological research in the Institute is conducted by a new staff of psychologists exclusively in the spirit of Marxist Materialism."

E. B. T.

We regret to announce the death, on November 22 last, of Edmund Clark Sanford, head of the department of psychology in Clark University, and for many years an associate editor of the Journal. A sketch of Dr. Sanford's life and work will appear in the next number.





Edmund C Sanford

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EDMUND CLARK SANFORD 1859-1924

ACTING EDITOR 1888-9; ASSOCIATE EDITOR 1895-1920; COOPERATING EDITOR 1920-4

Dr. Edmund C. Sanford, head of the department of psychology in Clark University, died suddenly in Boston, November 22, 1924. His loss will be felt nationally and institutionally, as well as personally. He was one of the acknowledged leaders of the second generation of American psychologists. He spent his whole professional life—with the exception of a year's instructorship at Johns Hopkins—in the service of Clark University and Clark College. In social intercourse he did not easily let down the bars of an unvarying and somewhat formal courtesy; but those who were privileged to know him intimately know that he had a genius for friendship.

Edmund Clark Sanford was born in Oakland, California, November 10, 1859. Both his father and his mother came of old New England stock. The Sanfords were among the early settlers of Connecticut; the line includes a colonial governor of that State; and a certain David Sanford served as chaplain in the revolutionary army. The Clarks, too, were an original Connecticut family; a Clark of the sixth generation behind Sanford was one of the founders of Farmingham. His grandmother, Milicent Washburn Clark, carried her pedigree back to Plymouth Rock and 1620; Thomas Welles, governor of Connecticut in 1655, was one of her ancestors. It happened, however,—I do not know the circumstances [illegible] Jennie Clark met and married in California, where their son was born.

Sanford was educated in the public schools of Oakland, whose citizens were largely of New England descent and took a characteristic pride in their school system. From the High School he passed, in 1879, to the university of California, then a private institution of the same town. His father, a druggist, died in February of the following year, and Sanford was obliged to leave the university for a time and to devote himself to business. As soon as he could be spared, he returned to his academic work, and graduated with the A.B. degree in May, 1883.

While he was in charge of the drug-store Sanford had decided upon the study of medicine; he "would rather write prescriptions than put them up!" He needed money for his medical preparation; and, as nothing better offered itself, he accepted a position as teacher in the neighboring village of Trout Brook Cañon, to and from which he rode daily. Almost immediately, however, he received an invitation (probably through the employment secretary of the university) to go for two years to Oahu College, Honolulu, as teacher of "the Classics, Mathematics (Algebra, Geometry, Trigonometry, Surveying and Navigation) and Ancient History." He had the courage to accept, and sailed off in September, 1883, taking his medical books with him. I know nothing of his time in Honolulu except that he spent the summer of 1884 as a guest in the house of some pupils and had the opportunity to make the famous 'volcano trip' to Kilauea. The two years proved to him, however, that his real interest lay in teaching; he gave up the idea of medicine, and returned to California in June, 1885, bent upon an educational career.

In the autumn of the same year he entered the Johns Hopkins University as a graduate student. It was natural for him to turn to Baltimore; for not only was the Hopkins practically the only university in the country offering graduate work, but Sanford's cousin, Charles Howard Shinn, was also a Hopkins man; the change from medicine to education was, therefore, in all probability, nothing more than a change from department to department within the chosen university. Education, however, meant Stanley Hall, and the Stanley Hall of that period was busied with experimental psychology as well as with education. It was, undoubtedly, Hall's influence that led San-

ford to adopt psychology as his major interest; but I know no details. He stayed at the Hopkins for three years as student, receiving a scholarship in 1886 and a fellowship in 1887; he graduated with the Ph.D. in 1888; and he remained for a fourth year as instructor in psychology.[1]

During this year, 1888-9, he was also on the books of Clark University; Stanley Hall was in Europe, and Sanford edited the JOURNAL in his absence. He went to Clark, as instructor in psychology, in July, 1889, and maintained an unbroken connection with the University and College to his death in 1924. In 1892 he was made assistant professor of psychology, with exclusive charge of the laboratory; in 1900 he was promoted to the professorship of experimental and comparative psychology. In 1909 he became the second president (following Carroll Davidson Wright) of Clark College.

Sanford did not turn, without mature consideration, from scientific to administrative and educational work; he realized that he was giving up a profession in which he had become a leader for another, equally arduous, in which he was untried. In seeking to understand his reasons, we must remember, first, that education had been his early love; he had 'found himself' during his two years in Honolulu. We must remember also—I return to the point later—that he was never robust, and that a man of fifty cannot meet the sheerly physical demands of laboratory work as a younger man can. I believe, however, that the real ground of the change lay in Sanford's attitude to psychology itself. No man of high intelligence can be content to produce experimental papers, year after year, without looking beyond them for some sanction, some coordinating principle that shall hold them all together. And experimental psychology, it seems to me, can offer only two such sanctions: system, the applied logic of general psychology, and application, the putting of psychological facts and laws to some social use. But Sanford, like his master Stanley Hall, was not a systematist; the problems of systematic psychology found him cold—even a little impatient. It was natural, then, that he should turn in middle life to

[1] I owe to Sanford's sister, Miss Martha L. Sanford, most of the information contained in these paragraphs. Certain details have come to me from Mrs. W. F. Frear, one of Sanford's pupils at Oahu College, and from Dr. L. N. Wilson, Librarian of Clark University.

some form of application; and the presidency of the College promised him an opportunity of applying his psychology that accorded admirably with his adolescent interests. He thought the matter over, and accepted the position.

It seemed, at the time, that the decision meant a final separation from psychology, and that Sanford was to spend the rest of his working life as administrator.[2] Circumstances ruled otherwise. In 1920 it became possible, for the first time under the provisions of the Founder's will, to bring Clark University and Clark College under a single administrative head. The Trustees approved the union, and Sanford resigned the presidency of the College. He was immediately appointed professor of psychology and education in the University, in succession to Stanley Hall, and he held this chair to the end. He spent the academic year 1920-21 in Baltimore, preparing for his new duties, and then took up again his professional work; he would have retired, for age, in February, 1925.

Sanford married, December 28, 1901, Florence Bartling, his class-mate in the Oakland High School and at the University of California. His wife died December 1, 1922.

He received honorary degrees from Hobart College (Sc. D., 1909), the University of California (LL. D., 1912), and Clark University (LL. D., 1924). He was president of the American Psychological Association in 1902.

Sanford was a man of one book—and he left that only half finished. He had planned to write another, that should deal with the educational ventures of Clark College from its establishment in 1902 to the end of the first decade of his own presidency. As late as 1923 he was asking my advice whether he should attempt this new work or should add the lacking Part II. to his *Laboratory Course*. So far as I know, he did not even make a beginning upon either. This seeming inertia calls for explanation, especially as there are other indications of the same sort. No one, I think, who knows Sanford's reputation as a psycholog-

[2]Sanford himself was so sure of his farewell to psychology that he distributed his professional library among those colleagues and former students who he thought would make good use of it; he had an exceptionally valuable collection of 'separates.' It was characteristic of him to give the books away instead of selling them; it is to the honor of the recipients that, when he resumed his chair, they offered with one accord to return them.

ist, can look over the Bibliography appended to this sketch without wondering why the list is not longer. And those of us who recall the early meetings of the Experimentalists remember also that Sanford had all sorts of plans, problems, ideas for apparatus and what not, that were formulated, discussed and then—dropped. There must have been a reason.

I feel sure that the reason may be stated, very simply, as precarious health, constitutional weakness. Sanford has more than once told me that he worked very slowly; and he gave an immense amount of time to his students; the contents of the papers presently to be listed show that with all clearness, even if one had no outside knowledge of the fact. But there can be little doubt that he was handicapped by bodily limitations. In his school-days he suffered two severe illnesses, typhoid fever and what was then called inflammation of the brain; and in his early years at Clark he underwent a similarly severe attack of rheumatic fever, which very probably affected his heart. The true wonder is, then, not that he did not publish more, but that he was able to accomplish as much as he did.[3]

The one book is, of course, an historic document. Six instalments, under the title A Laboratory Course in Physiological Psychology, were published in the JOURNAL between 1891 and 1896. The first four, recast into six chapters which covered the field of sensation, were issued in book-form ('edition of advanced sheets') in 1894, as A Course in Experimental Psychology. The early copies appeared in paper covers,—one of my professional treasures is the booklet in dingy blue that carries Sanford's autographed 'compliments,'—but these were soon exchanged for the familiar cloth of Heath's Pedagogical Library. Part I. was completed by the issue in 1898 of three more chapters: on the visual perception of space and motion (this chapter includes the last two of the JOURNAL instalments), on Weber's Law and the psychophysical methods, and on apparatus. The new chapters, with appendices on Listing's Law and the horopter, and with notes and suggestions on chapters I.-VI., were published both as

[3]Miss Sanford tells me that she remembers having heard her brother called 'lazy,'—surely a curious adjective to apply to a youth who, with Sanford's handicaps, had passed through High School and College in the normal period!

a separate book and also in combination with the 'advanced sheets' of these earlier chapters. The two portions of the single volume were not, however, brought down to the same date; chapters I.-VI. take us only to the end of 1893, chs. VII.-IX. to the end of 1896. Part. II was to have contained chapters on "Reflex and Voluntary Movement, The Time Relations of Mental Phenomena, Association, Memory, Attention and Emotion, so far as these subjects can be approached with experiments of moderate difficulty, together with a chapter on the apparatus necessary for such experiments."

The importance of this Laboratory Course, in one respect, may be gathered from the words of Sanford's first preface. "The author's excuse," he says, "for allowing the publication, even in this modest form, of so incomplete a work, must be the very extraordinary condition of experimental psychology at this time. Many laboratories have been opened, and many teachers of psychology are anxious to give their students the benefit of demonstrations and practice work, and yet there is absolutely no laboratory handbook of the subject to be had. At such a time half a loaf may be better than no bread." The 'very extraordinary' is strong, but not too strong. In the nineties of the last century psychological laboratories—laboratories in which psychology was supposed to be taught to undergraduate students—were springing up all over the continent, while as yet nobody knew what to teach or how to teach it. The *Einführungscursus* which some of us had taken in Leipzig covered only the metric methods and the technique of the chronoscope, and these were hardly subjects for the American sophomore. It is therefore no overstatement to say that Sanford's JOURNAL papers and the resulting book saved a rather critical situation; they furnished, so to speak, a common denominator for laboratory courses; they guided the uninformed and steadied the unstable. And since—as the Preface does not and could not tell us—the work of selection and presentation was excellently done, the book has a permanent as well as an historic value. I should like to repeat what I wrote of it in 1901, just ten years after the appearance of the first paper: "It has a high historical importance, as the first manual of experimental psychology; it has exerted, and still exerts, a wide influence, as the gateway through which American

students are introduced to laboratory work; and it is a monument of accurate erudition." Would that it had been brought to completion!

The list of Sanford's published writings, like those of the writings of many other men of science, begins with verse. The little poems are not intrinsically important; but the mention of them gives me the opportunity to refer to a second principal influence brought to bear on Sanford's thought and attitude,—an influence felt earlier than that of Stanley Hall and on the whole, perhaps, felt more deeply. In his undergraduate days Sanford came under the spell of Edward Rowland Sill;[4] and all his life long his judgment in matters of literary taste, his choice of books for holiday reading, his attitude to music and to the other arts, his whole aesthetic outlook were, with but little modification by personal temperament, the judgment and attitude and outlook of his former professor. One could almost always tell beforehand what his response would be to a given problem of aesthetics or to a given work of art; and once I was goaded to remark—he was fond of the word 'reaction'—that he need not on that account write himself down a reactionary. I used especially to argue with him that novel-reading is an essential part of a psychologist's education; but I could not convince him, and he stuck to his Biographies. He had it in him, I am sure, to acquire independence: but, after all, the gaining of an independent aesthetic judgment means very hard work, and I have already intimated that Sanford had no reserve of physical stamina upon which to draw. From this point of view, then, we may count it a stroke of good fortune that the influence which moulded his aesthetic life was as wholesome and as discriminating as Sill's must undoubtedly have been.

For the rest, the bibliography, besides showing Sanford's general competence in experimental and comparative psychology, bears witness to three special abilities that should not be left

[4]Miss Sanford has shown me a copy of *The University of California Magazine*, 2, 1896, no. 4, *Sill Memorial Number*, in which Sanford writes (134-136) of "Professor Sill as a Teacher." Sanford speaks of "the transcendent good fortune" of having come under Sill's instruction. He mentions a "Psychology Club, which met from time to time at Professor Sill's home in the evening," when topics of philosophy and ethics came under discussion. It is at least possible that this connection of 'psychology' with Sill prepared him to accept psychology when he met Stanley Hall.

without a word or two. First of all, he had a particular gift for popularisation; the lectures on the Function of the Several Senses in the Mental Life and on Psychic Research in the Animal Field are models of their kind; and there were many others of the same type and of like merit—I remember vividly a lecture on the Psychology of Sleight of Hand—which have not found their way into print. Secondly, he had what for want of a better term I must call the knack of the Minor Study. It is far easier now than it was thirty years ago to find experimental problems that shall occupy a student only for a single academic year and whose results shall still be worth publishing; but Sanford's first Minor Study appeared in 1893. Professor Jastrow and he were, indeed, the pioneers in a line of work that has proved both fruitful to science and valuable for education. And, thirdly, he had the love of the true laboratory man for making things with his own hands; I suppose that he was never happier than when 'playing in the shop.'[5] The Clark Laboratory of the older days had something of the attraction of a puzzle or a conundrum; one was always coming upon bits of apparatus whose use and meaning set one guessing. Many of these inventions, as I have said, were left unfinished; but many were completed and described, and a good number have become part of the regular equipment of our laboratories. One of the best known, the vernier chronoscope, has won international recognition; Sanford described a rough model in 1890, and the perfected forms in 1898 and 1901.

If, then, one might be tempted, without knowledge of Sanford's physical condition, to express surprise that the list of his psychological writings is not longer, one could have no least doubt of the quality of his output or of the mental calibre of the man behind it. America is justly proud of the exceptional three who make up the first generation of her psychologists; two of them, we all agree, showed marks of what may properly be called genius. But America has no reason to be anything else than proud of a second generation of which, in range, in originality and in thoroughness of work, Sanford should be a typical representative.

E. B. T.

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[5]Sanford was an excellent mechanic. He built, among other things, a telescope,—grinding and polishing the lenses himself,—which he recently made over to Professor F. B. Williams for use in the department of mathematics in Clark University.

APPENDIX I

Sanford's Work with Students

I remarked above that Sanford gave an immense amount of time to work with and for his students. That was the Clark policy and the Clark tradition,—that every member of the department should be accessible for consultation to every student; Hall and Sanford and Professor Burnham were thus equally at the student's disposal, whichever of the three might be ultimately responsible for publication. I feel, as his former students themselves will also feel, that any outline of Sanford's professional activity which left out a reference to this work—work both of direction and of participation—would be misleading and incomplete. I have therefore decided, as the most nearly objective procedure, to print a list of published articles in which we know that Sanford was in some way and in some degree concerned. The list will be a reminder to his old students of that intellectual partnership of two generations which is one of the delights of work of research; and his suggestions of subjects of investigation, since they indicate the direction of his own psychological interest, will help to complete the picture that the list of his published writings gives us.

I. Theses signed by G. S. Hall and E. C. Sanford[6]

1891. Nichols, H. The psychology of time, 3, 453-529; 4, 60-112.
"I was requested by the instructor in Psychology at Clark University to investigate . . . the Constant Error of Time-judgments." Sanford served as observer.

1892. Bryan, W. L. On the development of voluntary motor ability, 5, 125-204.
Sanford served as observer, and devised mechanical counter.

1894. Bergström, J. A. An experimental study of the conditions of mental activity, 6, 247-274.
"Chiefly done under the direction of" Sanford, who served as observer.

1894. Dresslar, F. B. Studies in the psychology of touch, 6, 313-368.

1897. Lindley, E. H. A study of puzzles with special reference to the psychology of mental adaptation, 8, 431-493.
"Suggestion of the topic; . . . advice and criticism in the working out of it."

II. Theses signed by E. C. Sanford

1896. Drew, F. Attention; experimental and critical, 7, 533-572.
"General direction and constant aid and counsel." Sanford served as observer, and added a Note reporting check experiments.

1900. Small, W. S. An experimental study of the mental processes of the rat, 11, 133-165; 12, 206-239.
"Initial suggestion, ample laboratory facilities, continued interest and helpful criticism."

1901. Kinnaman, A. J. A comparison of judgments for weights lifted with the hand and foot, 12, 240-263.

1902. Sears, C. H. A contribution to the psychology of rhythm, 13, 28-61.
"Suggestion [of topic]; . . . help rendered throughout."

1903. Swift, E. J. Studies in the psychology and physiology of learning, 14, 201-251.

1904. Kuhlmann, F. Experimental studies in mental deficiency, 15, 391-440.
"Suggestion [of topic]; constant advice and many valuable suggestions."

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[6]For the headings and items of the first two lists I am indebted to Dr. L. N. Wilson. All references, unless otherwise specified, are to this Journal.

1906. Terman, L. M. Genius and stupidity, *Ped. Sem.*, 13, 307-373.
1906. Browne, C. E. The psychology of the simple arithmetical processes: a study of certain habits of attention and association, 17, 1-37.
1906. Porter, J. P. The habits, instincts, and mental powers of spiders, genera *argiope* and *epeira*, 17, 306-357.

Thanks Hall and Sanford "for suggesting the topic of this research and for assistance in studying it."

1906. Gesell, A. L. Accuracy in handwriting as related to school intelligence and sex, 17, 394-405.
1907. Cleveland, A. A. The psychology of chess and of learning to play it, 18, 269-308.

"Suggestion of this topic of study, and...generous assistance in following it out."

1907. Davis, H. B. The raccoon: a study in animal intelligence, 18, 447-489.
1908. Book, W. F. The psychology of skill: with special reference to its acquisition in typewriting, *U. of Montana Publications in Psych.*, 1, 1-188.
1909. Ellison, L. The acquisition of technical skill, *Ped. Sem.*, 16, 49-63.
1910. Rowe, E. C. Voluntary movement, 21, 513-562.
1911. Kakise, H. A preliminary experimental study of the conscious concomitants of understanding, 22, 14-64.
1923. Fryer, D. Intelligence and interest in vocational adjustment, *Ped Sem.*, 30, 127-151.
1924. Richards-Nash, A. A. The psychology of superior children, *Ped. Sem.*, 30, 209–246.

It is plain to a psychologist that these papers do not all stand on the same level; the thesis is a function of the student, as well as of his professor! Yet I think it would be difficult to find another list of eighteen consecutive theses that should show a larger proportion of solid work. Drew's article has held its place among the selected references of Ebbinghaus' *Psychologie* through three editions; Washburn attributes to Small priority in the use of the 'labyrinth method'; and many other items of the list have received like recognition.[7]

Several of the authors cited express their thanks to Sanford for 'criticism.' To one who did not know him well, the term will seem to accord ill with his usual gentle speech and with the tone of his printed reviews. He disliked, it is true, to say harsh things in print, and he disliked controversy. Man to man, however, in correspondence and in conversation, Sanford could command a very searching criticism,—all the more searching as it was invariably of the internal sort; he did not set up against yours an argument or a method of his own, but he looked to see what flaw there was in your method and your argument, and thus helped you to strengthen an attack or a position. Many, many times I have invited his criticism, and always I have profited by it. So his students, looking back over their struggles with procedure and with literary presentation, could hardly fail to realise—though the full realisation would come only later, when their own experience had ripened—something of what his critical comments and objections had done for their work.

III. Other Articles in this Journal[8]

1891. Fraser, A. Visualisation as a chief source of the psychology of Hobbes, Locke, Berkeley, and Hume, 4, 230-247.

"The starting-point of this paper was a suggestion [from Sanford]... that I should investigate the figures of speech used in psychology."

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[7]H. Ebbinghaus, *Grundzüge der Psychologie*, 1, 1902, 595; 1905, 621; 1911, 780. M. F. Washburn, *The Animal Mind*, 1908, 219.

[8]This list has been hurriedly prepared, and I am not sure that it is complete.

1894. Bolton, T. L. Rhythm, 6, 145-238.
Ascribes to Sanford "much of the credit for the success of the work. But for his skill in devising and constructing apparatus, the work could not have been carried on. His suggestions as regards methods for making the experiment were no less valuable than his assistance in devising apparatus."

1894. Bergström, J. A. The relation of the interference to the practice effect of an association, 6, 433-442.

1896. Scott, C. A. On old age and death, 8, 67-122.

1896. Smith, T. L. On muscular memory, 7, 453-490.
Sanford observed, and devised automatic shutter.

1896. Lindley, E. H. A preliminary study of some of the motor phenomena of mental effort, 7, 491-517.

1897. Phillips, D. E. Genesis of number-forms, 8, 506-527.

1898. Kline, L. W. The migratory impulse *vs.* love of home, 10, 1-81.

1899. Colegrove, F. W. Individual memories, 10, 228-255.

1899. Kline, L. W. Methods in animal psychology, 10, 256-279.
Thanks for "suggesting the work itself"; and for "valuable help and timely suggestions at every turn during its execution."

1899. Kline, L. W. Suggestions toward a laboratory course in comparative psychology, 10, 399-430.
Thanks for "the original plan of the work, and for seeing that ample laboratory material was provided me, together with much assistance in the arrangement of the subject-matter of this paper."

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THE EFFECT OF VARYING THE INTENSITY OF LIGHT ON THE DISAGREEMENT OF FLICKER AND EQUALITY-OF-BRIGHTNESS PHOTOMETRY FOR LIGHTS OF DIFFERENT COMPOSITION

By C. E. FERREE and GERTRUDE RAND, Bryn Mawr College

In two previous papers[1], the cause of the disagreement of the flicker and equality-of-brightness methods of photometry was discussed, and the relation of the lag in visual sensation to this disagreement was experimentally shown. It is the purpose of this and the following papers to develop farther the argument advanced in those papers, by showing that the effect of varying the intensity of light and the speed of rotation of the exposure disc on the disagreement of the results obtained by these two methods of photometry is just what would be expected from lag as the cause of the disagreement.

In the series of experiments planned to carry out the comparison between the results by the methods of flicker and equality-of-brightness on the one hand, and the rise of sensation on the other, three intensities of light were used, 12.5, 25 and 50 meter-candles. Several intensities were added during the course of the work in order to investigate certain points that came out in the study of the relation of the two photometric methods. The general trend of the results obtained in these experiments was checked and verified by 20 *Os*.

It was found that the type of disagreement between the two methods was different for each of these three intensities. For example, at 12.5 m.c. B was overestimated by 10.23%; and R, Y and G were underestimated by 22.54, 21.26 and 4.72% respectively. At 25 m.c. R, Y, G and B were all underestimated,—R by 6.56%, Y by 3.96%, G by 17.15% and B by 24.28%. At 50 m.c. R was overestimated by 17.89% and Y by 23.83%. B and G were underestimated,—B by 16.67% and G by 15.74%. At intensities somewhat lower than 12.5 m.c., *e. g.*, at 10.20 m.c., B and G were overestimated and R and Y were underestimated. That is, between these lower intensities and intensities in the neighborhood of 50 m.c. there was a complete reversal of the type of disagreement for the R and Y and for the B and G. Figg. 1-4 should be consulted for a compari‹n

[1] The Cause of the Disagreement Between Flicker and Equality-of-Brightness Photometry, this JOURNAL, 35, 1924, 190-208; and Flicker Photometry and the Lag of Visual Sensation, *ibid.*, 35, 1924, 209-216.

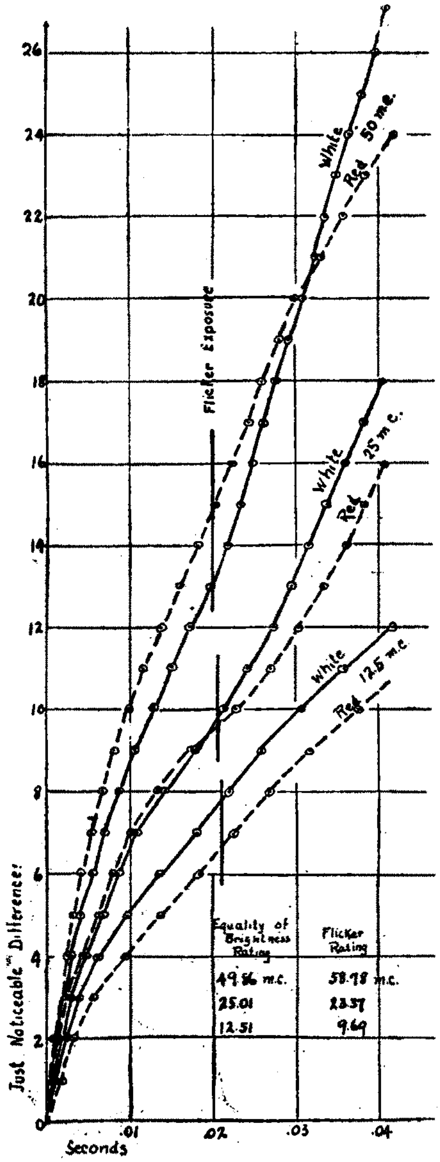

FIG. 1. Curves showing the rise of sensation up to 0.04 sec. exposure for red and white, 12.5, 25 and 50 meter-candles

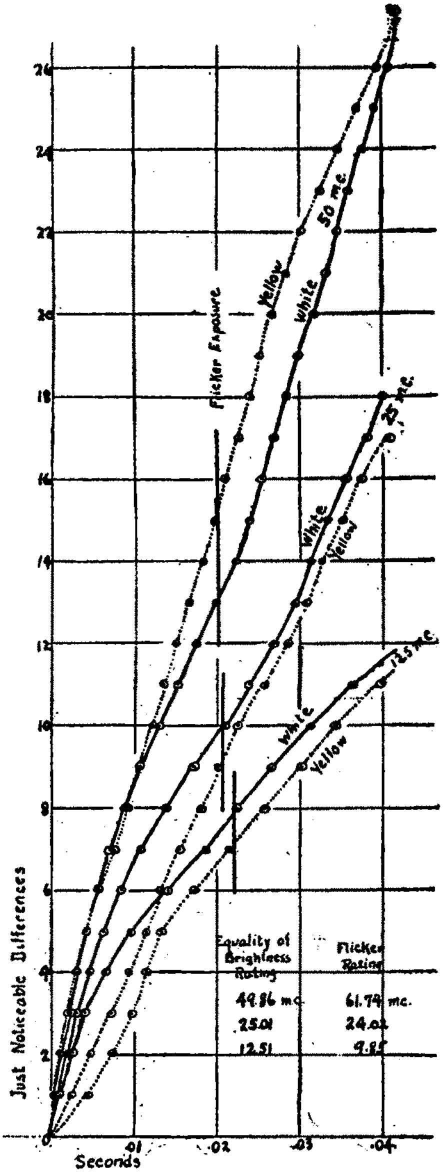

FIG. 2. Curves showing the rise of sensation up to 0.04 sec. exposure for yellow and white, 12.5, 25 and 50 meter-candles

of the rise-of-sensation curves with the amount and direction of these deviations from those obtained by the equality-of-brightness method.

Since all of the colors showed the characteristic shift from underestimation to overestimation or the converse, it seemed reasonable to expect that an intensity might be found for each pair of lights at which agreement with the equality-of-brightness rating would occur. The search for this intensity was somewhat of a trial-and-error character,—controlled, however, by a fairly regular decrease in the disagreement as the intensity in question was approached. For example, R was overestimated at 50 m.c.; underestimated at 25 m.c.; and still more underestimated at 12.5 m.c. Agreement was found at 29.39 m.c. A similar indication of the region of intensity in which agreement might be expected to occur was had in case of each of the other colors. Agreement was found for Y at 26.12 m.c.; for G at 11.91 m.c.; and for B at 14.91 m.c. The region over which agreement could be obtained was narrow, as is shown by the large angle at which the curve expressing percentage disagreement of the results by the method of flicker crosses the line representing agreement for the different intensities as rated by the equality-of-brightness method (Fig. 6). The three remaining colors were photometered at each of the intensities at which agreement was found for one of the colors, in order to get some idea of the deviation that might be expected for the other parts of the spectrum if one of these intensities were selected as standard. For example, at 14.91 m.c. (intensity at which agreement occurred for B) R was underestimated by 14.69%; Y by 17.71%; and G by 8.79%. At 11.91 m.c. (intensity at which agreement occurred for G) R was underestimated by 21.92%; and Y by 21.25%. B was overestimated by 10.83%. These determinations were made at the speed of rotation of the disc ordinarily used in the method of flicker, *i. e.*, the speed which gives the greatest sensitivity. When the speed was changed, agreement no longer occurred at these intensities. The amounts of disagreement for the other colors were also shifted by the change of speed. It is quite possible, of course, that another intensity may be found for each pair of lights above 50 m.c. or below 12.5 m.c. at which agreement would occur.

In searching for the intensity at which agreement occurs in case of B, an interesting and very baffling phenomenon of low flicker sensitivity was found over a range on either side of the point at which agreement occurred with the equality-of-brightness method. A change from 13.6 to 15.8 m.c., as rated by the equality-of-brightness method, gave no difference in rating by the method of flicker. This low sensitivity by the method of flicker was very hard to understand until the rise-of-sensation

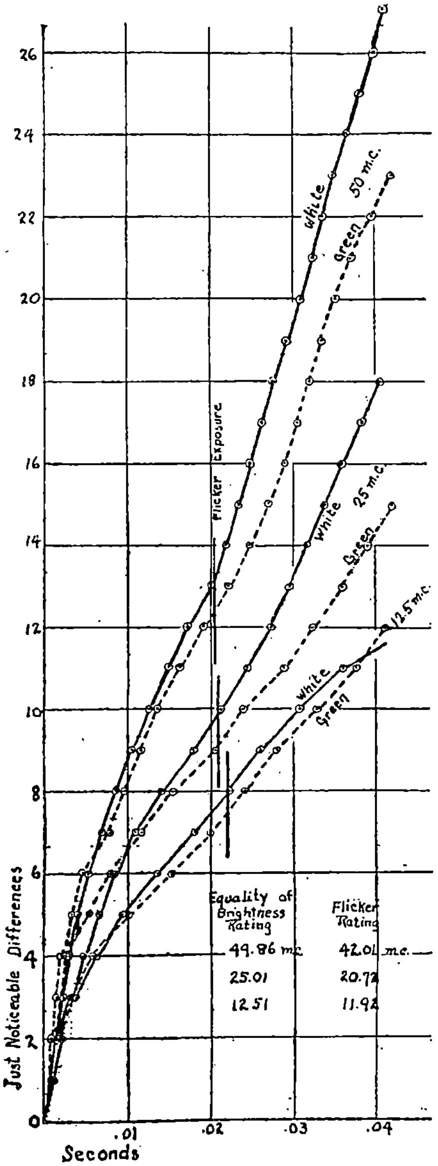

Fig. 3. Curves showing the rise of sensation up to 0.04 sec. exposure for green and white, 12.5, 25 and 50 meter-candles

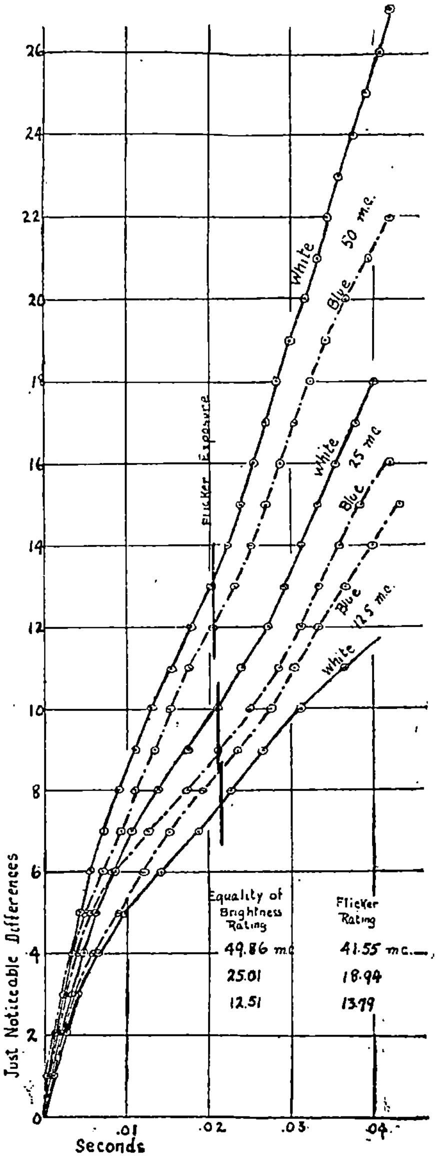

Fig. 4. Curves showing the rise of sensation up to 0.04 sec. exposure for blue and white, 12.5, 25 and 50 meter-candles

work for 12.5 and 25 m.c. was completed and the curves were plotted. A comparison of these curves showed very little difference in the sensation level for the lengths of exposure used in the method of flicker even for a change as great as from 12.5 to 25 m.c. That is, the rise-of-sensation curves for these two intensities of B lie very close together for quite a little distance above and below the point representing the flicker exposure. We were confronted here, then, by the somewhat anomalous result that, when the carbon standard was set to give 14.91 m.c. on the photometric field, the colored light could be changed through a range from 131.6 to 15.8 m.c., as rated by the equality-of-bright-

TABLE I

Showing the Effect of Intensity of Light on the Amount and Type of Disagreement of the Results by the Equality-of-Brightness and Flicker Methods

Color	Photometric Rating: Equality-of-Brightness Method	Photometric Rating: Flicker Method	Difference between the two methods		Revolutions per sec. Flicker Disc.	Length of Individual Exposures
	m.c.	m.c.	m.c.	%		sec.
R (675$m\mu$)	49.86	58.78	+ 8.92	+17.89	12.5	0.020
	34.03	35.56	+ 1.53	+ 4.50	12.4	0.02016
	29.39	29.39	0	0	12.2	0.02049
	26.12	24.46	− 1.66	− 6.36	12.2	0.02049
	25.01	23.37	− 1.64	− 6.56	12.15	0.02058
	14.91	12.72	− 2.19	−14.69	11.8	0.02119
	12.51	9.69	− 2.82	−22.54	12.0	0.02083
	11.91	9.30	− 2.61	−21.92	11.75	0.02128
Y (579$m\mu$)	49.86	61.74	+11.88	+23.83	12.6	0.01984
	29.39	32.87	+ 3.48	+11.84	12.2	0.02049
	26.12	26.12	0	0	11.9	0.02101
	25.01	24.02	− 0.99	− 3.96	12.2	0.02049
	14.91	12.27	− 2.64	−17.71	11.5	0.02174
	12.51	9.85	− 2.66	−21.26	11.35	0.02203
	11.91	9.38	− 2.53	−21.25	11.1	0.02252
G (515$m\mu$)	49.86	42.01	− 7.85	−15.74	12.3	0.02033
	29.39	23.80	− 5.59	−19.02	12.4	0.02016
	26.12	21.71	− 4.41	−16.89	11.9	0.02101
	25.01	20.72	− 4.29	−17.15	11.8	0.02119
	14.91	13.60	− 1.31	− 8.79	11.5	0.02174
	12.51	11.92	− 0.59	− 4.72	11.3	0.02212
	11.91	11.91	0	0	11.2	0.02232
	10.20	11.18	+ 0.98	+ 9.61	11.0	0.02273
B (466 $m\mu$)	49.86	41.55	− 8.31	−16.67	12.3	0.02033
	29.39	22.55	− 6.84	−23.27	12.0	0.02083
	26.12	20.00	− 6.12	−23.43	12.2	0.02049
	25.01	18.94	[illegible]	[illegible]	[illegible]	[illegible]
	15.80	14.91	− 0.89	− 5.63	12.0	0.02083
	14.91	14.91	0	0	12.0	0.02083
	13.60	14.91	+ 1.31	+ 9.63	12.0	0.02083
	12.51	13.79	+ 1.28	+10.23	11.6	0.02155
	11.91	13.20	+ 1.29	+10.83	11.8	0.02119

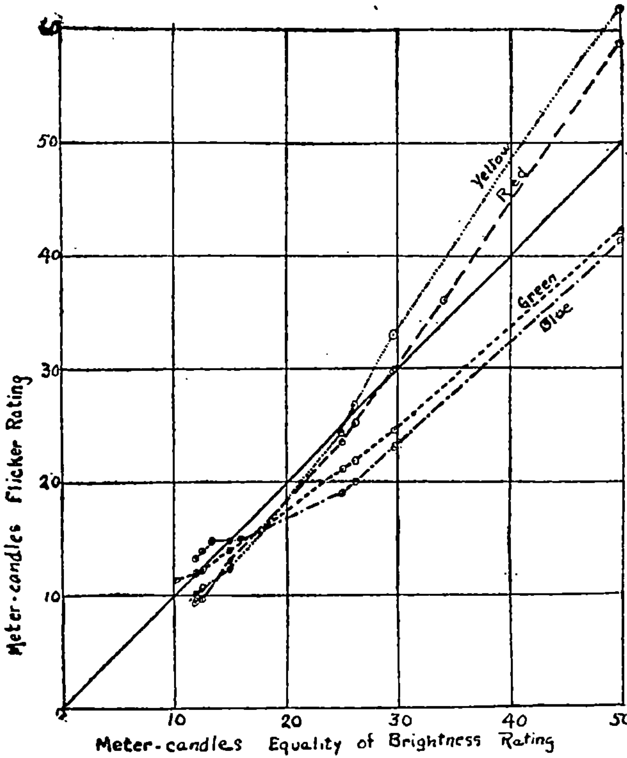

FIG. 5. Curves showing the effect of varying intensity on the disagreement between the equality-of-brightness and flicker ratings for lights of different composition

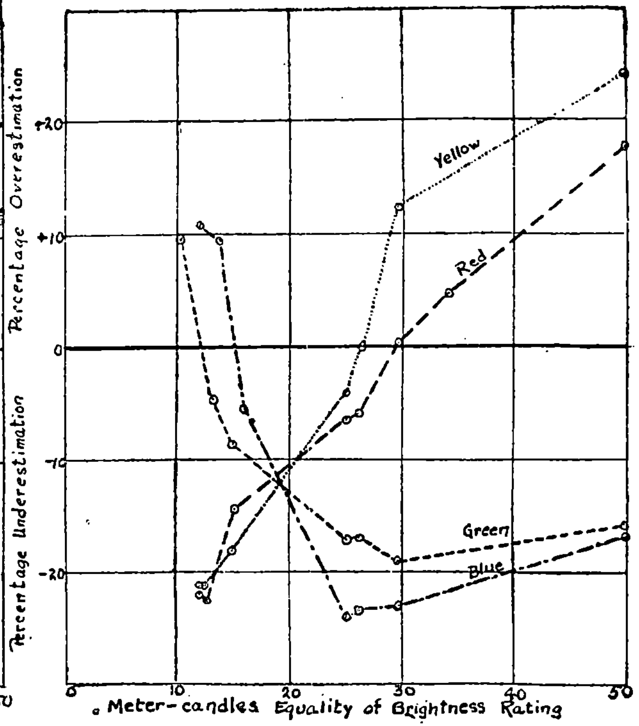

FIG. 6. Showing in percent. the overestimation and underestimation of the flicker as compared with the equality-of-brightness ratings for lights of different compositions at different intensities

ness method, without producing any change of result by the flicker method; yet the result of the comparison with the equality-of-brightness method had to be pronounced an underestimation of 9.63% by the method of flicker at the lowest point of this range; agreement at 14.91 m.c.; and an overestimation of 5.63% at the highest point in the range. As the sensitivity of the equality-of-brightness method was of the order of 1.5 to 2.5% we have here the unusual phenomenon of the equality-of-brightness method showing much greater sensitivity than the method of flicker.

The results of this work on the effect of variation of intensity are shown in Table I and Figg. 5 and 6. In Fig. 5 equality-of-brightness rating is plotted along the horizontal coordinate and flicker rating along the vertical coordinate. Agreement between the two methods is represented by a solid line bisecting the angle formed by the two coordinates. Points on the curve for each of the colors falling above the line show a higher rating by the flicker than by the equality-of-brightness method or overestimation by the method of flicker, and points falling below the line a lower rating by the flicker than by the equality-of-brightness method or underestimation by the method of flicker.

Fig. 6 represents percentage overestimation and underestimation by the method of flicker. The rating in m. c. by the equality-of-brightness method is plotted along the horizontal coordinate and percentage disagreement along the vertical. The zero line parallel to the horizontal coordinate represents agreement between the two methods. Points above and below this line represent respectively percentage overestimation and underestimation by the method of flicker at the different m.c. values. Agreement between the two methods occurs at the points where the flicker curve crosses the zero line.

THE EFFECT OF SPEED OF ROTATION OF THE DISC ON THE DISAGREEMENT OF FLICKER AND EQUALITY-OF-BRIGHTNESS PHOTOMETRY FOR LIGHTS OF DIFFERENT COMPOSITION AND INTENSITY

By C. E. Ferree and Gertrude Rand, Bryn Mawr College

It has been shown in previous papers[1] that time of exposure is the chief cause of the disagreement between the results obtained by the flicker and equality-of-brightness methods of photometry. If time of exposure is a factor in the disagreement between these two methods, it would seem altogether probable that speed of rotation of the exposure disc would affect the amount and perhaps the type of disagreement. The effect had been noticed in the work in our laboratory long before it was made the subject of systematic investigation. In fact in the present series of Studies this was our first point of attack on the problem.

In making an investigation of this point, a means of producing very small changes of speed of rotation of the disc is required. For this purpose we were provided with a motor specially wound for speed control (500 to 6000 rev. per min.) and a rheostat of the ordinary sliding contact type connected in series with a drum which could be rotated, wound with many turns of low resistance wire. The speed of rotation of the disc was automatically recorded by a small speed-counter attached to the axle of the motor. The general trend of the results obtained was, as in the former Study, checked and verified by 20 *O*s.

For each of the 7 intensities of light mentioned in the preceding paper, the following speeds were used: the speed giving the greatest sensitivity to the method of flicker, one speed lower, and a number of speeds higher than this. For the most sensitive speed and the speeds higher than this, a position or a small range of positions could be found for the standard lamp which gave no flicker; for the lower speed, however, the balance had to be made

[1]The Cause of the Disagreement Between Flicker and Equality-of-Brightness Photometry, this Journal, 35, 1924, 190-208; Flicker Photometry and the Lag of Visual Sensation, *ibid.*, 209-216; The Effect of Varying the Intensity of Light on the Disagreement of Flicker and Equality-of-Brightness Photometry for Lights of Different Composition, *supra.*

in terms of minimum flicker. The sensitivity of the determinations varied considerably, of course, with change of speed. For the most sensitive speed it ranged from 0.20 to 0.68%; for the lower speed from 0.53 to 2.59%; and for the higher speeds from 0.64 to 25.68%.

The results showed consistently: (1) that the rating by the method of flicker varies with the speed of rotation of the disc; and (2) that the flicker and equality-of-brightness ratings were in some cases in closest agreement when the speed of rotation of the disc was low; in others when it was high. The result would obviously depend upon the relation of the rate of rise of sensation for the two lights above and below the point from which the change of speed was made. In much the greater number of cases studied, it will be noted that the closest agreement came with the lowest speed and the longest exposure. If we consider the trend of results very broadly, it may be said that, as the speed is decreased and the length of exposure is increased, agreement is approached irregularly as a limit, the limiting value of exposure being that used in the equality-of-brightness method. The results showed also (3) that the speed which gives the method of flicker its greatest sensitivity does not give the closest agreement in result with the equality-of-brightness method.

The changes in result are quite striking. The details are given in Tables I-III and in Figg. 1-4. In these charts meter-candles as rated by the flicker method are plotted along the vertical coordinate and the length of exposure used, also cycles of the flicker disc per sec., along the horizontal coordinate. For comparison the equality-of-brightness ratings for the different intensities are indicated on the same m.c. scale at the right of the chart. A few of the results may be singled out here. A change in the speed from 11.8 to 14.1 cycles per sec. (a change in the length of the individual exposures from 0.02119 to 0.01773 sec.) for R at 50 m.c. changed an overestimation by the method of flicker from 8.08 to 18.31 m.c., a percentage change from 16.21 to 36.72. A change in speed from 12 to 16.2 cycles per sec. (a change in the exposure from 0.02083 to 0.01543 sec.) for B at 50 m.c. changed an underestimation of 7.34 to 12.67 m.c., a percentage change of from 14.72 to 25.41. A change in speed from 11.9 to 14.8 cycles per sec. (a change in the exposure from 0.2101 to 0.01689 sec.) for G at 50 c.m. changed an underestimation from 6.5[illegible] to [illegible].67 m.c., a percentage change of 13.04 to 25.41. And a change of speed from 11.9 to 14.4 cycles per sec. (a change in the exposure from 0.02101 to 0.01736 sec.) for Y at 50 m.c. changed an overestimation of 9.76 to 21.56 m.c., a percentage change of 19.57 to 43.24.

TABLE I

Showing the Effect of Speed of Rotation of the Disc on the Amount and Type of Disagreement of the Results by the Equality-of-Brightness and Flicker Methods

Color: R ($675m\mu$)

Photometric Rating Equality-of-Brightness Method	Rev. per Sec. Flicker Disc	Length of Individual Exposures	Photometric Rating Flicker Method	Difference between the Two Methods	Sensitivity Flicker Method	Change Produced by Variation of Speed	
m.c.		sec.	m.c.	%	%	m.c.	%
49.86	11.8	0.02119	57.94	+16.21	2.23	10.23	17.66
	12.5	0.020	58.78	+17.89	0.58		
	12.9	0.01938	60.06	+20.46	2.13		
	13.4	0.01866	62.28	+24.91	4.28		
	14.1	0.01773	68.17	+36.72	11.29		
34.03	11.6	0.02155	35.16	+ 3.32	2.16	5.17	14.70
	12.4	0.02016	35.56	+ 4.50	0.67		
	12.9	0.01938	35.95	+ 5.64	2.20		
	13.4	0.01866	37.62	+10.55	5.48		
	14.5	0.01724	40.33	+18.51	16.62		
29.39	11.4	0.02193	29.39	0	2.01	1.54	5.24
	12.2	0.02049	29.39	0	0.61		
	13.0	0.01923	29.68	+ 0.98	2.97		
	13.3	0.01880	30.30	+ 3.09	8.65		
	13.6	0.01838	30.61	+ 4.15	11.31		
	14.2	0.01761	30.93	+ 5.24	13.89		
26.12	11.6	0.02155	23.80	− 8.89	1.81	5.62	23.61
	11.9	0.02101	24.24	− 7.20	1.44		
	12.2	0.02049	24.46	− 6.36	0.53		
	12.9	0.01938	27.12	− 3.83	2.39		
	13.8	0.01812	28.12	+ 7.66	6.37		
	14.3	0.01748	29.42	+12.63	7.38		
25.01	11.8	0.02119	22.36	−10.60	2.59	7.63	34.12
	12.15	0.02058	23.37	− 6.56	0.68		
	12.4	0.02016	25.39	+ 1.52	2.76		
	12.9	0.01938	26.63	+ 6.48	2.82		
	13.3	0.01880	26.89	+ 7.52	4.69		
	13.8	0.01812	27.68	+10.68	8.27		
	14.0	0.01786	28.01	+11.91	8.46		
	15.0	0.01667	29.99	+19.92	9.46		
14.91	11.4	0.02193	13.15	−11.81	0.53	1.61	13.95
	11.8	0.02119	12.72	−14.69	0.39		
	12.4	0.02016	12.55	−15.84	0.64		
	13.2	0.01894	12.22	−18.05	4.42		
	13.9	0.01799	11.76	−21.14	4.84		
	14.8	0.01689	11.54	−22.61	6.07		
12.51	11.2	0.02232	10.40	−16.86	1.83	1.21	13.12
	12.0	0.02083	9.69	−22.54	0.20		
	12.7	0.01969	9.46	−24.37	4.45		
	13.5	0.01852	9.30	−25.65	6.55		
	14.0	0.01786	9.19	−26.53	20.06		
11.91	11.4	0.02193	9.34	−21.59	0.64	0.83	9.72
	11.75	0.02128	9.30	−21.92	0.22		
	12.3	0.02033	9.11	−23.52	0.88		
	12.8	0.01953	8.97	−24.70	2.22		
	14.0	0.01786	8.79	−26.21	5.36		
	14.6	0.01712	8.51	−28.56	7.28		

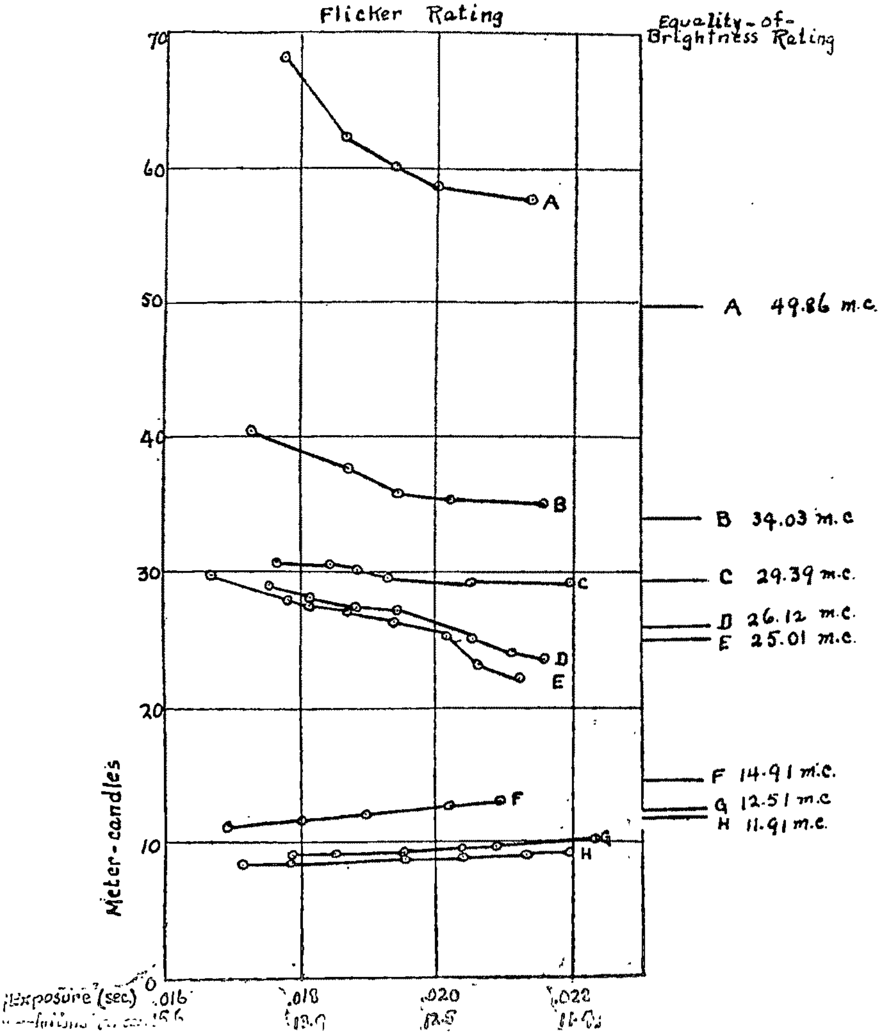

FIG. 1. Showing the [illegible] of the eye on the disagreement of the flicker and equality-of-brightness ratings [illegible]

TABLE II

Showing the Effect of Speed of Rotation of the Disc on the Type and Amount of Disagreement of the Results by the Equality-of-Brightness and Flicker Methods

Color: Y (579$m\mu$)

Photometric Rating Equality-of-Brightness Method	Rev. per Sec. Flicker Disc	Length of Individual Exposures	Photometric Rating Flicker Method	Difference between the Two Methods	Sensitivity Flicker Method	Change Produced by Variation of Speed	
m.c.		sec.	m.c.	%	%	m.c.	%
49.86	11.9	0.02101	59.62	+19.57	2.27	11.80	19.92
	12.6	0.01984	61.74	+23.83	0.58		
	13.2	0.01894	63.21	+26.77	2.04		
	13.8	0.01812	65.13	+30.62	7.10		
	14.4	0.01736	71.42	+43.24	10.16		
29.39	11.9	0.02101	32.59	+10.88	1.66	3.97	12.19
	12.2	0.02049	32.87	+11.84	0.64		
	12.7	0.01969	34.03	+15.78	1.73		
	13.2	0.01894	34.97	+18.98	3.75		
	14.0	0.01786	36.56	+24.39	6.93		
26.12	11.3	0.02212	25.88	− 0.92	1.51	3.75	14.48
	11.9	0.02101	26.12	0	0.57		
	12.2	0.02049	26.53	+ 1.57	3.39		
	12.7	0.01969	27.68	+ 5.97	3.79		
	13.0	0.01923	28.80	+10.26	5.73		
	13.5	0.01852	29.53	+13.06	8.04		
25.01	11.9	0.02101	24.69	− 1.28	1.42	5.96	31.83
	12.2	0.02049	24.02	− 3.96	0.46		
	12.9	0.01938	23.80	− 4.84	2.65		
	13.2	0.01894	21.40	−14.44	6.54		
	13.8	0.01812	19.67	−21.36	9.91		
	14.1	0.01773	18.73	−25.12	13.08		
14.91	10.9	0.02294	12.47	−16.37	1.36	1.29	11.53
	11.5	0.02174	12.27	−17.71	0.33		
	12.0	0.02083	12.14	−18.58	1.24		
	12.8	0.01953	11.83	−20.67	1.83		
	13.4	0.01866	11.46	−23.15	5.41		
	14.3	0.01748	11.18	−25.03	10.37		
12.51	11.2	0.02232	10.15	−18.86	1.87	1.93	20.84
	11.35	0.02203	9.85	−21.26	0.41		
	11.45	0.02183	9.69	−22.53	0.62		
	11.6	0.02155	9.46	−24.37	1.17		
	11.8	0.02119	9.40	−24.85	8.48		
	12.2	0.02049	9.22	−26.29	16.09		
11.91	10.9	0.02294	9.51	−20.16	0.84	0.52	5.78
	11.1	0.02252	9.38	−21.25	0.32		
	11.6	0.02155	9.30	−21.92	3.32		
	11.7	0.02137	9.09	−23.69	4.40		
	12.4	0.02016	8.99	−24.53	11.21		

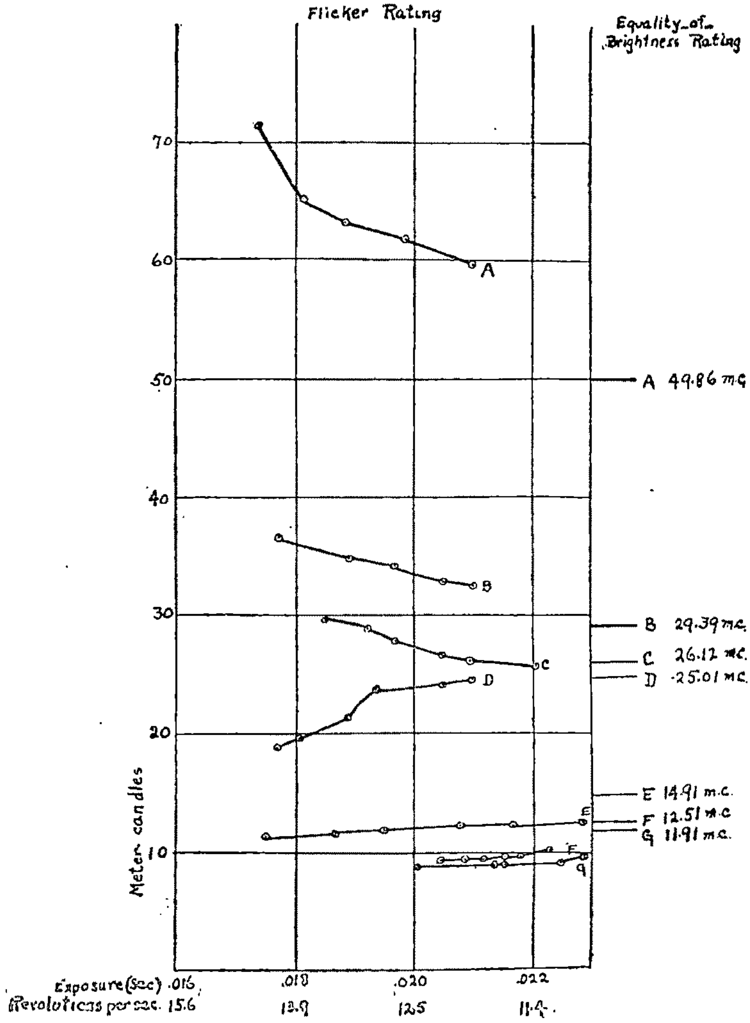
Flicker Rating
Equality-of-Brightness Rating
70
60
50
40
30
20
10
Meter candles
A
B
C
D
E
F
G
A 49.86 m.c.
B 29.39 m.c.
C 26.12 m.c.
D 25.01 m.c.
E 14.91 m.c.
F 12.51 m.c.
G 11.91 m.c.
Exposure (sec) .016 .018 .020 .022
Revolutions per sec. 15.6 13.9 12.5 11.4

TABLE III

Showing the Effect of Speed of Rotation of the Disc on the Type and Amount of Disagreement of the Results by the Equality-of-Brightness and Flicker Methods

Color: G (515$m\mu$)

Photometric Rating Equality-of-Brightness Method	Rev. per Sec. Flicker Disc	Length of Individual Exposures	Photometric Rating Flicker Method	Difference between the Two Methods	Sensitivity Flicker Method	Change Produced by Variation of Speed	
m.c.		sec.	m.c.	%	%	m.c.	%
49.86	11.9	0.02101	43.36	−13.04	1.45	6.17	16.60
	12.3	0.02033	42.01	−15.74	0.47		
	13.4	0.01866	39.86	−20.06	2.31		
	14.3	0.01748	37.62	−24.55	7.55		
	14.8	0.01689	37.19	−25.41	14.20		
29.39	12.0	0.02083	24.69	−15.99	1.82	2.91	13.36
	12.4	0.02016	23.80	−19.02	0.55		
	12.7	0.01969	23.48	−20.19	3.07		
	13.2	0.01894	22.76	−22.56	6.76		
	13.5	0.01852	21.78	−25.89	14.00		
26.12	11.5	0.02174	22.20	−15.01	1.76	3.41	17.63
	11.9	0.02101	21.71	−16.89	0.45		
	12.6	0.01984	21.22	−18.77	2.54		
	13.3	0.01880	21.04	−19.46	4.99		
	13.7	0.01825	19.92	−23.75	11.04		
	14.4	0.01736	19.35	−25.92	11.94		
25.01	10.5	0.02381	21.40	−14.44	2.52	5.71	36.37
	11.8	0.02119	20.72	−17.16	0.34		
	12.35	0.02024	20.51	−18.00	2.49		
	12.75	0.01961	18.46	−26.20	6.23		
	13.5	0.01852	17.86	−28.60	7.45		
	14.1	0.01773	15.69	−37.28	7.89		
14.91	11.3	0.02212	13.70	− 8.10	1.10	1.23	9.86
	11.5	0.02174	13.60	− 8.79	0.37		
	11.9	0.02101	13.51	− 9.39	1.11		
	12.5	0.020	13.33	−10.60	3.30		
	13.0	0.01923	13.06	−12.41	3.90		
	13.6	0.01838	12.72	−14.69	8.17		
	14.0	0.01786	12.47	−16.37	11.95		
12.51	11.1	0.02252	11.96	− 4.40	0.68	1.12	10.33
	11.3	0.02212	11.92	− 4.72	0.34		
	11.6	0.02155	11.72	− 6.31	0.69		
	12.2	0.02049	11.39	− 8.95	3.07		
	12.6	0.01984	11.11	− 9.11	9.27		
	13.4	0.01866	10.84	−13.34	13.29		
11.91	10.8	0.02315	11.99	+ 0.67	1.08	1.48	14.07
	11.2	0.02232	11.91	0	0.34		
	11.4	0.02193	11.79	− 1.01	0.42		
	11.6	0.02155	11.52	− 3.28	1.74		
	11.8	0.02119	11.43	− 4.03	3.50		
	11.9	0.02101	11.25	− 5.54	5.96		
	12.4	0.02016	10.51	−11.76	12.55		
10.20	10.6	0.02358	11.39	+11.66	0.97	1.19	11.66
	11.0	0.02273	11.18	+ 9.60	0.36		
	11.5	0.02174	10.77	+ 5.59	1.21		
	11.8	0.02119	10.51	+ 3.04	2.28		
	12.4	0.02016	10.20	0	6.76		

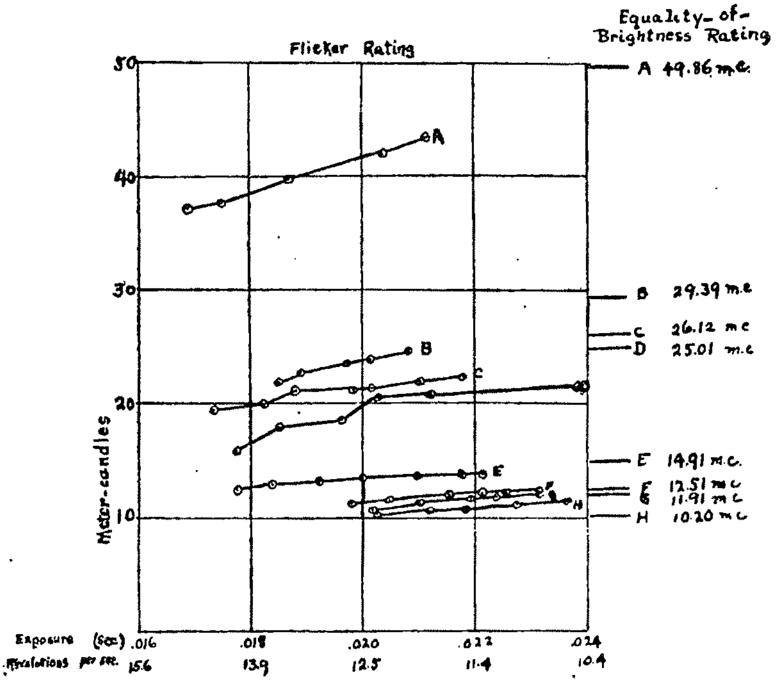

FIG. 3. Effect of speed of rotation of the disc for green

TABLE IV

Showing the Effect of Speed of Rotation of the Disc on the Type and Amount of Disagreement of the Results by the Equality-of-Brightness and Flicker Methods

Color: B ($466m\mu$)

Photometric Rating Equality-of-Brightness Method	Rev. per Sec. Flicker Disc	Length of Individual Exposures	Photometric Rating Flicker Method	Difference between the Two Methods	Sensitivity Flicker Method	Change Produced by Variation of Speed	
m.c.		sec.	m.c.	%	%	m.c.	%
49.86	12.0	0.02083	42.52	−14.72	2.28	5.33	14.34
	12.3	0.02033	41.55	−16.67	0.58		
	12.9	0.01938	41.07	−17.63	1.80		
	13.2	0.01894	40.33	−19.11	3.45		
	13.9	0.01799	39.40	−20.98	4.52		
	14.4	0.01736	38.49	−22.80	9.65		
	16.2	0.01543	37.19	−25.41	22.57		
29.39	11.7	0.02137	22.96	−21.87	1.78	3.87	20.28
	12.0	0.02083	22.55	−23.27	0.53		
	12.4	0.02016	21.87	−25.58	1.28		
	12.8	0.01953	21.48	−26.91	6.10		
	13.3	0.01880	21.22	−27.79	7.30		
	13.9	0.01799	21.13	−28.10	10.64		
	14.3	0.01748	19.09	−35.04	17.82		
26.12	11.85	0.02110	20.24	−22.52	1.19	3.20	18.78
	12.2	0.02049	20.00	−23.43	0.35		
	12.8	0.01953	19.75	−24.40	1.21		
	13.4	0.01866	18.88	−27.73	6.89		
	13.9	0.01799	18.14	−30.56	8.87		
	14.4	0.01736	17.04	−34.78	18.49		
25.01	11.7	0.02137	19.10	−23.64	0.84	3.98	26.31
	12.0	0.02083	18.94	−24.28	0.32		
	12.2	0.02049	18.70	−25.24	0.80		
	12.45	0.02008	18.58	−25.72	2.37		
	13.0	0.01923	17.58	−29.72	3.81		
	13.4	0.01866	17.17	−31.36	4.72		
	13.7	0.01825	16.53	−33.92	9.80		
	14.0	0.01786	15.12	−39.56	25.58		
14.91	11.7	0.02137	15.01	+ 0.67	1.13	1.35	12.89
	12.0	0.02083	14.91	0	0.34		
	12.4	0.02016	14.66	− 1.67	0.69		
	12.9	0.01938	14.66	− 1.67	0.98		
	13.6	0.01838	14.66	− 1.67	4.05		
	14.1	0.01773	14.66	− 1.67	5.25		
12.51	11.4	0.02193	13.60	+ 8.71	1.03	1.20	8.82
	11.6	0.02155	13.79	+10.23	0.36		
	12.2	0.02049	14.18	+13.34	1.27		
	12.8	0.01953	14.45	+15.50	3.11		
	13.4	0.01866	14.66	+17.18	5.25		
	14.2	0.01761	14.66	+17.18	7.44		
11.91	11.6	0.02155	12.89	+ 8.23	1.09	1.39	10.97
	11.8	0.02119	13.20	+10.83	0.23		
	12.35	0.02024	13.42	+12.68	2.01		
	13.0	0.01923	13.53	+13.61	5.76		
	13.6	0.01838	13.89	+16.63	11.45		
	14.7	0.01701	14.28	+19.91	17.15		

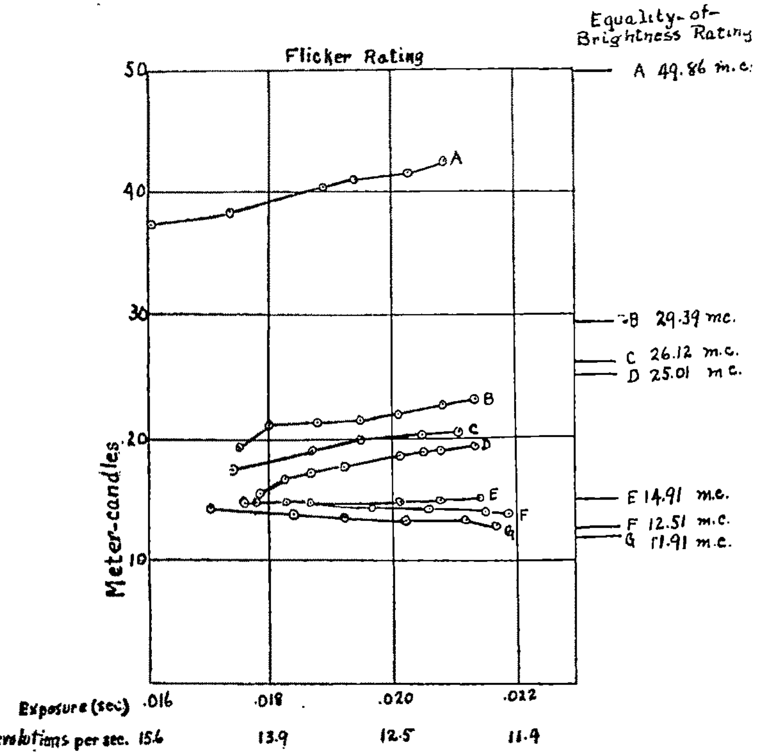

FIG. 4. Effect of speed of rotation of the disc for blue

The results of these Tables for 50, 25 and 12.5 m.c. should be studied in connection with the rise-of-sensation curves[2] for these intensities. However, it is a somewhat difficult task in this case to make even roughly the comparisons needed. That is, while the colors were set to give 50, 25 and 12.5 m.c. by the equality-of-brightness rating, the white light which matches this by the method of flicker at the different speeds would not have been given these ratings. The intensities used, then, for the white light in this Study of the effect of speed are only approximately represented by the curves for the rise of sensation for

[2] Op. cit., The Effect of Varying the Intensity of Light on the Disagreement of Flicker and Equality-of-Brightness Photometry for Lights of Different Composition, supra.

THE AGREEMENT OF FLICKER AND EQUALITY-OF-BRIGHTNESS PHOTOMETRY WHEN THE SAME LENGTHS OF EXPOSURE ARE USED IN BOTH METHODS

By C. E. Ferree and Gertrude Rand, Bryn Mawr College

In the two previous papers[1] it has been shown that the effect of varying the intensity of light and the speed of rotation of the exposure disc on the disagreement of the results obtained by the flicker and equality-of-brightness methods of photometry is just what would be expected from time of exposure as the cause of the disagreement. As a final test whether the disagreement is due to the difference in the time during which the eye is exposed to the lights compared, it was decided to make the comparison using equal lengths of exposure by both methods. That is, the exposure of the eye in the equality-of-brightness method was cut down to equal that used in the method of flicker. These short exposures were given by means of the rotary tachistoscope described in one of the earlier papers of this series[2], provided however with only one pair of sectored discs. The photometric field was the same as that used in the previous equality-of-brightness comparisons. Some apprehension might be felt as to the sensitivity of the equality-of-brightness comparison with these short exposures. The judgment, however, was not difficult, because the saturation of the colors was greatly reduced by the brevity of the exposure, and the brightness comparison was rendered much less uncertain thereby. The sensitivity of the determination ranged from 1.3 to 2.7%.

The determinations were made for these values of exposure at 50, 25 and 12.5 m.c. In every case the flicker rating fell within the small range of values that were called equal in the short-exposure equality-of-brightness judgment. The results of this work are given in Table I. The general trend of these results has been confirmed with several *O*s.

[1]The Effect of Varying the Intensity of Light on the Disagreement of Flicker and Equality-of-Brightness Photometry for Lights of Different Composition; The Effect of Speed of Rotation of the Disc on the Disagreement of Flicker and Equality-of-Brightness Photometry for Lights of Different Composition and Intensity, *supra.*

[2]Flicker Photometry and the Lag of Visual Sensation, this Journal, 35, 1924, 209-216.

It will be seen from the results given in Table I that agreement was obtained between the two methods within the limits of sensitivity of the judgment. In every case the flicker result fell within the small range of values that was judged equal by the equality-of-brightness method. As was stated in an earlier paper, this might well be considered the most important step in the argument as to the cause of the disagreement between the two methods of photometry. It has been added as a final confirmation, not only to show that difference in time of exposure is the cause of the disagreement between the two methods, but also to ascertain whether there was any detectable effect of summation on the disagreement due to the succession of impressions in the method of flicker. Since agreement of result is obtained by the two methods under this condition, it seems reasonable to conclude that no considerable effect can be attributed to summation.

TABLE I

Showing Agreement of the Results by the Flicker and Equality-of-Brightness Methods when the Same Length of Exposure is used in Both Methods

Color	Photometric Rating Equality-of-Brightness Method	Photometric Rating Flicker Method	Length of Exposure Flicker Method	Rating by Equality-of-Brightness Method with Exposures Equal to those used in Flicker Method
	m.c.	m.c.	sec.	m.c.
R	49.86	58.78	0.020	57.48 - 60.05
($675m\mu$)	25.01	23.37	0.02058	23.20 - 23.80
	12.51	9.69	0.02083	9.61 - 10.02
Y	49.86	61.74	0.01984	60.05 - 62.79
($579m\mu$)	25.01	24.02	0.02049	23.80 - 24.43
	12.51	9.85	0.02203	9.66 - 10.19
G	49.86	42.01	0.02033	40.79 - 42.85
($515m\mu$)	25.01	20.72	0.02119	20.28 - 21.18
	12.51	11.92	0.02212	11.68 - 12.20
B	49.86	41.55	0.02033	40.60 - 42.83
($466m\mu$)	25.01	18.94	0.02083	18.65 - 19.44
	12.51	13.79	0.02155	13.67 - 14.17

In concluding this series of articles it may not be out of place to discuss still further two questions which may arise in the mind of the reader.

(1) The determinations for only a small part of a curve for the rise of sensation could be made at a single sitting. How, then, could one put together [illegible] made on different days, and say that the differences found were due to changes in the length of exposure and not to some variations in the condition or sensitivity of the eye? Our reply ...

that, in cases of this kind, where a number of results taken at different times have to be compared with reference to the effect of a given variable, the same type of precaution has to be applied to the use of the eye as is exercised when a sensitivity tester is used in connection with a galvanometer or other physical instrument whose sensitivity is liable to vary from time to time. That is, on resuming the work at any particular sitting, it had first to be determined whether the curve made at the preceding sitting could be picked up or duplicated for a few points back. When this could be done, we felt justified in assuming that the eye had not changed its characteristics of response with reference to the function investigated; and the work was allowed to continue on that day. When the curve could not be picked up, determinations were not made on that day. In no other way than this could curves of the regularity of those given in our paper have been obtained. Furthermore, each value that went into the curve as the increment of time required to produce a just noticeable change in sensation was checked at each sitting by a number of repetitions. Done in this way, not more than 5 or 6 points of the curve were determined in a single day. Few pieces of work in psychological optics require more care than this work has required, and few have received or will receive more care than it has received. We can say quite definitely, then, that we consider these curves throughout definitely established for the *O* in question. However, as we have stated earlier in the series of papers, another *O* or group of *O*s picked at random may be found to show a different type of result. But in case a different type of *O* were found, no conclusion could be drawn pertinent to the problem in hand until his curves for the rise of sensation were compared with his photometric results by the two methods. Curves very similar to ours in general characteristics were determined by M. A. Bills[3] in the Bryn Mawr laboratory in 1917 for lights of a lower range of intensities.

(2) The rise-of-sensation curves show the relative levels attained by the two sensations only during the first exposure given by the flicker disc. Can it be assumed that these relations of level persist after the second, third, etc., exposure? In short, are the relative levels to which the sensations rise in the single exposure modified by the succession of exposures occurring in the use of the flicker method? It was just to answer this question that we planned our last experiment,—a comparison of the results of the flicker with the equality-of-brightness method when using for the equality-of-brightness method a single exposure of the length of the individual exposures occurring in the method of

[3]The Lag of Visual Sensation in Relation to Wave-length and Intensity of Light, *Psych. Mon.*, 28, 101 pp.

flicker. This, it seems to us, should give the answer direct to that question. That is, one of the terms compared was the photometric balance obtained by a succession of exposures; the other, that obtained by a single exposure of the length of the individual exposures occurring in flicker, in accordance with the conditions of the question. If agreement were obtained, it seems fair to assume that the relation of levels attained by the two sensations in a single exposure could not have been significantly altered from any cause *whatsoever* in the succession of exposures. This does not mean, of course, that neither sensation has risen to a higher level in the succession of exposures than it attained in the single exposure. It means only that, in rising, the relation of level has not changed by a detectable amount,—in other words, that the sensation which lags the more in a single exposure has not gained on the other sensation in the succession of exposures. Why should it? If an *O* should be found for whom consistently no disagreement occurs in the two ratings,—and we have never heard that an *O* exists for whom this is true over any considerable range of wave lengths at any intensity of light,—there are, of course, two possibilities of explanation: either that there is no differential lag in sensation for the lights of different wave-length for this *O;* or that there is a compensating, differentially modifying or summating effect on the rates of rise in the succession of exposures. Of these two possibilities our results indicate that the former is by far the more probable. That is, at the intensities at which agreement occurred separately for each of the pairs of lights used, the sensations had risen in the single exposure, so far as could be told, to the same level.

In brief, the comparison of the difference in level to which the two sensations are allowed to rise with the type and amount of disagreement of the photometric ratings for the same *O*, state of adaptation of the eye, etc., for the different intensities of light employed, furnishes strong circumstantial evidence in support of our theory that differential lag for wave-length and intensity is the cause of the disagreement in the photometric ratings. The argument is supplemented and completed by the concordant evidence given by the effect of change in the relative lengths of exposure given to the two lights on the type and amount of the disagreement; by the effect of change of speed of rotation of the flicker disc; and particularly by the fact that agreement was found when the same lengths of exposure were used in the two methods.

THE APPARENT WEIGHT OF COLOR AND CORRELATED PHENOMENA[1]

By Marion Monroe

Introduction

The three experiments described in this Study are concerned with a determination of the factors effective in the phenomenon of "weight" as frequently attributed to color, and with an investigation of the influence of those factors on judgments of aesthetic proportion and on judgments involved in bisecting a line. The problems dealt with may be stated in the form of the following questions. (1) Upon what factor, or factors, brightness, chroma, saturation, relation to background, does the apparent heaviness of a color depend? (2) To what extent do these factors influence judgments of aesthetic proportion? (3) Do these factors also influence one's judgment in bisecting a line?

In mechanical weight, two balls, attached to the ends of a rod, can be balanced on a fulcrum. The fulcrum must necessarily be placed nearer to the heavier ball. The distance between the fulcrum and each ball may serve as an indication of the relative difference between the weights of various pairs of balls. The weight of colors, being a subjective phenomenon, can be determined only by comparisons between pairs of colors as to which color appears to be the heavier. This method does not give an indication of quantitative difference in weight, that is, of how much heavier one member of the pair of colors appears than the other. If, however, two colored circles of equal size were placed at the ends of a line, the judgment as to the place where an imaginary fulcrum would need to be placed on the line in order for the colors to appear balanced in weight would give a means of measuring the difference between the apparent weights of the colors in numerical terms. The values thus assigned to the difference in weight between colors would be comparable for all pairs of colors and for all persons. A judgment could be made also, by using the same line, as to the most pleasing proportion and as to the apparent center of the line.

An apparatus, represented in Fig. 1, was constructed by means of which judgments of this nature could be made conveniently. A brass plate was fitted with narrow brass strips around the sides and lower edge in such a manner as to form a frame for exposing the cards employed as backgrounds for the colors. The cards were held by these strips firmly and securely in place during use, but could be removed easily and replaced when different backgrounds were desired. Three cards were used for backgrounds, *W*, medium *Gr* (grey), and *Bk*, each made from a thin but stiff quality of bristol

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[1]This experiment was conducted in the Psychological Laboratory of the University of Chicago. The writer is indebted to Professor Harvey A. Carr for many helpful suggestions during the experimental work and for advice in the interpretation of the data, and to Dr. E. S. Robinson and Dr. F. A. Kingsbury for critical reading of the manuscript.

board cut 9 by 12 in. In the middle of each card a horizontal line, 20 cm. in length, was ruled in black ink in the case of the *W* and *Gr* cards and in white ink in the case of the *Bk* card. With the extremities of the line taken as centers, two circular holes, 1/2 in. in diam., were punched with a steel die constructed for that purpose. These two circles and the line joining them were of a size convenient to be apprehended as a unit from the position of the *S*. The brass plate with the attached framework for exposing the cards was supported in a vertical position by iron uprights screwed to a table, and adjusted in height so that the figure of line and circles was on a level with the eyes of the seated *S* and at a distance from him of approximately arm's length.

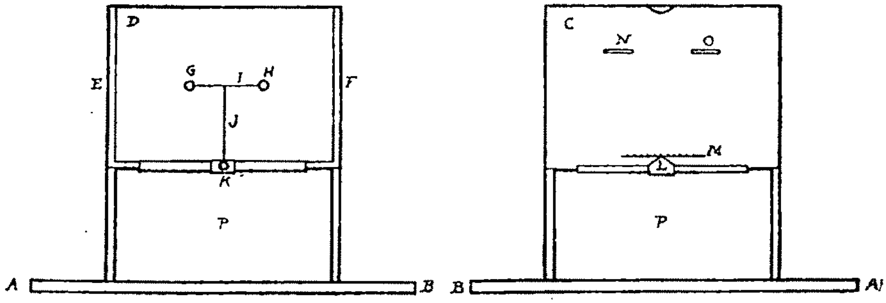

Fig. 1

Front and Back Views of Exposure Apparatus

AB	Top of table	J	Brass pointer
C	Brass plate	K	Handle of pointer
D	Background card	L	Brass finger
EF	Frame	M	Millimeter scale
GH	Circular holes	NO	Slits for exposing colors
I	Ruled line	P	Screen

A small brass pointer, fitting snugly over the card and extending upward until it just touched the ruled line, slid freely back and forth along a brass rod at the base of the frame. A rubber handle was attached to the pointer at its base, and by means of this the *S* moved the pointer to any desired position along the length of the line. Fastened to the lower attachment of the pointer to the rod, a small brass finger extended down under the plate and upward on the rear side to a mm. scale, where it indicated the exact position of the pointer in relation to the center of the line, and thus enabled the *E* to record data out of sight and knowledge of the *S*.

On the back of the plate two slits were milled through the brass at an angle sloping downward but placed slightly above the positions corresponding to the circular holes in the card when fitted into the frame. Strips of colored paper were inserted through these slits and made to lie flat between the card and the brass plate. From the front the papers were exposed through the openings in the card, thus giving the appearance of colored circles at the ends of the line. In this manner, the background card, pointer, line, illumination, distance from *S*, and any possible errors due to [illegible] held constant throughout each series of pairs of colors.

The experiments were conducted in a dark room with artificial light in order to avoid the variability of daylight. A drop-light was suspended above the chair of the *S* at such an angle as to throw the light directly and without shadow upon the colors.

The standard colored papers of the Abbott Educational Company were employed. Three series of paired colors were presented, consisting of *R*, *G*, *B*, and *Y*, varying in brightness, chroma, and saturation. The first series was composed of pairs of colors highly similar in chroma and saturation but differing greatly in brightness. The second series was composed of pairs of colors having approximately the same degree of saturation but varying in chroma. An attempt was made to choose colors in this series nearly equal in brightness; but there was, nevertheless, a varying amount of brightness inherent in each chroma of good saturation which it was impossible to equalize and still to maintain an approximation of equal saturation between pairs of colors. The third series consisted of pairs of colors highly similar in chroma and brightness but differing greatly in saturation. The choice of colors was made by the writer who has had experience with color in the Psychological Laboratory as well as several years' training in the use of color in art schools. While it was impossible to control exactly the factors of brightness, chroma, and saturation, with the pigment colors provided in the assortment, the variable factor in each series was always very great and any difference in the other factors was slight and not very apparent.

The pairs of colors used in each series were as follows. Series I, Brightness difference: Light *R* - Dark *R*, Light *G* - Dark *G*, Light *B* - Dark *B*, Light *Y* - Dark *Y*. Series II, Chroma difference: *R* - *Y*, *R* - *B*, *R* - *G*, *G* - *B*, *G* - *Y*, *Y* - *B*. Series III, Saturation difference: Saturated *R* - Less saturated *R*, Saturated *G* - Less saturated *G*, Saturated *B* - Less saturated *B*, Saturated *Y* - Less saturated *Y*.

All of the pairs of colors from these three series were mixed together at random and a chance order of presentation was drawn so that the pairs of colors did not follow one another in the order of the series just described or have any regular right-left relationship between the members of each pair. After the sequence and right-left positions had been determined by chance, they were kept constant for all *S*s. After judgments for each sequence had been completed, the pairs of colors were presented again 24 hrs. later with a reversed right-left relationship. In this manner any errors due to preferences in terms of position were corrected.

Two groups of *S*s took part in the experiments. The first group consisted of seven graduate students in the department of Psychology in the University of Chicago. The second group consisted of three graduate and four undergraduate students in the same department. There were four women and three men in each group. All of the *S*s were naïve as to the purpose of the experiment. Only two judgments were made by each *S* for each pair of colors, one with the normal and one with the reversed right-left relationship between each pair of colors. For this reason, group results are considered rather than records of individuals. Each numerical value is the average of 14 judgments and is valid only as an indication of the general trend of judgments and as a suggestion of results that might be expected if the values were refined by larger groups of *S*s and a larger number of individual judgments.

The sittings took place on successive days and followed the conditions of the experiments in the following order.

Group I

Sitting 1.	Experiment I, *W* background, normal position
	Experiment II, *W* background, normal position
	Experiment III, *W* background, normal position
Sitting 2.	Experiment I, *W* background, reversed position
	Experiment II, *W* background, reversed position
	Experiment III, *W* background, reversed position

Sitting 3. Experiment I, *Gr* background, normal position
Experiment II, *Gr* background, normal position
Experiment III, *Gr* background, normal position

Sitting 4. Experiment I, *Gr* background, reversed position
Experiment II, *Gr* background, reversed position
Experiment III, *Gr* background, reversed position

Sitting 5. Experiment I, *Bk* background, normal position
Experiment II, *Bk* background, normal position
Experiment III, *Bk* background, normal position

Sitting 6. Experiment I, *Bk* background, reversed position
Experiment II, *Bk* background, reversed position
Experiment III, *Bk* background, reversed position

Group II

Sitting 1. Experiment III, *W* background, normal position
Experiment II, *W* background, normal position

Sitting 2. Experiment III, *W* background, reversed position
Experiment II, *W* background, reversed position

Experiment I

The purpose of this experiment was to discover to what extent the apparent heaviness of a color is a function of its brightness, chroma, saturation, or relation to background.

The following typewritten directions were handed to the *S* at the beginning of the experiment. "Imagine the black line to be a rigid iron bar connecting two heavy colored balls and supported by the pointer as a fulcrum. Move the pointer along the line until the two balls appear to be exactly balanced in weight." In the case of the black background the word "white" was substituted for "black" in reference to the line.

The results of this experiment are given in Tables I, II and III. The numerical values refer to the average difference in mm. between the left and right judgments of the *S*s of Group I for each pair of colors in the various series. The initial letters of the colors are used as symbols to denote the pair of colors for which the judgment is made and to denote the direction of the difference. For example, in Table I, "*DR - LR*" refers to the pair of colors Dark Red - Light Red, and "5.2 to *DR*" means that the average position of the pointer was 5.2 mm. nearer to the Dark Red color when the pair of colors appeared balanced in weight, or that Dark Red was judged to be heavier than Light Red. The greater the amount of difference, the more marked is the apparent disparity in weight between the two colors. The average amount of difference between the right and left judgments is given for each pair of colors on each background.

In the series of pairs of colors differing in brightness, as shown in Table I, the darker colors were judged to be heavier than the lighter colors. The results are consistent throughout the series except in the case of Light *Y* on the *Bk* background. In this one instance the light color appeared to be heavier than the dark color, perhaps owing to the extreme contrast between the color and the background. The background affected the degree of heaviness to some extent, particularly in the case of Dark *R*, which grew progressively heavier from *W* to *Gr* to *Bk* background, and in the case of Dark *Y*, which grew progressively lighter, weighing even less than Light *Y* on the *Bk* background.

In the series of pairs of colors differing in chroma, as shown in Table II, the following order of weight was found, beginning with the heaviest color: *B, R, G, Y*. The results are consistent for this order of weight throughout the series. A close similarity is found between the numerical values of the succeeding steps for colors on the *W* and *Bk* backgrounds. The results on the *Gr* background do not fit so closely into the specific scheme suggested on the *W* and *Bk* backgrounds, but are, nevertheless, in harmony with the general order of weight. It would be expected from this order that the greatest disparity in apparent weight should occur between *B* and *Y*; and this expectation is confirmed by the greatest numerical difference between right and left judgments for this pair of colors. The next largest difference should appear between *R* and *Y*, and *B* and *G*, and the smallest difference should be found between *B* and *R*, *R* and *G*, and *G* and *Y*. The data given in Table II consistently conform to this relationship between the apparent weights of these various pairs of colors. Since the order of weight in the series of chroma difference is the same as the order of the inherent brightness of the

TABLE I*

The Effect of Brightness on the Apparent Weight of Color

Pair of colors Series I	*W* background	*Gr* background	*Bk* background
DR - LR	5.2 to DR	9.0 to DR	11.2 to DR
DG - LG	5.6 to DG	6.4 to DG	3.8 to DG
DB - LB	11.4 to DB	11.8 to DB	11.4 to DB
DY - LY	10.0 to DY	3.6 to DY	2.0 to LY

TABLE II

The Effect of Chroma (or Brightness) on the Apparent Weight of Color

Pair of colors Series II	*W* background	*Gr* background	*Bk* background
B - Y	9.8 to B	12.2 to B	8.2 to B
R - Y	7.2 to R	6.8 to R	6.8 to R
B - G	5.6 to B	11.2 to B	6.8 to B
B - R	4.4 to B	2.6 to B	4.4 to B
R - G	2.8 to R	5.4 to R	4.0 to R
G - Y	2.8 to G	2.2 to G	2.2 to G

TABLE III

The Effect of Saturation on the Apparent Weight of Color

Pair of colors Series III	*W* background	*Gr* background	*Bk* background
Sat R - LSR	5.8 to LSR	5.7 to LSR	0.2 to LSR
Sat G - LSG	3.6 to LSG	1.9 to LSG	3.0 to LSG
Sat Y - LSY	6.6 to LSY	2.9 to LSY	3.4 to LSY
Sat B - LSB	4.2 to LSB	3.8 to LSB	2.2 to LSB

*The values given in this and in the following tables are the average differences in mm. between the right and left judgments of 7 *Ss* making two judgments, one for each position, for each pair of colors.

chromas themselves, *B* and *R* being darker than *G* and *Y*, it is probable that the results of the second series may be subsumed under the results of the first. The results of both series may be summed up in the general statement: *dark colors appear heavier than light colors.*

In the series of pairs of colors differing in saturation, the less saturated color was judged to appear the heavier. The results, as shown in Table III, are consistent throughout the series and for all backgrounds. While the average of the judgments of the group was invariably toward the less saturated color as heavier, individual judgments in many cases favored the saturated color as appearing to have the greater weight. An example of the consistency between individuals in their judgments of the apparent weight of colors differing in brightness or chroma, as contrasted with the variability of judgments of the apparent weight of colors differing in saturation, is given in Table IV. The in-

TABLE IV

Typical Records Showing the Variation between Individuals in Judgments of the Apparent Weight of Color

Pair of colors	*S*	Difference between right and left judgment
Series I	Brown	11.0 to DR
DR - LR	Haffner	2.0 to DR
Gr background	Stratton	4.0 to DR
	Bills	20.0 to DR
	Prophet	14.0 to DR
	Miller	10.0 to DR
	Darrow	2.5 to DR
	Average	9.0 to DR
Series II	Brown	3.0 to R
R - Y	Haffner	7.0 to R
Gr background	Stratton	0.0
	Bills	6.0 to R
	Prophet	9.0 to R
	Miller	12.0 to R
	Darrow	11.0 to R
	Average	6.8 to R
Series III	Brown	16.0 to LSR
Sat R - LSR	Haffner	3.0 to Sat R
Gr background	Stratton	3.0 to Sat R
	Bills	6.0 to Sat R
	Prophet	8.0 to LSR
	Miller	23.0 to LSR
	Darrow	5.0 to LSR
	Average	5.7 to LSR

dividual records here tabulated for a pair of colors in each series are typical of the records obtained throughout each series. In Series I, the *S*s were unanimous in judging the dark colors to appear heavier than the corresponding lighter colors. In Series

II, the records here tabulated indicate that *R* consistently appeared heavier than *Y* except in the case of one *S* whose left and right judgments were identical and cancelled each other. In Series III, however, to three of the seven *S*s the saturated color appeared to be heavier than the less saturated color. With the majority of *S*s, the greater saturation of a color seems to make it appear lighter than the less saturated color. The saturated color appears virile and full of an inner energy that seems to lift it of its own force and make it buoyant in comparison with the "lifeless" and "flat" less saturated color. One *S* particularly had trouble with the saturated *G*. "It goes up like a balloon," she said. "It will not stay down and be weighed."

The relation of the color to the background did not affect the apparent weight of color in any regular manner in either the series of chroma or in that of saturation differences. An increase in brightness of the background decreased the apparent heaviness of Dark *R* and increased the apparent heaviness of Dark *Y* in the series of brightness difference. Since the only variable used in the background was brightness, *W*, *Gr* and *Bk*, the greatest opposition between the brightness of color and background occurred in this series with, consequently, the greatest effect upon the apparent weight of the colors.

As an indication of the comparative effectiveness of the three factors, brightness, chroma (or brightness), and saturation, in determining the apparent weight of color, the average values of each series may be cited.

Brightness: 7.6 to the darker color
Chroma (or Brightness): 6.0 to *B*, *R*, *G*
Saturation: 3.6 to the less saturated color

Experiment II

The purpose of this experiment was to determine whether the factors that influence the apparent weight of color also influence judgments of aesthetic proportion.

The following typewritten direction was handed to the *S* at the beginning of the experiment. "Move the pointer along the line to the most pleasing position between the two colored circles."

The results of the experiment are given in Tables V, VI, VII, and VIII. As in the previous experiment, the numerical values are the average differences in mm. between the right and left judgments of the *S*s for each pair of colors, and the initial letters of the colors are used as symbols to denote the pair of colors and the direction of the preference.

In the series of pairs of colors varying in brightness, the preferred position was consistently nearer to the heavier color. A comparison of the judgments of aesthetic proportion with the judgments of apparent weight in this series is given in Table V. The averages of the values assigned to aesthetic proportion are greater than those assigned to apparent weight with one exception in the case of one pair of colors.

In the series of pairs of colors differing in chroma, there was very little correspondence between the apparent weight of color and aesthetic proportion. There was a tendency to prefer the pointer near to *G* or *Y*, particulaıly on the *Gr* background. One *S* remarked: "It does not look right to see *Y* at the extreme end of the line"; and she put the pointer next to it in order to bring it nearer to the center of interest. Another *S* had a decided dislike for the combination of *R* and *G*. "The only way I can like

TABLE V

The Correlation of Judgments of the Apparent Weight of Color and Judgments of Aesthetic Proportion as Dependent upon Brightness of Color

Pair of colors Series I	Background	Weight	Aesthetic Proportion
DR - LR	*W*	5.2 to DR	12.8 to DR
DG - LG	*W*	5.6 to DG	9.0 to DG
DB - LB	*W*	11.4 to DB	16.4 to DB
DY - LY	*W*	10.0 to DY	11.8 to DY
DR - LR	*Gr*	9.0 to DR	12.8 to DR
DG - LG	*Gr*	6.4 to DG	12.2 to DG
DB - LB	*Gr*	11.8 to DB	12.6 to DB
DY - LY	*Gr*	3.6 to DY	6.6 to DY
DR - LR	*Bk*	11.2 to DR	10.0 to DR
DG - LG	*Bk*	3.8 to DG	8.0 to DG
DB - LB	*Bk*	11.4 to DB	14.8 to DB
DY - LY	*Bk*	2.0 to LY	7.6 to LY

TABLE VI

The Correlation of Judgments of the Apparent Weight of Color and Judgments of Aesthetic Proportion as Dependent upon Chroma (or Brightness) of Color

Pair of colors Series II	Background	Weight	Aesthetic Proportion
B - Y	*W*	9.8 to B	7.6 to B
R - Y	*W*	7.2 to R	8.4 to R
B - G	*W*	5.6 to B	2.2 to G
B - R	*W*	4.4 to B	5.0 to B
R - G	*W*	2.8 to R	2.6 to G
G - Y	*W*	2.8 to G	14.8 to Y
B - Y	*Gr*	12.2 to B	4.6 to Y
R - Y	*Gr*	6.8 to R	2.4 to Y
B - G	*Gr*	11.2 to B	2.8 to G
B - R	*Gr*	2.6 to B	5.2 to B
R - G	*Gr*	5.4 to R	2.6 to G
G - Y	*Gr*	2.2 to G	4.6 to Y
[illegible]	[illegible]	[illegible]	[illegible]
R - Y	*Bk*	6.8 to R	9.2 to [illegible]
B - G	*Bk*	5.8 to B	6.6 to B
B - R	*Bk*	4.4 to B	2.0 to B
R - G	*Bk*	4.6 to R	[illegible]
G - Y	*Bk*	2.2 to G	2.6 to G

them together is to separate them as far as possible." This *S* always preferred the pointer in the center of the line between this pair of colors. The *S*s were influenced in their judgments of aesthetic proportion by the factor of chroma rather than by the factor of the brightness of the chroma, which possibly had operated in the previous judgments of apparent weight. There was not a complete opposition of the influence of chroma upon judgments of aesthetic proportion to the influence of brightness upon the judgments of apparent weight. In about half of the cases, and particularly on the *Bk* background, the direction of the preferred proportion was toward the heavier color. The results of this series are given in Table VI.

In the series of pairs of colors differing in saturation, the position of the pointer was preferred nearer to the heavier color with the one exception of Saturated *B* on the *W* background. A comparison of the values assigned to aesthetic proportion with those assigned to apparent weight is given in Table VII. The

TABLE VII

The Correlation of Judgments of the Apparent Weight of Color and Judgments of Aesthetic Proportion as Dependent upon Saturation of Color

Pair of colors Series III	Background	Weight	Aesthetic Proportion
Sat R - LSR	*W*	5.8 to LSR	0.8 to LSR
Sat G - LSG	*W*	3.6 to LSG	9.2 to LSG
Sat Y - LSY	*W*	6.6 to LSY	0.6 to LSY
Sat B - LSB	*W*	4.2 to LSB	4.0 to Sat B
Sat R - LSR	*Gr*	5.7to LSR	7.0 to LSR
Sat G - LSG	*Gr*	1.9 to LSG	3.4 to LSG
Sat Y - LSY	*Gr*	2.9 to LSY	4.2 to LSY
Sat B - LSB	*Gr*	3.8 to LSB	11.2 to LSB
Sat R - LSR	*Bk*	0.2 to LSR	5.0 to LSR
Sat G - LSG	*Bk*	3.0 to LSG	6.0 to LSG
Sat Y - LSY	*Bk*	3.4 to LSY	10.4 to LSY
Sat B - LSB	*Bk*	2.2 to LSB	7.2 to LSB

values for aesthetic proportion are larger than the values for apparent weight on the *Gr* and *Bk* backgrounds but not on the *W* background. As in the series of brightness difference, there is a close correspondence between the direction of the apparent weight of color and the direction of the preferred proportion.

There is clearly a correlation between the judgments of apparent weight and those of aesthetic proportion in the series of brightness and saturation differences. There is a lack of correlation between the judgments of apparent weight and aesthetic proportion in the series of chroma difference. So far as there is a correlation, the common factors of brightness and saturation operate in both judgments in the same manner. So far as there

is a lack of correlation, the apparent weight of color is probably affected by one factor, the inherent brightness of chroma, while aesthetic proportion is affected by another factor, the chroma, regardless of brightness.

The fact of the correlation might be explained as a result of the direct influence of the apparent weight of color upon the preferred proportion. Since, however, the preferred proportion is not identical with the position of balanced weight but only in the same direction, *i. e.*, toward the heavier color, it is difficult to understand how a direct relationship would operate. That the correlation is due to the similar influence of some common factor in both judgments is a simpler explanation.

TABLE VIII

The Effect of Practice in Judging the Apparent Weight of Color upon Judgments of Aesthetic Proportion

Pair of colors Series I, II, III	Background	Group I (Practised)	Group II (Unpractised)
DR - LR	*W*	12.8 to DR	8.8 to DR
DG - LG	*W*	9.0 to DG	8 to LG
DB - LB	*W*	16.4 to DB	0. to LB
DY - LY	*W*	11.8 to DY	1.2 to DY
B - Y	*W*	7.6 to B	5.2 to Y
R - Y	*W*	8.4 to R	7.4 to Y
B - G	*W*	2.2 to G	1.2 to G
B - R	*W*	5.0 to B	12.4 to B
R - G	*W*	2.6 to G	6.4 to G
G - Y	*W*	14.8 to Y	10.4 to Y
Sat R - LSR	*W*	0.8 to LSR	14.0 to LSR
Sat G - LSG	*W*	9.2 to LSG	20.4 to LSG
Sat Y - LSY	*W*	0.6 to LSY	0.2 to LSY
Sat B - LSB	*W*	4.0 to Sat B	11.8 to LSB

There is a possibility that the close correlation between the apparent weight of color and aesthetic proportion might be due to the fact that the *S*s making the judgments of aesthetic proportion had just previously made judgments of the apparent weight of color and thus had had their attention directed to that phenomenon. It is possible that an attitude toward the colors could result from this experience which might cause the correspondence between the judgments of apparent weight and those of aesthetic proportion. In order to discover the effect of this previous experience and to avoid its influence, the experiment was repeated on a second group of *S*s with only the *W* background. [illegible] in weight as the original group had done, but had previously made judgments in bisecting the line between the pairs of colors. The results of the two groups are given in Table [illegible] [illegible]. The group of *S*s who had not made judgments of the

apparent weight of color had less correspondence between that factor and aesthetic proportion than did the original group. There was a greater variation in individual judgments in the second group than in the first, and a much wider range of numerical values. The group of *S*s employed in obtaining the averages given in the Table was not large enough to mitigate the effects of a few instances where the extreme judgment of a single *S* biassed the average of the group in a manner unwarranted by a consideration of the individual records. The judgments toward Light *G* and Light *B* in Series I are examples of this apparent departure from the general trend of judgments made by the original group. If we consider the records of individuals rather than group averages, the majority of the individual judgments made by the second group showed, nevertheless, the same direction of preference as the apparent weight of color. Practice in judging the relative weights of colors increases the correspondence between the apparent weight of color and aesthetic proportion and slightly decreases the individual variability of aesthetic judgments.

Experiment III

The purpose of this experiment was to discover whether the same factors that affect the apparent weight of color also influence the judgments involved in bisecting a line.

The following typewritten direction was handed to the *S* at the beginning of the experiment. "Move the pointer along the line until it is half way between the two colored circles." The *S* was allowed to move the pointer in only one direction, from right to left. Before presenting the next pair of colors after a judgment had been made, and also in cases where the *S* desired to make a correction of a judgment, the *E* moved the pointer back to the right of the line, placing it at varying positions in order to avoid the *S*'s making a judgment on the basis of the extent of movement.

The results of the experiment are given in Tables IX, X, XI, and XII, in which the numerical values, as in the preceding experiments, refer to the average difference in mm. between right and left judgments for each pair of colors, and the color symbols indicate the direction of the difference.

The judgments of the midpoints of the line were biassed toward the heavier color. This illusion was greatest during the early part of the experiment, that is, while the *S*s were using the *W* background. With practice the illusion tended to disappear, although the *S*s were ignorant of the amount and direction of their errors. On the *W* background, the results, as shown in Table IX, are consistently and invariably biassed toward the heavier color. With the *Gr* and *Bk* backgrounds, as shown in Tables X and XI, the heavier color predominates in the majority of cases. This is probably due to the fact that with practice the *S*s gradually disregarded the colors at the end of the line and became more intent upon the line itself. To verify this practice-effect, the experiment was repeated on a second group of *S*s with

TABLE IX
The Correlation of Judgments of the Apparent Weight of Color and Judgments Involved in Bisecting a Line

Pair of colors Series I, II, III	Background	Weight	Judgment of center of line
DR - LR	*W*	5.2 to DR	0.6 to DR
DG - LG	*W*	5.6 to DG	0.2 to DG
DB - LB	*W*	11.4 to DB	0.5 to DB
DY - LY	*W*	10.0 to DY	0.6 to DY
B - Y	*W*	9.8 to B	0.2 to B
R - Y	*W*	7.2 to R	0.9 to R
B - G	*W*	5.6 to B	0.3 to B
B - R	*W*	4.4 to B	0.1 to B
R - G	*W*	2.8 to R	0.3 to R
G - Y	*W*	2.8 to G	0.3 to G
Sat R - LSR	*W*	5.8 to LSR	1.3 to LSR
Sat G - LSG	*W*	3.6 to LSG	0.1 to LSG
Sat Y - LSY	*W*	6.6 to LSY	0.5 to LSY
Sat B - LSB	*W*	4.2 to LSB	0.3 to LSB

TABLE X
The Correlation of Judgments of the Apparent Weight of Color and Judgments involved in bisecting a Line after Practice on the White Background

Pair of colors Series I, II, III	Background	Weight	Judgment of center of line
DR - LR	*Gr*	9.0 to DR	0.1 to LR
DG - LG	*Gr*	6.4 to DG	0.2 to DG
DB - LB	*Gr*	11.8 to DB	0.3 to DB
DY - LY	*Gr*	3.6 to DY	0.1 to LY
B - Y	*Gr*	12.2 to B	0.6 to B
R - Y	*Gr*	6.8 to R	0.1 to R
B - G	*Gr*	11.2 to B	0.1 to B
B - R	*Gr*	2.6 to B	0.4 to R
R - G	*Gr*	5.4 to R	0.2 to G
G - Y	*Gr*	2.2 to G	0.3 to Y
Sat R - LSR	*Gr*	5.7 to LSR	0.1 to LSR
Sat G - LSG	*Gr*	1.9 to LSG	0.5 to Sat G
Sat Y - LSY	*Gr*	2.9 to LSY	0.2 to LSY
Sat B - LSB	*Gr*	3.8 to LSB	0.3 to LSB

the *W* background. The results of the two groups are given in Table XII for comparison. With the unpractised group of *Ss*, the results of the early part of the experiment were duplicated and the judgments of the midpoints of the line were again found to be consistently biassed toward the heavier color.

There is a [illegible] ments of the apparent weight of color and the judgments of the midpoint of the line. This correlation gradually decreases with an increase in practice on the part of the *Ss* in making judgments of the apparent center of the line. The values of the judgments

of the midpoint of the line are much smaller than the values for apparent weight. The *S*s were very much influenced by the line itself in making the judgment and were able to gauge the middle with considerable accuracy. They were only slightly influenced by the factors of brightness, chroma (or brightness), and saturation. The correspondence between the direction of the judgments of the apparent weight of color and the judgments of the midpoint of the line is so consistent that it is evident that the same factors operating in the judgments of apparent weight of color operate in the same manner but to a less degree in the judgments involved in bisecting a line.

TABLE XI

The Correlation of Judgments of the Apparent Weight of Color and Judgments Involved in Bisecting a Line after Practice on the White and Gray Backgrounds

Pair of colors Series I, II, III	Background	Weight	Judgment of center of line
DR - LR	*Bk*	11.2 to DR	0.7 to DR
DG - LG	*Bk*	3.8 to DG	0.4 to DG
DB - LB	*Bk*	11.4 to DB	0.1 to LB
DY - LY	*Bk*	2.0 to LY	0.9 to DY
B - Y	*Bk*	8.2 to B	0.2 to B
R - Y	*Bk*	6.8 to R	0.4 to R
B - G	*Bk*	6.8 to B	0.4 to B
B - R	*Bk*	4.4 to B	0.6 to B
R - G	*Bk*	4.0 to R	0.1 to G
G - Y	*Bk*	2.2 to G	0.2 to G
Sat R - LSR	*Bk*	0.2 to LSR	0.1 to LSR
Sat G - LSG	*Bk*	3.0 to LSG	0.1 to Sat G
Sat Y - LSY	*Bk*	3.4 to LSY	0.8 to LSY
Sat B - LSB	*Bk*	2.2 to LSB	0.2 to LSB

TABLE XII

Comparison of the Judgments of Two Different Groups of Subjects in Bisecting a Line

Pair of colors Series I, II, III	Background	Group I	Group II
DR - LR	*W*	0.6 to DR	0.9 to DR
DG - LG	*W*	0.2 to DG	0.2 to DG
DB - LB	*W*	0.5 to DB	0.4 to DB
DY - LY	*W*	0.6 to DY	0.3 to DY
B - Y	*W*	0.2 to B	0.8 to B
R - Y	*W*	0.9 to R	1.0 to R
B - G	*W*	0.3 to B	0.7 to B
B - R	*W*	0.1 to B	1.0 to B
R - G	*W*	0.3 to R	0.3 to R
G - Y	*W*	0.3 to G	0.1 to G
Sat R - LSR	*W*	1.3 to LSR	1.3 to LSR
Sat G - LSG	*W*	0.1 to LSG	0.3 to LSG
Sat Y - LSY	*W*	0.5 to LSY	0.5 to LSY
Sat B - LSB	*W*	0.6 to LSB	0.0+ to LSB

Review of Previous Work

Comparatively little experimentation has been done in the field of the aesthetics of color, aside from the analysis of the color preferences of groups of *S*s of different age, sex, and social environment.

The phenomenon of the apparent weight of color was first investigated by Bullough[2], who attempted to test out the "rule" of artists that "the dark colors should stand below the light colors." The reason for this preference he designates as the weight principle, which he thinks is due to an apparent "moreness" of substance in the dark color. More pink added to pink, for example, makes the color pinker, and therefore apparently heavier, because of the increase of pigment. The addition of pigment decreases the luminosity of the color as it increases its apparent weight. The experiment performed consisted of obtaining preferences of *S*s between pairs of triangles, squares, and circles, the upper half of which was filled with one color and the lower half with another color, or a different shade of the first color. The *S* was presented with two forms, alike except for the reversed position of the colors, and was asked to choose the more pleasing of the two and to give a reason for his preference. Many of the *S*s were able to trace their preferences immediately to the "weight principle," particularly in cases where different shades of the same color were employed. More variation was found where colors of different "tones," *i. e.*, differing in chroma, were used. Often the subject was at a loss to account for his preference, in which case a number of possible reasons were suggested to him and he chose the one that seemed most appropriate. The majority of the preferences were made for the form having the darker color at the bottom because of its apparently greater weight, and consequent stability.

The influence of color upon the apparent weight of objects was investigated by De Camp[3], who covered a number of wooden and cardboard cubes of exactly the same mechanical weight in each group with colored papers and obtained judgments between various blocks as to which appeared to be the heavier when lifted. In the first experiment, with wooden blocks, he obtained the following arrangement of the blocks according to apparent weight, beginning with the heaviest: *R, W, O, V, G, P, B, Y, Bk.* The second experiment was performed with very light cubes made from bristol board, in order to obtain the maximum effect of the influence of the colors; since the lighter the object, the smaller would be the appreciable difference n weight, and hence the greater any influence of the apparent weight of the colors. According to the second experiment, the order of the apparent weight of the blocks, beginning with the heaviest was: *Bk, Gr, W,* for one series, and *B, Y, R, W,* for another series. There was no agreement with the rule, "light colors - light weight, dark colors, - heavy weight," although the results of the second experiment are more suggestive of that arrangement than are the results of the first experiment. De Camp concludes from the data of this experiment that "seemingly there is no simple correlation between the affective quality of a color and its influence upon apparent weight." The *S*s were asked also to judge which cubes "looked" heaviest, without actually lifting them, and under these conditions the order of the apparent weight of the cubes beginning with the heaviest, was: *Bk, B, R, Gr, Y, W,* which is in harmony with the results obtained in the present investigation.

The work upon illusions in making judgments in bisecting a line is extensive and generally out of the province of the present investigation. As far as the writer has been able to discover, there has been no study made, previous to this inquiry, of the effect of color upon judgments of this nature.

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[2]E. Bullough, On the Apparent Heaviness of Colors, *Brit. Jour. Psych.*, 2, 1907, 111-152.

[3]J. E. De Camp, The Influence of Color on Apparent Weight, *Jour. Exper. Psych.*, 2, 1917, 347-370.

Summary of Results

(1) The apparent weight of color varies inversely with its brightness.

(2) *B* appears heavier than *R*, *R* heavier than *G*, and *G* heavier than *Y*. This order of apparent weight may be a function of the chroma, or of the inherent brightness of the chroma, or of both.

(3) There is a great variation between individuals in their judgments of the effect of saturation upon the apparent weight of color. In the majority of cases increased saturation increases the apparent buoyancy of the color and makes it appear lighter than the less saturated color.

(4) The relation of color to background affects the relative weights of colors differing in brightness. An increase in brightness of the background decreases the apparent heaviness of Dark *R* and increases the apparent heaviness of Dark *Y*. The background does not affect in any regular manner the apparent weight of colors differing in chroma or saturation.

(5) The factors involved in the apparent weight of color also influence judgments of aesthetic proportion.

(6) There is generally a close correlation between the direction of the judgments of the apparent weight of color and the judgments of aesthetic proportion. The greatest correspondence occurs between pairs of colors having the same chroma but different degrees of brightness and saturation. The least correspondence occurs between pairs of colors differing in chroma.

(7) In the majority of cases the values for aesthetic proportion are greater than the values for the apparent weight of color.

(8) Practice in judging the apparent weight of color increases the closeness of the correlation between judgments of apparent weight and judgments of aesthetic proportion and slightly decreases the individual variability of aesthetic judgments.

(9) The factors effective in the apparent weight of color also influence judgments involved in bisecting a line.

(10) There is a close correlation in direction between judgments of the apparent weight of color and judgments of the midpoint of the line. Judgments of the midpoint of the line are biassed toward the heavier color.

(11) The values for the apparent weight of color are much larger than the values for the midpoint of the line.

(12) The correlation between the apparent weight of color and the judgments of the midpoint of the line tends to decrease with practice.

A STUDY OF THE EFFECT OF HYPNOSIS ON A CASE OF DISSOCIATION PRECIPITATED BY MIGRAINE[1]

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We feel warranted in reporting cases of this type because of their unique contrast to, and radical deviation from, the normal tendency. But, as will be gleaned from the studies of Freud, Prince and Janet, the phenomena of dissociation, amnesias, somnambulisms and fugues are in the main exaggerations of normal tendencies. Every normal individual forgets names, experiences and events during the course of his life. At times he may even have slight amnesias, sleep-walking episodes, and periods of absentmindedness, all of which are miniature forms of the more exaggerated disorders such as complete dissociation, amnesias, fugues and somnambulisms. The significant difference between the normal and the abnormal in this connection seems to lie in the symptoms manifested rather than in the mechanisms leading up to those symptoms. The mechanism involved seems to be the common element in both normal and abnormal, and the difference to be one of degree rather than of kind.

History. The patient's family history is of little concern, except that his mother has had migraine for many years. The patient's father and mother lived together only a short time after his birth. When the patient was born he was nearly blind, and did not open his eyes until the eighth day. This blindness was caused by the mother's previous attack of smallpox. At the age of 7 the patient had the measles, and two weeks later be became paralyzed. He could not move his head, arms or legs, and at times lost his speech. He did not have much control of himself for a year after this attack. At about the age of 14 he was thrown from a load of hay, struck his head against a post, and fell to the ground; but he soon arose, ran out into the field, and seemed to lose all reason. He had to be held in bed the following night, and behaved queerly for two or three days. One year after this accident he wandered away from home, became homesick within a few days, and his mother sent him money to come home.

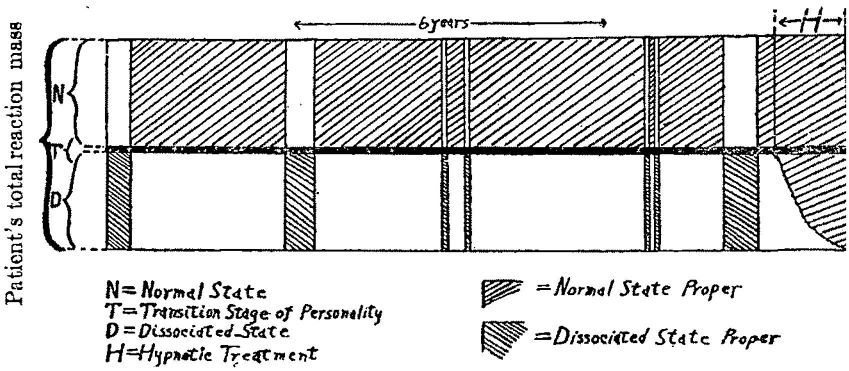
The patient recalls having had several sleep-walking episodes when he was about 13 years old. These episodes were not of the recurring type, but they are very vividly recalled. He has also had *pavor nocturnus* or night terrors in his childhood.

The patient enlisted in the army when he was 16; and after being transferred to two camps, he had an attack of hemiplegia or paralysis over the entire left side. The paralysis struck him while on the drill field; he suddenly fell to the ground in a sort of faint, and was unconscious for a short time. He could move none of the muscles of the left side and was entirely anaesthetic

[1]The case reported in this paper is a young man, twenty-two years of age' who came to the State Psychopathic Hospital in a state of anxiety and worry over his recent episode of amnesia and dissociation.

over the same side except on the skin of his face and neck and a small portion of the upper abdomen. He was given good medical attention during the paralysis, which endured about two months. The medical officers decided to send him to Plattsburg, N. Y., for electrical treatments. Upon hearing this he began to improve immediately, and within a day or two his entire left side began to tingle and felt as though needles were pricking him. The tingling began in the ends of his toes and fingers and worked medially. Within two days after the initiation of the tingling he was able to walk around, although he felt rather stiff in his legs and arms.

Graph showing relative extent of the periods of dissociation in comparison with the normal periods between the attacks. The attacks extend over a period of six years



The patient has had 7 episodes of dissociation and amnesia extending over a period of 6 years with durations as follows: two months, three months, seven days, five days, eight days, three days, and four months. The accompanying graph shows the relative extent of the periods of dissociation in comparison with the normal periods between the attacks.

The first attack occurred when the patient had been transferred back to his home camp and was waiting to be discharged. He left the camp one day in uniform, and two months later came to himself in a distant city. How he arrived there, or why, he did not know. When he came to himself he had no money, and after finding out where he was he immediately telegraphed home for money. He remembers having had a severe headache before he left the camp and when he came back into the normal state in the distant city.

The second episode occurred while he was in the navy. He did as before; just wandered away from the camp with a severe headache, and after three months traveling, beating his way on trains, working at odd jobs, he awakened to find himself 800 miles from the naval station in civilian clothes. He had no money, and again telegraphed home for money. He was punished severely for having deserted from the navy, but gave no excuses for having left.

The next four episodes were of short duration, and occurred when the patient was at home and after his discharge from the navy. They show the characteristics of the other two with the exception that they were a great deal more confused.

The last or seventh episode of dissociation occurred after the patient had enlisted in the marine corps. He had worked up to the position of first class private, and apparently was enjoying his work immensely, when he was

suddenly struck with a severe headache. He went a few blocks to the "sick barracks" with his friend to get something to relieve his headache,—and this is the last he remembers in his normal state. He went from here to Washington and traveled from city to city in the eastern and central states, beating his way on trains, in autos, sometimes walking. He became an expert window cleaner, and this was his occupation when he remained for any length of time in a city. He finally came back to his normal state, after four months of wandering from city to city, in Windsor, Canada. He had a strange feeling that he was lost, and immediately oriented himself as to time and place and mailed a card to his mother saying that he would be home immediately.

After the patient's arrival at home he was forcibly impressed by the duration and possible consequences of his desertion from the marine corps. This was the condition in which he came to the hospital for treatment.

Physical and Mental Examination. The patient has always been an alert, energetic and athletic type of individual in his normal state. He has enjoyed good health since childhood with the exception of a terrific headache of irregularly persistent occurrence closely resembling migraine. The headache comes on instantly, and usually endures from 10 to 24 hours. It starts in the left frontal region of the head, and the pain shoots backward, at times becoming so intense that the patient says he cannot think. The facial artery throbs and is very much distended during the headache. The left eye and the left side of the head and the back of the neck feel sore after the headache has subsided. The patient sees occasional blotches and specks of light during the more intense part of the headache. Any kind or amount of medicine seems to have no effect on the headache. It runs its own course as far as the effects of drugs are concerned, again resembling migraine in this respect.

These headaches always accompany the transitional stages between the normal state and the state of dissociation. The headaches seem to act as a precipitant in the transition from one state to another. The headaches may occur and do occur, however, without the patient having any change in personality; but the transition from one state to the other never occurs without the accompanying headache.

The patient's physical examination showed nothing pathological. His spinal and blood Wasserman's tests were both negative. The X-ray of the head showed nothing abnormal. All reflexes were intact and normal.

The patient has had but an eighth-grade education, although his I. Q. was 106 on the Stanford Revision of the Binet Scale. This means that he has normal intellectual capacity, within the reliability of this test. He has a very definite *Wanderlust* characteristic, and has been over a great deal of territory for a man of his age. This trait is a great deal more pronounced during his state of dissociation, and he remained in one place but a short time,—a few weeks.

The patient passes from one state to the other seemingly in the midst of a terrific headache. He seems to come out of his dissociated state when he has run out of funds, when the weather is too cold for his slight clothing, when he is "up against it," so to speak. He seems to go into the dissociated state when he can stand no more of the domineering attitude of his officers, when he has a sudden impulse to see the world, when he wants to be free from all responsibilities.

When the transition from the dissociated state to the normal [illegible] place, he is very hungry, and has little or no money on his person. He describes his feelings as those of a person just waking from a dream. He feels lost and cannot realize where he is. He feels that he is in a strange country, and shouldn't be there. Then he makes haste to orientate himself, then

thinks of his mother and wonders if she is still alive. This prompts him to write home immediately, saying that he is coming home on the next train. He is ragged, and would pass very adequately for a tramp.

Treatment. The writer worked with the patient three weeks getting at the details of his history in regard to his physical and mental condition and his episodes of dissociation. In so far as his states of dissociation did not enter, to cause periodic and persistent blanks in his memory in his normal state, he recalled experiences as readily as any normal individual. But at the end of those three weeks the patient could recall nothing that had happened in the state of dissociation. He was becoming very much concerned over the whole matter. He saw more than ever before what such episodes meant to his well-being and success in life; but he could do nothing to prevent them; and he had, up to a few days before coming to the hospital, kept his amnesias, as such, a secret to himself, letting his relatives think what they might of his previous actions.

During these three weeks little or nothing was being accomplished by way of psychoanalytic attack. It was then decided to try hypnosis. After the third sitting in the hypnotic state the patient was able to recall almost in detail everything that had happened in his periods of dissociation even after he was awakened into the normal state. The results of the hypnotic treatments will be given in more detail.

The patient's willingness to submit to hypnosis was no doubt built up as the confidence increased between the writer and the patient during the three weeks of note taking. It required about 10 min. to get the patient under rather deep hypnosis. He was then asked to repeat everything that had happened to him in the past. He immediately told of his trip to Windsor; how he had come over on the ferry from Detroit that noon, that it was now 2 p.m., that he was now in Windsor, and that he expected to go back to Detroit Monday morning, for he had a job at cleaning windows in Detroit. He said he had a headache. He was asked to give his name, which he gave as Earl ———, an entirely different name from the one in his normal state. During this sitting and the next three sittings the major portion of the travelings and experiences of the patient during his states of dissociation were worked out and were connected up into a meaningful whole.

The hypnosis reinstated the dissociated state and brought the patient up to the last transition into his normal state. This is somewhat similar to the case in which Janet produced an artificial state by hypnosis in his patient Marceline.[2] Janet made Marceline think she was a well and happy girl again. But in the case under discussion, the patient went automatically, it seems, into the other state under hypnosis. The state could not be reproduced artificially, but could only be reinstated. While in the dissociated state induced by hypnosis, the patient could recall nothing that had happened in his normal state; and after the first hypnotic sitting he could recall nothing that was asked him or anything that he had said in the hypnotic state, or anything that had happened during his previous amnesic episodes.

A rather unusual and surprising discovery was made after the second hypnotic sitting. The patient could recall everything that had happened in his amnesic episodes and could connect it up with the rest of his previous experiences, but he could recall none of the questions asked him or the answers he gave. The process of hypnosis seemed to link the two states together, with the exception that the periods of transition from one state to the other were still a perfect blank whether in his normal state or under hypnosis.

In the third hypnotic sitting the patient was again in the dissociated state. He had a different name for each state, and while in the dissociated

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[2]P. Janet, *Major Symptoms of Hysteria*, 1907, 86-92.

state could not recall his name of the normal state, his previous marriage, his life in the army and navy, his parents, his birthday (except that he was twenty *plus*), his home, any of his previous friends, or anything that had happened in his normal state. But he seemed to transfer the ability to make adjustments over into the dissociated state; for he beat his way on trains, worked at odd jobs, got into fights, and had normal sex-relations with women. While in the normal state he is a very good roller and ice skater, and can play football and many other athletic games; but in the dissociated state he cannot skate, play football, or any other game, can fight when he has to, and enjoys cleaning windows at great heights.

Nothing but a few more details was obtained in the fourth hypnotic sitting. In the fifth hypnotic sitting, the patient was hypnotized for the first time in his normal state. While under hypnosis this time he could recall nothing concerning the amnesic state. He was oriented as to time and place and could remember his experiences of his normal state. It was then suggested to the patient that he go into the other state and recall the experiences of the other state; this he did quite readily. It was then suggested that he concentrate upon the transition stage, especially the one previous to his last amnesic period. This occurred, it will be recalled, when the patient left the marine corps and went to Washington. It was tried very assiduously to get the patient to recall what had happened in several of the transitional periods, while he was hypnotized in the normal and amnesic states, but he could recall nothing. Everything was a perfect blank. It was repeatedly suggested that he would dream about the time that he went from the marine corps to Washington and that he would finally be able to tell everything that happened on that trip. (He was under hypnosis when these suggestions were made.) After about 10 min. of directed suggestion, the patient was asked to tell of his trip from the marine corps to Washington. He said he had no trip. He was asked "Where are you?" "I don't know." "Where were you yesterday?" "I don't know." "How do you feel?" "I have a headache." "What are you thinking about?" "Nothing—only my headache; it's hurting so bad I can't think." This was all that the patient could do in this state. Apparently he was in the transitional stage, and was totally disoriented as to time, person and place. His facial expression showed every evidence that he really had a severe headache. The patient was awakened directly into the normal state after having been in three states under hypnosis. He said he felt "all in" when he was brought out of the hypnosis. He was perspiring to excess, and felt as if he had done a hard day's work.

The patient had a headache for 2 hrs. immediately after the latter hypnotic sitting. This was probably caused by his being awakened directly from the transitional stage (the period when he has a very severe headache.)

The patient was somewhat resistant to being hypnotized again, because, he said, the effects of the last sitting gave him a headache; but he yielded to hypnosis after a little urging. This time it was possible to get the patient to recall all his experiences while under hypnosis, whether they were experiences of the normal state or of the state of dissociation. The transitional stages, however, still remained a blank. Several more hypnotic sittings revealed nothing in addition to the previous sittings, except that some of the details were ironed out more thoroughly.

In this case the experiences of the dissociated state have been properly connected up through a process of hypnosis with the experiences of the normal state. The transitional stages are still more or less a blank; but, from the facts obtainable on this period, the patient functions more nearly on a vegetative level, partially if not wholly unconscious of his surroundings.

The question arises whether the hypnosis has been adequate to the proper integration of experiences of the entire personality. Sidis and Goodhart[1] have said that "hypnosis alone is not sufficient to effect a synthesis of

two dissociated moments.[3] They quote James' description: "the waking state could give nothing else but Mr. Bourne's personality, the hypnotic could give only Mr. Brown's personality. By means of hypnotization, therefore, the two dissociated moments could not possibly be unified in one synthesis.". In reference to the case under discussion, the above statement is partially if not wholly refuted, or else our case is a milder form of dissociation.

Prince also states, as a general principle, "that from a dissociated state to the full waking state, it is commonly found that there is amnesia for the previous state."[4] In connection with our case the hypnotic state itself was amnesic to the patient in the waking state, but through hypnosis the dissociated state was linked to the normal waking state to the extent that the patient's recollections of his experiences of the dissociated state were as vivid as those of his normal state. This case contradicts Prince's principle.

Summary. This is a report of a case of dissociation that might well be called a double personality. Treatment by hypnosis revealed the character of the second personality and its relation to the first personality, and some of the conditions necessary for the transition from the one state to the other.

In the first few hypnotic sittings the patient seemed to go automatically into the dissociated state. After repeated hypnotic suggestions the patient was able to recall all his experiences of both the normal and the dissociated states, either under hypnosis or in the normal waking state.

The transition-stage between the normal state and the dissociated state was always accompanied by a severe headache which closely resembled migraine in its symptomatology. The headache seemed to act as a precipitant in the transition from the one state to the other.

It was not only possible to hypnotize the patient in the normal state and in the dissociated state, but in the transition state as well. Here the patient seemed to be in a sort of stupor, thinking only of his headache, and totally unconscious of his surroundings. It was tried to superimpose other artificial states upon the personality of the patient while under hypnosis, but no sign whatever of a different state from the three already mentioned was elicited.

The following is a summary of the patient's attitudes in the three states. (1) The Normal State. The patient feels that he has seen enough of the world, and that he ought to get an education and make something of himself. He feels that he can work up to a responsible position quite rapidly, and has done so as a matter of fact. He had been a first class private in the marine corps, in charge of a boiler room in the navy, and assistant manager in a fibre company. He does not enjoy working at common laboring jobs, and especially abhors the window-cleaning pro-

[3]B. Sidis and S. P. Goodhart, *Multiple Personality*, 1905, 376-382.
[4]M. Prince, *The Unconscious*, 1914, 62.

fession, which he thinks he could not work into without some practice at the trade. He has always had an ambition to see the world, and likes immensely to travel. He likes to save his money although he has been quite a gambler in the army and navy. He is very fond of athletics, and is himself a very good athlete. He does not care much about women, but would rather be with men. He does not believe in taking chances as he did in the otherstate, that is, in getting out on a window ledge about five stories high. He enjoys going to church, and is affiliated with a part cular Church. He desires to have a home of his own with chilidren. He does not care for strong drink, although he has drunk somewhat with his gang. He likes the army and navy life very much. (2) The Dissociated State. The patient wants to be free from all responsibilities. He feels care-free and irresponsible. He feels perfectly at home in any strange city. He cannot take orders from other people, and says that he does not have to, because he can get along in this world without bending to other peoples' desires. He despises army and navy life because one has to take orders and cannot talk back to the officers, and one cannot do what one wants to. He is an expert window-cleaner and enjoys the work very much; he gets a great thrill in going out on ledges or in dangerous places. He likes to wander from place to place, and can remain in one place but a short time. He has an ambition to see the world, and would rather travel than do anything else. He feels quite contented when he is traveling. It does not matter how he travels, on the train, riding the blinds, on a freight train, or catching rides in autos. When he arrives in one town he wonders what is in the next town and must go there at once to satisfy his curiosity. He likes to drink quite well. He likes men as companions better than women. He cannot skate or play football. (3) The Transition State. One preponderant idea seems to dominate his consciousness, that he has a terrific headache. He thinks of nothing else. He is disoriented as to time, place and person. He appears to be on a stuporous vegetative level, unconscious of his surroundings.

When the patient was discharged from the hospital he was feeling quite elated over the fact that he could now connect all his previous experiences into a meaningful whole.

AN EXPERIMENTAL STUDY OF MENTAL AND PHYSICAL FUNCTIONS IN THE NORMAL AND HYPNOTIC STATES[1]

By Paul Campbell Young

I. Historical and Critical Summary[2]. Past experimentation in the field of hypnotism has been, on the whole, successful in establishing a symptomatology of this form of suggestion, *i. e.*, a description of what the hypnotized person *will do* in hypnosis that he *will not do* in normal consciousness. On the other hand, for lack of a strictly comparative method, it has not been highly successful in establishing the capability-differentia of hypnosis, *i. e.*, what the hypnotized person *can do* in hypnosis that he *can not do* in waking. The methods in the past have been of three kinds. (1) *Non-scientific investigation, i. e.*, investigation without definite control-conditions. With this method all things were possible[3]. (2) *Avowedly scientific investigation* of hypnosis as an isolated phenomenon, *i. e.*, with control-conditions in hypnosis, but without a comparable investigation of the same persons in ordinary waking consciousness. All 'scientific' investigation of this sort is vitiated by the fact that the normal powers have been grossly underestimated, and that the possibility always remains that the very persons who showed such remarkable powers in hypnosis could have done just as well in the normal state if they had been given to understand that fully as much was expected of them. In fact, if one begins to recite wonders because they are wonders, one need not go to hypnosis for even a thousandth of one's cases[4]. (3) *The comparative method, i. e.*, comparing the

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[1]This paper is a condensation of a doctor's thesis presented at Harvard University in May, 1923. Because few readers will be interested in the mass of details connected with the investigation referred to in this paper, and because those who are so interested may consult the complete thesis in the Widener Library at Harvard, or may borrow a duplicate copy from that library, much matter which appears in the original thesis has been omitted from this article. At this time the author is interested mainly in setting forth the results of this investigation, which is, so far as he knows, the most extensive experimental study of hypnotism ever reported.

[2]A. Moll, *Hypnotism*, trans. from the 4th Ger. ed., 1909; J. Grasset, *L'hypnotisme et la suggestion*, 4th ed., 1916; L. Lowenfeld, *Hypnotismus und Medizin*, 1922. Any of these three books gives a good account of hypnotism *qua* hypnotism.

[3]Cf. Bourru et Burat: *Revue phil.*, 21, 1886, 311.

[4]For a feat almost as spectacular as that of Bergson's boy subject (*Revue phil.*, 22, 1886) see article to appear later on in regard to discriminating postage stamps in the normal as well as in the hypnotic state.

performance of *S*s while they are hypnotized with the performance of the same *S*s while they are in the normal state, under exactly similar conditions (except for the matter of hypnosis). This method has been consistently used up to the present time in only one series of experiments: that of N. C. Nicholson of Johns Hopkins University[5]. All other investigations which were meant to be strictly comparative in method are open to one or all of the following criticisms. (*a*) Often the comparison has been a desultory, not to say haphazard matter of a few trials in waking and a few trials in hypnosis, without any standardization of conditions. On the very face of them, most of the reports of comparative studies are worthless as scientific data. (*b*) There has been doubt as to what state of consciousness was being investigated. (*c*) The number and order of the experiments, the number and character of the persons acting as *S*s, and the exact technique in the hypnotic and waking session have not been fully published along with the results. This criticism holds for the work of N. Ach[6], E. Trömner[7], A. Chojecki[8], and to an extent of all previous experimenters. (*d*) There have been too few *S*s, often of a doubtful, neurotic kind; too few sessions; too limited a field of investigation. The studies of Claparède[9] and others suffer from these limitations. (*e*) The patent fact of wide variations in the successive performances of any given task by a normal *S*, even in waking life, has never been taken into account in interpreting the results of previous investigations[10]. It is a gratuitous assumption, in the face of all the possible factors of chance-bodily and mental states, *e. g.*, factors like mood, interest, tiredness, training under hypnosis, stage fright, etc., to select one factor, that of being awake or being hypnotized, as the cause of the differences (regardless of the direction in which the differences run!) which have been found in the two series of tests.

II. Present Status of the Problem. In the present research the following questions were dealt with. (1) Is there a real basis for the division of states of hypnosis into those that are active and those that are passive? Preliminary experimentation and the results of the 15 detailed experiments described below seek to answer this question. (2) Quite aside from the question of the ability to withstand fatigue, is there a difference in muscular capacity and nicety of adjustment? Experiments 1, 2 and 3

[5]Nicholson's work comes up later on in this article; 13 *infra*.
[6]Ueber geistige Leistungsfähighcit u.s.w., *Zeits.f. Hyp.*, 9, 1899, 1.
[7]*Journ. f. Psychol. u. Neur.*, 17, 191[illegible], 307-433; 19, 19[illegible], 2[illegible].
[8]*Arch. de Psy.*, 12, 1912, 61-67.
[9]E. Claparède et W. Baade, Recherches expérimentales, etc., *Arch. de Psy.*, 7, 1909, 300.
[10]For an example of wide variations see p. 221 *infra*.

bear upon this question. (3) Is sensitivity, *e. g.*, of the skin, greatly different in the two states? Experiment 4 deals in rough fashion with this problem. (4) Do the strictly mental functions involved in memory and perception vary consistently? This question received major attention in this research, and is taken up in all the experiments from No. 5 to No. 15, inclusive.

III. Summary Statement of the Methods used in the Present Investigation. A. Plan of this Series of Experiments. The purpose of this research was to put a large number of *S*s through waking and hypnotic performances of the same simple tests, under as nearly identical conditions as possible, in order to determine the relative capacity of the same persons in the two states. The first requirement was to find suitable *S*s; the second, to choose tests which should give comparable results in a relatively small number of sessions; the third, to establish such conditions and methods as would make for maximum performance in each state without being prejudicial to either; the fourth, so to treat the data obtained that the comparative performance would be apparent at a glance.

B. Comparison of the Present Methods with those of Past Experiments. The conditions of this research differed from those of previous ones in the following ways.

(1) In the number of *S*s and their range in the hypnotic state.

(*a*) *Number.* Altogether 22 *S*s, 16 hypnotic and 6 control *S*s, took part in these experiments. On the average, there were 12.9 hypnotic and 3.2 control *S*s for each of the 15 experiments. It is thus seen that all the *S*s did not take part in every experiment.

(*b*) *Status.* The *S*s were graduate and undergraduate students of Harvard University, and were palpably normal persons. Since they were all volunteer *S*s, they were not dependent on the results of the experiments for any return, either financial gain or scholastic credit. Only one had ever been hypnotized before this year. None had been coached in the classical hypnotic mental attitudes or moulded in regard to matters like contracture, rapport, or post-hypnotic amnesia; hence they could furnish fresh evidence on these points.

(*c*) *Classification.* As a result of preliminary investigations, all those of the original *sixty* volunteers who had difficulty in attaining a rather stable depth of hypnosis were rejected. Those retained (except the 6 control *S*s, who could not be hypnotized at all) were easily hypnotized, and each went into what, for him, was a characteristic depth of hypnosis. During the preliminary experimentation, using as a basis the *S*s' behavior, the *S*s were divided into the following classes:

(i) Six control *S*s, who were not hypnotizable, but who went through the whole series of experiments twice, just as the hypnotized *S*s did, once "awake" and once in "pseudo-hypnosis." During the pseudo-hypnotic session the control *S*s received all the suggestions that put and kept the hypnotic *S*s asleep, and were treated in every respect as though they had been hypnotized. (ii) Six light hypnotic *S*s. (iii) Six deep hypnotic *S*s.

(iv) Four somnambulistic *S*s.[11] The deep hypnotic *S*s were further classified into two groups: (1) those that were *alert*, active, and that would, if they went deeper yet, go into somnambulism, *i. e.*, into a clear-cut dissociation; (2) those that were *lethargic*, dull, and that would, if they went deeper still, fall into the most profound natural sleep. All the somnambulistic *S*s are alert, however sleepy-eyed they look, and can be made to do, in hypnosis, almost anything that they can do in ordinary life, without any danger of waking up. The light hypnotic *S*s do not go far enough in hypnosis to enable one to tell whether they are inclined to pass into somnambulism or into sleep.

Heretofore, there has been no comprehensive grading and classification of *S*s, the number of whom, when specified, has varied from one to seven[12] *S*s.

(2) In the use of control *S*s.

So far as is known to the writer, this very ordinary precaution for any scientific experiment of this sort has never been taken before. As an objective check on the probability of variations from test to test with such a series of experiments and on the effect of suggestion, both matters quite apart from hypnosis, the presence of control *S*s was indispensable.

(3) In the number of different kinds of tests used.

The advantages in using 15 different kinds of tests in this research are patent. Heretofore the most varied set of experiments was that of Claparède, with tests of memory for nonsense syllables, of adding, of reaction times, and of association times.

(4) In the use of sense material as contrasted both with nonsense and with rote material for testing memory.

All experiments made previously, that had measurable results, depended on either rote material or on nonsense syllables.

(5) In the number of sessions.

For the investigations reported in this paper there was a total of 252 sessions for the 22 *S*s, 126 sessions in hypnosis and 126 sessions in the normal state. This is an average of 5.25 sessions for each *S* in each state of consciousness. In each of the 15 experiments (and in both parts of Experiment 10) there were on the average two sessions for the control *S*s, and 2.4 sessions for the hypnotic *S*s in each state. These figures are exclusive of the preliminary sessions (at least 3 with each *S*).

Although in individual tasks previous *E*s have reported even more sessions than were given to individual problems in the present research, the advantage in the present case is that the number of tasks and the number of experiments make for a large total of sessions.

(6) In the total time spent in experimenting.

In the present research 234 hours, 117 hours for each state, were spent in actual experimenting in obtaining the material of the 15 experiments. This amount of time is exclusive of time spent in preliminary sessions. All of

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[11]The light hypnotic *S*s correspond fairly well to Wingfield's stages 1 and 2, the deep to his stages 3 and 4, the somnambulistic to his stages 5 and 6. In criticism of Wingfield it must be said that post-hypnotic amnesia has several grades, and cannot be used to characterize any stage of hypnosis unless it is specified what grade is meant.

[12]Braid, however, long ago spoke of two grades of hypnosis, and Trömner in 1910 contended that Chojecki had been experimenting in a sort of diffuse, twilight consciousness, resembling stupor, whereas he, Trömner, had been investigating a condition "mit particllem systematischem Wachsein, nach Vogt".

this experimenting took place between November 1, 1922 and April 15, 1923.

Experiments reported heretofore have covered from one or two hours to what seems to have been several hours; but no such extended research as the present has been reported.

C. Technique. (1) Place and conditions of experimenting.

All sessions, of which there were an equal number in each state, were held in the same room in Emerson Hall, at corresponding hours for any one *S*. Although the experimental sessions for all *S*s were not of equal length, the sessions for any one *S* were equally long in the normal and the hypnotic states. The bodily position was always similar for the same task in the two states. Lights, furniture, apparatus, etc., were kept uniform as regards the same individual experiment in the two states.

(2) Temporal order of the experiments, of sessions, and of waking and hypnotic periods.

For detailed treatment of this matter one must consult the original thesis. In any individual session, as well as from session to session, the experiments were taken up in the following order: 1, 7, 5, 10, 6, 9, 8, 4, 11, 12, 14, 13, 2, 3, 15.

The trials in hypnosis and in waking always alternated as follows, whether the sessions were single, *i. e.*, sessions in which only one state of consciousness was tested, or double, *i. e.*, sessions in which both states of consciousness were tested at one sitting: the first trial was a waking trial, the second an hypnotic, the third an hypnotic, the fourth a waking, etc.

(3) Method of hypnotizing.

The author did his own hypnotizing, by the method of fixation of the eyes and verbal suggestion, without manipulation.

(4) Apparatus and test materials.

No intricate apparatus was used. Samples of the test materials accompany the original thesis. Materials were either identical for the two states, or were so constructed (*e. g.*, lists of words composed in pairs) and so modified when necessary—as shown by several trials on persons not taking part in these experiments—as to be of equivalent value. Besides, in the case of paired lists for testing memory, one list was given to half the *S*s in the waking state and to half in the hypnotic state. In this way, if, after all, one list was more difficult than its companion, the effect would be shared by both states.

(5) Preliminary experimentation.

To prevent the appearance in the published results of the effects of novelty and stage fright, and the unequal influence of the *S*s' being "easy" in hypnosis and uneasy in waking (for in hypnosis everything that happens seems quite natural), 3 preliminary sessions, made up of both hypnotic and waking periods, were held; that is, 3 for each *S*.

(6) Keeping constant the state of hypnosis.

Although the original thesis gives details on this technical point, it should be noted that this matter was simplified by the rejection, during the preliminary sessions, of those candidates who could not achieve a stable condition of hypnosis.

(7) Instructions.

The instructions accompanying each test were purposely made identical for the two states. The highest possible performance was demanded at all times.

(8) Making observations and recording results.

The *E* read off the test materials and took down the simple records necessary in each experiment.

(9) Treatment of the data.

Those interested can consult, in the thesis, Tables for each of the 15 experiments, and, besides, 3 Tables of summaries. Here, however, only two Tables are given. These deal with the averages of the various classes of *S*s. Explanations accompany the Tables. Since not the actual accomplishment of the *S*s but the relation of their hypnotic performance to their waking performance is of importance in this study, the emphasis has been placed on the hypnotic-normal ratio for each experiment. This ratio is in every case the *percentage* that the *S*'s hypnotic score is of his waking score; that is, the average score made in the waking session in these experiments is assumed to be 100%. For example, if the ratio 90 appears in a Table, it means that the hypnotic performance is 10% less than the normal; if the ratio is 120, the hypnotic performance is 20% better than the normal. So, then, in this report the actual results do not appear; what is given is the ratio obtained by dividing the hypnotic performance by the normal performance, expressed in percentage.

IV. Experimentation.

Experiment 1: Movement of the Arm over a Sector

A. Procedure. The purpose of this experiment was to compare the accuracy in waking and hypnosis of moving the right hand to a mark on a sector, with the eyes closed. The materials consisted merely of a sector of 120° drawn in ink on a piece of beaver board, which was tacked securely to a table in such a manner that a uniformly smooth surface was within reach of the *S*'s hand, with zero at the left. The movements, all of which started from zero, and were consequently only in one direction, were of two kinds: single movements to a number, and movements reproducing such single movements. At each session, in both states of consciousness, the *S*s were oriented to the sector by making a set number of moves, 6 in all, which were guided by the *E*. The *S*, of course, sat at the table.

This experiment represents for each of the 18 *S*s who performed it either 3 or 4 hours of experimentation, according as it was gone through twice or three times in each state. The total time was 62 hours, half of which was in the normal state.

The record consisted in keeping account of the total amount of errors (in degrees) of the movements in each state, computing the average deviations of the errors, totalling separately for each state the amounts by which the *S* exceeded the mark chosen (overestimation), and the amounts by which he fell short of the mark (underestimation). These data were then reduced to a hypnotic-waking ratio, which clearly shows the relative ability in the two states.

B. Results. In general, the results show that 11 of the 12 hypnotic *S*s made more errors and 10 of the 12 had a greater mean deviation of error in the hypnotic state than in the waking state; whereas there was no appreciable difference in the accomplishments of the control *S*s in the two states. With the hypnotic *S*s there was a tendency in waking to overestimate rather than to underestimate by the ratio of 308° to 109°, and in hypnosis by the ratio of [illegible]° to [illegible]°. The word "ratio" here is not used in the sense described in the preceding section. The control *S*s, in contrast with the hypnotic *S*s, did not overdo the matter of overestimation or underestimation in either state, although the amount of overestimation in both states (waking and pseudo-hypnosis) was greater than the amount of underestimation: [illegible]° (o.e.) and 195° (u.e.), 182° (o.e.) and 152° (u.e.), respectively for waking and pseudo-hypnosis.

As for ratios (in the sense of the preceding Section), the control *Ss* have 104% as regards total errors (symbolized by t.e.), and 97% as regards the mean deviation of errors (m.d.); the light hypnotic have 89% t.e., and 86% m.d.; the one deep alert 89% and 91% resp.; the two deep lethargic 69% and 79% resp.; the somnambulistic 79% and 82%, resp. The final ratio of all hypnotic *Ss* is 79% for accuracy of the movements and 84% for consistency.

C. Discussion of the Results. Of the 15 experiments reported in the present article, this is the only one in which there is a significant and consistent difference between the capacity in waking and in hypnosis. The explanation of this lowered performance and tendency to overestimate in hypnosis on the part of the hypnotic *Ss* seems plain enough. Since the suggestions of easiness and relaxation have been accepted by the *Ss*, there is an inclination to freedom and largeness of movement. Whatever suggestions as to taking care and moving exactly to a number are superadded can merely restrain to some degree this tendency. The fact that the mean deviations are almost one-fifth greater in hypnosis shows that the keeping of a definite image of the board in mind was not quite so good in that state as in waking. That mere lackadaisicalness accounts for the difference is hard to believe, because the somnambulistic *Ss*, who were fully as intent on the task in hypnosis as in waking, showed the same tendencies very markedly.

Experiment 2: Dynamometer Test

A. Procedure. The apparatus for this test was Salter's improved dynamometer, registering 112 pounds by half-pounds. The experiment consisted of obtaining at each session 5 scores for each hand at gripping the apparatus. Thus 10 scores were obtained at each session. For the 14 *Ss* taking this test there were 616 scores in all, half of which were secured in the normal state. In hypnosis the *Ss'* eyes were closed, in waking they were kept open; otherwise the conditions were the same for the two states. The instructions, to grip the apparatus as tightly as possible, were repeated before each of the 10 trials at each of the sessions, in both states. In neither state was the *S* informed of his scores.

B. Results. This experiment, which measures only the ordinary capacity, not the capacity of resisting fatigue, shows what will become an old story in this paper; a lack of significant difference in capacity between the two states. The record shows that in hypnosis 7 hypnotic *Ss* did better than in waking, 4 did worse, and 1 equalled his record; whereas the three control *Ss* were scattered between a better, an equal, and a worse performance in pseudo-hypnotism than in waking. As far as average ratios go, the control *Ss* did 1% better in pseudo-hypnotism; the light, 4% better; the deep alert, 4% worse; the deep lethargic, 2% worse; the somnambulistic did equally as well in hypnotism as in waking. The average ratio of all hypnotic *Ss* is just 100%, as against 101% for the controls.

These results seem to indicate that the capacity of the muscles brought into play is about equal in the two states. Whether with continued trials (which would bring in question the power of resisting fatigue) the results would be similar to those in this test, is an entirely different matter, and one which Nicholson[13] seems to have settled in the negative, by obtaining results which were greatly in favor of the hypnotic performance at working at an ergograph.

Experiment 3: Steadiness Test

A. Procedure. The apparatus for the test was the tracing board with an ever narrowing groove[14]. At each session there were two preliminary

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[13]*Johns Hopkins Hospital Bulletin*, No. 31, 1920, 89-91.

[14]G. M. Whipple, *Manual of the Simple Processes*, 152, Fig. 32.

movements, and 10 successive trials of continuous movement down the groove; and besides, 25 trials of continuous jabbing the stylus between the sides at the 2-mm. point. The final ratio was obtained by adding to the ratios of the jabbing, twice the ratios of the continuous movement and dividing by three, thus weighting the scores of the continuous movement. In all, there were 2,156 scores, made by 3 control and 11 hypnotic *S*s, half of the scores, as usual, being obtained in the hypnotic state.

B. Results. Two of the three control *S*s are distinctly poorer in hypnosis; the third is distinctly better. The ratios are 45%, 73%, 134%[15]. Seven of the 11 hypnotic *S*s were better; the final average ratio is 109%. The light hypnotic *S*s vary greatly, with ratios of 105%, 125% and 73%, an average of 101%. All the deep hypnotic *S*s of both types give results clearly in favor of hypnosis, the average ratio being 132%. The somnambulistic *S*s have ratios of 109%, 109%, 76% and 75%, an average of 92%. If this small, possibly chance, difference between the capacity in waking and in hypnosis needs explanation, it may lie in the fact that this experiment as conducted bordered slightly on a fatigue experiment, and that in hypnosis the subjective factor of fatigue was in abeyance. The low score of the controls can be explained by chance, on the basis of the small number of *S*s, only 3 as against 11 for the hypnotic *S*s.

Experiment 4: Pressure of Two Hairs on the Skin

A. Procedure. For this experiment one hair of 15/100 mm. diam. and 570 mgm. force, and another of 1/10 mm. and 220 mgm. were used. The method was that of areal stimulation of that portion of each hand lying between the knuckles of the index and middle fingers. There were for each of the 15 *S*s an average of 756 stimulations, 378 in each state, making a total of 6,670 stimulations in each state of consciousness. The hairs were glued to skewers and applied by hand. In regard to the order of stimulations, precautions against fatigue, secondary cues, and prejudicial suggestions, the original thesis must be consulted.

The instructions in this experiment were the same in both states as far as the orders to pay attention and answer "yes" every time a touch was felt were concerned. They differed in the fact that in hypnosis the hand was "sensitized" by suggestions of greatly increased ability to feel the slightest touch, and by having the *S* report that he did feel this increased sensitivity in himself. Getting the hypnotized *S* to believe that his hands are extremely sensitive is one of the easiest tasks a hypnotist can perform. Every hypnotic *S* reported greatly increased sensitivity, and often reacted quite strongly until he was ordered to feel the touch only as *pleasantly* sensitive.

In computing the score the total number of correct responses given at the stimulation of the stronger hair was added to twice the number of reports at the stimulation of the weaker hair. In this way the ability to feel the slighter pressure was weighted in the final score.

B. Results. The two control *S*s differed greatly in this test, the ratios being 85% and 121%[16], with an average of 103%; the four light *S*s were equally divided, showing ratios of 103, 89, 117 and 86, an average of 99%; both deep alert *S*s were poorer in hypnosis, with ratios of 91 and 97; the three deep lethargic were much poorer, with ratios of 91, 72 and 69, and an average of 77; the somnambulistic, however, came back up the scale with scores of 97, 108, 95, and 101, an average of 100.

The average of all hypnotic *S*s was 93%, as against 105% for the control. Not much stock can be taken in this comparison, however, because of the

[15]An instance of very great chance differences, since the matter of hypnosis did not enter at all.

[16]Another case of wide variation within one state of consciousness; here, again, there was no hypnosis at all.

small number of control Ss and the small number of trials made with them, 896 altogether, in comparison with that of the other class, 10,444 altogether.

C. Discussion. That there is in hypnosis a very great subjective intensification of the consciousness in regard to a touch which is actually felt, there can be no doubt. That this subjectively increased sensitivity may exist along with an actual lowering of objective sensitivity seems probable. Judging from the results of this test, it is in an experiment such as this, more than in any other kind, that one would look for Braid's two grades of hypnosis: one bringing about greater sensitivity, the other lowering it. Even though the present series of experiments is far from verifying any increased sensitivity of the skin, it may justly be said to indicate two types of hypnosis: (1) the type in which the S becomes less sensitive to touch, and (2) the type in which, for all one knows (judging by the combined results of the deep alert and of the somnambulistic Ss), the S retains his ordinary sensitivity.

It is well to note, however, that this division is not on the basis of depth of hypnosis—as has been erroneously thought—but on the basis of the different qualities of the states induced. The results seem to indicate that the very deep-going type, *i. e.*, the somnambulistic S, shows the most nearly *normal* sensitivity.

Experiment 5: Spelling Backward

A. Procedure. After telling the S to listen carefully to some words which he was to spell backwards "as fast as you can, still being accurate," the E gave in turn each of the following couples: "Shakespearean criticism"; "Evolutionary hypothesis"; "Experimental psychology," and "Philadelphia, Pennsylvania." The same words were used in all the sessions, but the order was varied, so that if today "Shakespearean criticism" came first, next week it would come last.

From the test two sets of scores were obtained, one in sec. and one in number of mistakes. No attempt was made to reduce them to a unit. There were, on the average, 2.3 sessions in each state for the 3 control Ss, and 2.2 sessions in each state for the 16 hypnotic Ss participating in the experiment.

B. Results. In speed only one of the 3 control and only 5 of the 16 hypnotic Ss did better in hypnosis than in waking; the average ratios are 105 and 95 for control and hypnotic, respectively. The control Ss, however, made up for their increased speed by a greater number of errors, the average being 17 errors in waking and 21 in pseudo-hypnosis. The hypnotic Ss, on the other hand, had an average of 12 mistakes in each state.

Among the hypnotic Ss the ratios for speed were as follows: light, 94; deep, 100; somnambulistic, 89. The somnambulistic, with the exception of one S, who made a ratio of 100, showed less speed than in the normal state, obtaining ratios of 90, 84, and 81; while they were furnishing only one less error.

If the hypnotic state brings on any great increase in the power of visualization, the scores in this test should have shown a marked betterment in hypnosis; unless, of course, this increased power of imaging should be offset by a decreased power or will to seize the image and "read it off." It seems that it is just that tendency to forge ahead, even at a task commanded ever so explicitly, which is lacking in hypnosis.

Experiment 6: Saying the Alphabet Backward

A. Procedure. This simple test was given with the same instructions as Experiment 5, viz., "Go as fast as you can, still being accurate." The record, made by the E, consisted in copying down all letters spoken by the S and noting the number of sec. spent in the process. Sixteen hypnotic Ss and 3 control Ss with an average of 2 sessions each in each state took part in this test.

B. Results. Although, on the face of it, this test closely resembles Experiment 5, the results are somewhat different. Here, 2 of the 3 control, and 13 of the 16 hypnotic *S*s showed a better record in the relaxed state: with ratios of 106 and 108 for control and hypnotic *S*s respectively. The average errors were: for control, 2.3 errors in waking to 1.6 in pseudo-hypnosis; for hypnotic, 2.5 errors in waking to 2.3 errors in hypnosis.

The explanation of this higher performance in saying the alphabet, unless it be a mere matter of chance (an alternative not to be lightly dismissed) may be found in the fact that a certain amount of initiative is required in spelling words backwards; whereas with a few repetitions of the alphabet backward a habit is formed. The more mechanical saying of the alphabet may not disturb the hypnotic relaxation; whereas spelling backward brings up a conflict between remaining easy and exerting oneself.

Experiment 7: Digit Span

Results. Records were obtained on 3 control *S*s, with an average of 2.0 sessions in each state, and 16 hypnotic *S*s, with an average of 2.5 sessions in each state, in repeating digits which had been read aloud. Although in hypnosis two of the deep lethargic *S*s simply could not keep the digits in mind, and the rest of the hypnotic *S*s reported great subjective certainty (for one *S* the digits were "engraved in granite," albeit sometimes the wrong digits; another "read them from a blackboard"), still, 12 of the 16 hypnotic *S*s equalled or bettered their score, with a final average of 101. The control had a ratio of 106. The conclusion is that there is no difference in this type of rote memory of these *S*s in the two states. Although the somnambulistic *S*s did 5% better in hypnosis, the control *S*s, without being hypnotized, did 6% better in pseudo-hypnosis.

Experiment 8: Cumulative Adding

Results. This series of adding consisted in the *S*'s saying aloud the number read to him by the *E*, *e. g.*, 9, then adding each time a greater number by one to the sum until he had added the number 12, as follows: 10, 12, 15, 19, etc. Only the sums were said aloud by the *S*. The record, which was just as extensive as the preceding one in respect to *S*s and sessions, consisted of all the numbers spoken and of the time consumed (sec.). All 3 control *S*s increased their speed, but made more errors in the pseudo-hypnotic state; 8 of the hypnotic *S*s bettered both their speed and their accuracy in hypnosis; whereas 1 hypnotic *S* did equally well, and 7 did poorer. The final ratios for control and hypnotic *S*s are respectively 110 and 102. We thus see that the behavior of the control *S*s in the pseudo-hypnotic state was less "normal" than that of the hypnotic *S*s in hypnosis itself.

Experiment 9: Memory for Nonsense Syllables

A. Procedure. These tests were conducted on the basis of retained members, trial being made after 5 readings. After the various series, of 10 couples, had been carefully prepared with a view to making them of equal difficulty, they were divided into two classes, one class for half the *S*s in waking and half the *S*s in hypnosis; the other class for the remainder of the *S*s.

In all, 3 control subjects with an average of 2 sessions in each state, and 16 hypnotic *S*s with an average of 2.4 sessions in each state, performed this experiment.

B. Results. It is of interest that in this test there is a steady loss of performance as depth of relaxation increases until the somnambulistic *S*s are reached; with the somnambulistic, however, there is such an improvement as can hardly be laid to chance. The ratios are: control 98, light [illegible], deep lethargic 78, deep alert 71, somnambulistic 115. The individual somnambulistic *S*s have ratios of 97, 105, 135, 121. The ratio of all hypnotic *S*s is 91.

C. Discussion. The results seem to show that the monotonous reading of nonsense syllables, in the three lightest forms of hypnosis, overcame the

explicit direction to note carefully and to remember. The performance and the introspections of the somnambulistic *S*s lead the *E* to believe that associations of a rather fantastic, irresponsible kind are easily formed in the somnambulistic state.

Experiment 10: Memory for Adjective-Noun Associations

A. Learning: Procedure. The method was that of reading through once a series of 25 adjectives linked with nouns, and then, on the basis of retained members, getting a score for learning. This procedure was repeated until all the members were learned. The scores from successive readings were so weighted that, the fewer the readings necessary, the higher was the score. Scores were obtained for 4 control *S*s with an average of 2.2 sessions in each state and 15 hypnotic *S*s with an average of 3 sessions in each state.

Results of Series A. The average of all control *S*s shows a ratio of 96%; of all hypnotic, a ratio of 98%. That is, both types of *S*s were a little poorer when in the relaxed state than when in the normal state. However, the hypnotic varied as follows: light, 102; deep alert, 83; deep lethargic, 89; somanmbulistic, 109. The control *S*s, moreover, differed among themselves greatly, with ratios of 96, 143, 75, 71, as did also the somnambulistic, with ratios of 97, 136, 99, 102.

B. Retention. By giving only the first number of the couples learned at previous sessions, scores for retention were obtained. (Eight distinct sets of 25 associations each were used in Experiment 10.) The control *S*s did better in pseudo-hypnosis, having a ratio of 107%. The hypnotic *S*s, likewise, made a better score in hypnosis, showing a ratio of 105%. The hypnotic classes varied widely: light, 101%; deep alert, 101; deep lethargic, 89; somnambulistic, 116. In the control group the individual ratios were 102, 170, 62, 95.

Discussion of Results of Experiment 10. One might say that the results of this experiment show either chance differences or individual differences. If they show individual differences, some *S*s tend to do better in the relaxed state, others worse. That the differences cannot be laid to hypnosis *qua* hypnosis is apparent; since the control *S*s, without being hypnotized at all, manifest the same kind of differences as do the hypnotic *S*s.

Experiment 11: Memory Span

Immediately after each reading of various lists of 10 names each (philosophers, poets, etc.), trial was made to see how many the *S* could repeat aloud. This procedure was followed until all 10 items were remembered.

The idea of hypnosis as a state in which the *S*'s mind is a *tabula rasa*, which may be written on in indelible ink, receives no confirmation from this test. The light hypnotic *S*s have an average ratio of 105, the deep 84, the somnambulistic 98. Only 9 hypnotic *S*s, and no control *S*s, took this test. The final average for all hypnotic *S*s is 96%.

Experiment 12: Memory of Miscellaneous Associations

The methods of giving this test and of computing the score were the same as those of Experiment 10. Here, however, only scores of learning were obtained. The materials consisted of unrelated words read aloud in couples. Two control *S*s with an average of 2.5 sessions in each state, and 11 hypnotic *S*s with an average of 2.5 sessions in each state took part.

This test, which surpasses most of the past experimentation in extent of time and the number of *S*s, gives results from which one may rightly generalize. For example, the two control *S*s, with hypnosis out of account, made ratios of 111 and 167, an average of 139; whereas the hypnotic made a ratio of 97, with individual scores running from 78 to 130. The conclusion is not that two normal states (as shown by the scores of the control *S*s) differ more than a normal state does from an hypnotic, but that chance differences

and individual differences may account for the wonderful reports of increased powers in hypnosis, with which the literature abounds. With a few *S*s, in a few experiments, one *E* gets one set of results; another gets quite contrary results. With a relatively large number of *S*s, in several sets of experiments, the chance and individual factors tend to be averaged away. In this very experiment the somnambulistic *S*s had individual ratios of 110, 113 80, and 78.

Experiment 13: Logical Associations

A. Procedure. Following a suggestion given by Professor McDougall, the *E* made lists of 20 words in logical sequence, the words leading from one to the next in such a fashion that they could be easily remembered as a whole; *e. g.,* Sir Galahad, knight, tournament, tennis, ball, etc. After reading the list of 20 words, the *E* gave the first word, and allowed 2 sec. for the *S* to produce each of the following words in order. If within that time no word, or a word not appearing in the list, was given, the correct word was said by the *E*, and the *S* was charged with a full mistake. If within the 2 sec. the wrong word, but a word appearing in the list, was given, the correct word was supplied by the *E* and the *S* was charged with half a mistake. Without further readings of that list, the *S* continued to repeat the list until he could go through it without making a mistake.

Three control *S*s, with an average of 2.0 sessions in each state, and 11 hypnotic *S*s, with an average of 2.8 sessions in each state, performed this experiment.

B. Results. Two of the 3 control *S*s were poorer in the relaxed state, the ratios being 96, 90, and 106, an average of 97%. Seven of the 11 hypnotic *S*s bettered their score while in hypnosis, the final average being 99%. The range of hypnotic scores is shown by ratios of 85 to 114. The somnambulistic show individual ratios of 98, 94, 114 and 101.

C. Discussion. In this experiment and in Experiment 10, if anywhere in this research, one should have expected to find a decided advantage in favor of the hypnotic performance. Since not the *S*'s own report of his memory, but his actual performance in remembering was taken, this test would have been almost final, if consistent differences between the normal and hypnotic abilities had been found. That no such differences were found leads one to conclude that in bettering *present* memory[17], either in respect to learning or to retention, hypnosis can do little or nothing.

Experiment 14: Free Association

Thirty sec. was allowed for each of the 2 trials given at each session in both states in saying as fast as possible the words that came to mind. The *E* gave a common word, such as "city," for a stimulus at each trial.

The 3 control *S*s, with an average of 2.0 sessions in each state, showed no consistent improvement in the relaxed state, the final ratio being 98%. The 11 hypnotic *S*s, with an average of 2.3 sessions in each state, ranged from 77% to 128%, with an average ratio of exactly 100% (a marvel of chance to get so round a number!). Five of the hypnotic *S*s did as well or better in hypnosis than in waking.

Since saying the words aloud required bodily exertion, and was, to some extent, contrary to the general suggestions of relaxation, there may be in hypnosis a very slightly increased power of association in one direction. It is not clear whether this is a case of the so-called narrowed consciousness (*i. e.*, an inhibition of all activities in the brain but the one in play at the [illegible] quite the reverse: a diffusion of the energy of the brain, so that whatever activity is in play [illegible] number of channels.

[17] A later paper will seek to show that, [illegible] revival of long-forgotten memories.

Experiment 15: Discrimination of Ink Blots

A. Procedure. Under Professor McDougall's direction the *E* formed 26 ink blots on 26 regular visiting cards, in such a way that several of the figures resembled each other to some extent, and yet had differences which, with anything like close study of the individual card foi 2 sec., could be easily distinguished. Before the cards were used, they were divided into two sets (by trial on persons not in the experiment) of about equal difficulty as far as discrimination went. The technique was as follows: after telling the *S* that he was to be shown a set of cards each of which he was to study very carefully so as to be able to pick it out later, the *E* let the *S* look at each of the 13 cards under a good desk-light for two full sec. Then the cards were shuffled with the other set, and the *S* was asked to say at the showing of each of the 26 cards, whether he had just studied it or not. A simple "yes" or "no" was all that was required. The score for each trial was computed by adding up all the wrong choices and subtracting the sum from 26. Two such scores were obtained at each session in both states for 3 control and 11 hypnotic *S*s, with 2.0 sessions in each state for every *S*.

B. Results. The scores in the relaxed state, in this test, more nearly approximate the normal scores than in any other of the 15 experiments. The average ratios of both the control and the hypnotic *S*s are exactly 100%. The 3 light *S*s give an average of 102; the deep alert give 92; the deep lethargic 96; the somnambulistic 104. The numerical distribution shows one control doing better, one worse, and one equally well in hypnosis; 5 hypnotic *S*s doing better, 3 worse, and 3 equally well.

The showing made in this test of discrimination, with the results both measurable and comparable, should have some weight in establishing the fact that normal persons of well developed powers can receive from hypnosis little, if anything, by way of augmented powers.

V. *Summary of the Results of the Fifteen Experiments.* Some of the facts summarized in this section may be studied in the two Tables printed herewith; for other facts, the thesis itself must be consulted.

The summary can be best set forth under the heads of (1) results according to *S*s, (2) results according to tests, and (3) results according to the physical or mental nature of the experiment.

(1) *Results according to Ss.* (*a*) *Control Ss.* Table 21 of the original thesis shows that in the average of 8.66 tasks in which the 6 control *S*s participated (from other Tables it is plain that all *S*s did not take all tests) they made a better score in pseudo-hypnosis than in "waking" in 4 experiments, the same score in .5 experiments, and a poorer score in 4.16 experiments. The figures just given are marked "a" in Table 21, and represent the score of individuals in single tests (not averaging the scores of individual *S*s). On the basis of averaging the scores (*i.e.*, the ratios) of each of the *S*s in one experiment, the results were better in 9 tasks, the same in 1 task, and worse in 6 tasks. Taking into consideration both of these sets of figures, but without trying to reduce them to a unit, one concludes that in the experiments reported above the control *S*s were on the average slightly better in pseudo-hypnosis, *i. e.*, in the relaxed state in which they

had received the same kind of suggestions as had induced hypnosis in the other *S*s, than they were in the ordinary state. This fact may be seen by reference to the Table published in this paper, Summary I; the final ratio is 103%.

(*b*) *Hypnotic Ss.* By referring to Table 21 of the original thesis, one sees that on the basis of the individual *S*'s score in each test (not averaging the scores) the 16 hypnotic *S*s, participating in 13.7 tasks on the average, did better in 5.875 tasks, did equally well in .875 tasks, and worse in 6.9 tasks. On the basis of averaging the scores of the 16 *S*s in each of the experiments, the hypnotic *S*s in hypnosis did better in 3 tasks, equally well in 3 tasks, and worse in 9 tasks. From this summary of the results one must conclude that, for these experiments as a whole, performance of all hypnotic *S*s in hypnosis was slightly lower than in the normal state. This conclusion is borne out by the final ratio (shown in Summary I in this paper), which is 97%.

It is important, however, to notice this fact: if the results of the first set of experiments (movement of the arm over a sector) are not averaged in with the other experiments, the grand summary of all hypnotic *S*s is exactly 100%, a remarkable example of the efficacy of having a large number of *S*s and sessions.

The reader's attention is called to the fact that there is less difference, on the average, between the performance of the hypnotic *S*s in hypnosis and their performance in waking consciousness than there is between the performance of the control *S*s in the two states of normal consciousness (*i. e.*, the "waking" and "pseudo-hypnotic"). That the differences in the performances of the hypnotic *S*s in any given test, or of any individual *S* in one or all tests, are due to hypnosis *per se*, is a gratuitous assumption. Experiment 1, Accuracy of Movement, provides the only exception to this sweeping statement.

A summary of the classes of hypnotic *S*s follows. (1) The light hypnotic *S*s did better in hypnosis in 6.33 tasks, equally well in .66, and poorer in 6.0. On the basis of averaging the scores of all *S*s in a given task, however, they did better in 12 and worse in 5 tasks. The conclusion that they were slightly better in hypnosis than in waking should be made, even though Summary I of this paper shows their final ratio as exactly 100%. (2) The deep alert *S*s were better in hypnosis in 5.0 tasks, equal in .5, and worse in 9.5; whereas the deep lethargic were better in 4.33, equal in .66, and poorer in 7.66. In the scores obtained by averaging the performance of the *S*s in a given test the results are: for the deep lethargic, 3 with better scores, 1 with equal, and 13 with poorer scores; for the deep alert, 4 with better, 1 with equal, and 12 with poorer scores. Summary I in this paper gives ratios of 93 for the deep lethargic and 95 for the deep alert, as final ratios. (3) The somnambulistic *S*s, on the basis of tak-

SUMMARY I

Subjects	Exp. 1	Exp. 2	Exp. 3	Exp. 4	Exp. 5	Exp. 6	Exp. 7	Exp. 8	Exp. 9	Exp. 10	Exp. 11	Exp. 12	Exp. 13	Exp. 14	Exp. 15	Avg. All Exper.
Light Hyp.	A81 B86	104	101	99	94	111	103	102	91	A102 B101	105	104	102	106	102	100
Deep Hyp. A. Alert type	A89 B91	96	128	94	100	125	104	106	71	A83 B101	75	95	88	84	92	95
B. Lethargic type	A69 B79	98	137	70	100	105	92	87	78	A89 B89	89	93	97	106	95	93
Avg. of All Deep Subjects	A76 B83	97	132	84	100	112	96	101	75	A86 B93	84	94	93	95	96	94
Somnambulistic	A79 B82	100	92	100	89	100	105	104	115	A109 B116	98	96	102	99	104	99
Avg. of All Hyp. Subjects	A79 B84	100	109	93	95	108	101	102	91	A92 B104	96	97	99	100	100	97
Avg. of All Control Subjects	A104 B97	101	84	103	105	106	106	110	98	A96 B107	—	139	97	98	100	103
Avg. of All Subjects	A88 B89	100	104	95	96	108	102	103	92	A98 B105	96	104	95	99	100	98.7

In every case the numbers above refer to average ratios obtained by dividing the average normal performance by the average hypnotic performance (or pseudo-hypnotic with the control subjects) and multiplying by 100%.

Experiment 1 { A = efficiency of performance as judged by amount of errors
{ B = " " " " " " mean deviations in errors

Experiment 10 { A = score in learning associations
{ B = score in retaining associations

ing each *S*'s score in each test, did better in hypnosis in 7.5 tasks, equally well in 1.5, and worse in 7.25. This result almost exactly corresponds to the scores obtained by averaging the individual ratios: 7 better, 3 equal, and 7 worse. Their final ratio in Summary I of this paper is 99. If, however, the scores on both parts of Experiment 1 (Accuracy of Movement) be disregarded, the grand average is 101%. Thus the somnambulistic *S*s, the deepest stage of all, are seen to be the most "normal" of all the deeper grades of hypnosis, as far as the results of this research show.

(2) *Results according to tests.* (*a*) *Control Ss.* Table 22, in the original thesis, gives a picture of the way in which the *S*s distributed themselves in regard to the height of their performance in each experiment. Of the 3.2 average control *S*s in all 15 experiments 1.5 did better in pseudo-hypnosis than in the normal state, .18 did the same, and 1.5 did worse. From this angle, then, there appears to be no difference between the "waking" and "pseudo-hypnotic" performance of the control *S*s.

(*b*) *Hypnotic Ss.* For the (average) 12.9 hypnotic *S*s in all 15 experiments, 5.5 did better in hypnosis, .82 did equally well, and 6.5 did worse. From this point of view, then, the hypnotic *S*s on the average did slightly poorer in hypnosis than in the normal condition. It must be remembered, however, that the low scores of all hypnotic *S*s on the first experiment (Movement) make the average hypnotic performance below the average waking performance.

(3) *Results on the basis of physical and mental tests.* (*a*) *Physical tests.* By studying Summary II in this paper, one sees that the light hypnotic *S*s averaged 94%[18]; the deep alert, 100; the deep lethargic, 88; the somnambulistic, 91. The grand

SUMMARY II

Groups	Physical Tests Exps. 1 - 4	Mental Tests Exps. 5 - 15	All Tests Exps. 1 - 15
I. All Control Subjects	98	106	103
II. All Hypnotic Subjects	93	99	97
1. Light Subjects	94	102	100
2. Deep Subjects	92	94	94
a. Alert type	100	94	95
b. Lethargic type	88	94	93
3. Somnambulistic	91	103	99
III. All Subjects	95	100	98.7

average of all hypnotic *S*s was 93% (on account of the low scores of Experiment 1); whereas the control *S*s made an average of 98%.

(*b*) *Mental tests.* In these tests the control *S*s have a ratio of 106, whereas all hypnotic [illegible] ic *S*s show a ratio of 102; the deep alert, 94; the deep lethargic, 94, and the somnambulistic, 103.

[18]See above, p. 219, for an explanation of the percentages given here

The survey of both types of tests shows that in these experiments in regard to physical tests the control *S*s did 2% poorer in pseudo-hypnosis, in regard to mental tests 6% better in pseudo-hypnosis; whereas the hypnotic *S*s did 7% poorer in regard to physical tests, and 1% poorer in regard to mental tests. The grand average shows that while the control *S*s in pseudo-hypnosis were doing 3% better in all tests, the hypnotic *S*s in hypnosis were doing exactly 3% worse. From these figures it is plain that it is as easy to explain the behavior of the hypnotic *S*s in hypnosis as it is to explain the behavior of the control *S*s in pseudo-hypnosis, in which latter state there was no intimation of hypnosis.

VI. Conclusions from the Above Experiments. The experimentation of which this paper is a condensed report has been extensive enough to leave us with some general impressions, which have arisen from a careful consideration of the data. It is clear that some of the popular conceptions in regard to hypnosis which have more or less won their way into scientific psychological thinking are untrue, or need to be restated in a way which casts a different light on the nature of the hypnotic consciousness. Despite individual differences in both the waking and the hypnotic states, differences which are very great, the following facts stand out in the results of the experimenter's work.

(1) The hypnotic state seems to vary as much between individuals and, in the same individual, between different times, as does the normal state. As far as abilities are concerned, there is no unitary hypnotic consciousness which is pretty much alike in various persons.

(2) There seems to be about as much variation in the individual, on the whole, from one normal session to another, or from one hypnotic session to another, as there is from a normal session to an hypnotic session, or from an hypnotic session to a normal session.

(3) An exception to the two statements just made lies in the fact that there is in hypnosis (at least in the somnambulistic stage) greater ability to resist fatigue, to resist pain, and to recall long past events (a later article will give the data on long past memories), on account of the tendency in hypnosis to take a certain mental attitude with conviction.

(4) A possible exception to the first two statements is found in the slightly lower ability in hypnosis to do tasks which require continuous responses, on account of the great relaxation, amounting at times to inertia.

(5) There is in hypnosis a definitely lower ability in the making of muscular movements, on account of the feeling of easiness which influences the judgment, and results in considerably freer,

sometimes almost reckless movements. (Cf. intoxication, which lowers inhibitions and manifests itself in largeness of movement.)

(6) On the whole, there is no noticeable difference between the normal and hypnotic states in the ability of normal persons in the fields of sensation, perception, finer discriminations, present memory (learning and retention), or physical work which does not involve fatigue.

(7) Judging from the great individual variations in both hypnotic and control *S*s, the past reports of greatly bettered performances of normal *S*s in hypnosis may be explained by one or all of the following considerations: (*a*) the small number of *S*s, (*b*) the small number of sessions, or (*c*) the lack of strictly comparable methods in the two states.

(8) Instead of Trömner's twofold division of the various conditions as regards mental abilities (light hypnosis, in which abilities are increased; deep hypnosis, in which abilities are decreased), there seems to be a threefold condition: (1) light hypnosis, in which the performance is fully as good as in waking; (*b*) deep hypnosis (exclusive of somnambulism), in which the performance is perhaps slightly lower than in waking; (*c*) somnambulism, the classical form of hypnotism, in which the normal performance is again equalled.

(9) The so-called hypersensitivity, *e. g.*, to a light touch, seems to be really a fact of subjective sensitivity, or better, perhaps, hyperactivity, *i. e.*, of an attitude of mind which makes the bodily reaction out of all proportion to the size of the stimulus actually felt. Instead of being able to react to a lighter stimulus (as would be the case if there were objective hypersensitivity), the *S* in hypnosis reacts more strongly than in waking to a stimulus which he can, however, feel in waking. That this subjective attitude may continue at the very time that the threshold of sensitivity is raised is probable from the results of Experiment 4. The cause, perhaps, is the removal through dissociation of the ordinary inhibitions.

(10) The possibility remains, of course, that the results shown in this research are a function of the methods of hypnotizing or of the class of *S*s used. It is thinkable, for example, that results such as S. Alrutz described[19] may be got by using his method or by using his *S*s; but until the comparative method as described in this paper is assiduously used in such cases, the results of his experiments (*e. g.*, affecting the sensitivity of the hand by passes which never come in contact with the hand) must be considered as without scientific repetition and verification.

[19] *Proc. Soc. Psy. Research*, 32, 1921, 151.

(11) In order to substantiate differences in mental ability in the two states, the following precautions should be taken: (*a*) there should be a methodology which is almost or quite identical for the two states; and (*b*) the following sets of *S*s should be used: several hypnotic *S*s, preferably of varying depths of hypnosis; several control *S*s who are treated just as the hypnotic *S*s are; and several control *S*s who go through the tests as many times as the two sets just mentioned, but without receiving the suggestion to sleep, or whatever suggestions are given.

The great variability in the performance of normal people without the factor of suggestion, and the astounding effects of suggestion on normal people in waking consciousness, both make the methods described above essential[20].

The hypnotic state is thus seen to be but a modified state of normal consciousness, which is differentiated from other normal states, not so much by way of any changes in physical ability, or of peculiar bodily conditions, as by the tendency to take an attitude suggested (within what limits no one has yet determined) in spite of bodily conditions. Since almost all persons can be hypnotized, and yet not all hypnotized persons (perhaps only one in twenty) can attain to the state of somnambulism, in which the increased abilities referred to in (3) above occur, the differences which appear in somnambulism are differences in the constitution of the persons involved; and would be better described, perhaps, merely as individual differences in normal persons than as differences between the normal and the hypnotic states as such. This would leave the term "hypnosis" with a fairly definite meaning: a state in which a person *will do*, in a *bona fide* manner, possessed of conviction, what he *will not do* in waking life for lack of such conviction.

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[20]C. W. Perky, Experimental Study of Imagination, this JOURNAL, 21, 1910, 422; O. Külpe, Ueber die Objectivirung und Subjectivirung von Sinneseindrücken, *Philos. Studien*, 19, 1902, 508.

THE PROBLEM OF MEANING IN BEHAVIORISM

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Many psychologists are strongly attracted to Behaviorism but hesitate to call themselves Behaviorists. In view of this situation, a discussion of the parts of the doctrine which attract us and of the parts which repel us should be worth while; for such a discussion must lead either to a broader definition of Behaviorism or else to a definite break with it on the part of these sympathizers.

It seems to me that all of us who sympathize with Behaviorism at all are attracted by its emphasis on the study of behavior as a biological phenomenon and by its frank recognition of the sterility of the study of states of consciousness. There does not appear to be any one aspect which antagonizes all of us. I find it convenient to distinguish two, as follows. First, at least some of the findings of the introspectionist are accepted as 'states of consciousness' and it is felt to be unscientific to ignore facts, whatever their value; and, secondly, the mechanistic prejudice of the prominent exponents of Behaviorism is felt to be an unnecessary bias which prevents impartial observation and interpretation of the facts of behavior.

In a recent article I presented a Behavioristic point of view which took account of the facts observed by the introspectionist, *but not as states of consciousness*, and which exhibited neither a mechanistic nor a vitalistic bias.[1] In an even more recent article Lashley does accept the findings of introspection as 'states of consciousness' and attempts to show how such facts can be explained by a mechanistic Behaviorism.[2] He attempts to meet the first of the objections stated above and gives the clearest and most systematic example of the second which it has been my good fortune to read. Accordingly I wish to discuss the parts of his doctrine which appear to me most inadequate in order to lead up to further discussion of the position taken by me in the earlier article.

The positions of Lashley to which I object are these. First, I object to the statement that a *complete* description and explanation of behavior can be given in physico-chemical terms[3]. No one doubts that such knowledge as we have of the physics and chemistry of behavior is valuable scientific knowledge, and every scientist hopes to see this knowledge extended. But

[1]Behavior and Conscious Behavior, *Psy. Rev.*, 30, 1923, 192 ff.

[2]K. S. Lashley, The Behavioristic Interpretation of Consciousness, *ibid.*, 30, 1923, 237 ff.

[3]*Op. cit.*, 238.

the dogmatic assertion that the principles of physics and chemistry will some day explain everything that is worth explaining is not only valueless, but, so far as it directs the attention of the investigator away from important facts which do not have the remotest connection with the *known* principles of physics and chemistry, it is vicious. Unfortunately the point is usually discussed under the heading 'Mechanism *versus* Vitalism', as if these two positions were the only ones possible. But there is a third position,—one of indifference to the problem. The typical argument between the vitalist and the mechanist proceeds about as follows. The vitalist challenges: "You cannot explain (let us say) meaning." The mechanist replies with an hypothetical account of the physical events which take place when the phenomenon 'meaning' occurs. The vitalist rejoins: "That does not explain." The mechanist snaps back: "It does"; and there we are. It is obvious that the debaters have divergent views of the nature of explanation. The vitalist demands logical continuity; the mechanist replies with temporal correlation. The problem with which they are dealing can be cast into the following form. If we try to envisage the process of evolution within the categories of space and time, we have no difficulty in granting, in principle, the temporal and logical continuity of physical development. When meaning appears as an observable characteristic of behavior, the temporal continuity of its development from meaningless behavior can be understood, in principle, easily enough. But we are unable to see any logical continuity, and this lack of understanding seems to us more than mere ignorance of detail. At most (if the statement means anything) we can say that prior to the advent of meaning there existed the potentialities which made its evolution possible. But neither the physicist, nor Watson, nor Lashley, nor Bergson has been able to enlighten me about the nature of these potentialities. What follows from this inability to understand? So far as I can see, there follows—nothing, *absolutely nothing*. The sensible thing seems to be to abandon the problem. Meantime I find that I can observe and understand (in a measure) the growth and development of meaningful behavior and, inasmuch as these problems of growth and development interest me, I follow that path.

That leads to my second objection. While I agree that the point of view of psychology should be biological, it does not follow that it should be mechanistic. All branches of biology, including physiology and psychology, have their mechanists, vitalists, and those who are neither. Without attempting to draw a fine line between psychology and physiology, it seems to me that physiology is interested in every aspect of organic behavior, psychology chiefly in its meaning, if there is one. Thus the physics and chemistry of the phototropism are of interest to the physiologist, but the question what, if anything, Euglena means when it reacts to light,—that is, the question whether the phototropism presents a problem which can be called a psychological problem,—that problem is excessively obscure. And it seems to me that one's position in psychology should not depend on the arbitrary solution of an apparently insoluble problem. The case is quite otherwise when we consider human behavior. Here meaning rather than mechanism is of preponderating practical and psychological importance; and if Behaviorism cannot deal with it, so much the worse for Behaviorism.

We come here to the heart of the problem which Behaviorism must face, the problem of meaning. And my third and most serious objection to Lashley's position is that he relegates the description of meaning to Titchener's introspection[4]. Introspection, I shall try to show, is biased observation. The bias is a double one, and becomes particularly unfortunate when applied to meaning. In the first place, introspection finds states of conscious-

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[4]*Op. cit.*, 258 ff. "The fact is that meaning, on subjective analysis, reduces to a succession of images accompanied by vague affects."

ness where unbiased observation finds nothing of the kind. Whether a stone is regarded as a physical object or as a state of consciousness does not depend on the technique of observation, but on the bias of the observer. (Meantime the stone, or human behavior, can be described without raising the question.) In the second place, the introspectionist describes his states of consciousness from what Titchener calls the 'existential' point of view. That is, he is interested in what sensation *'is'*, not in what it means. And sensation, Titchener finds, is a sensory *quale* of a given duration and intensity. What then *is* meaning? Titchener answers and Lashley quotes approvingly: "An idea means another idea, *is* [emphasis mine] the meaning of that other idea, if it is that idea's context." And again: "Meaning is, originally, kinaesthesis." That is, one sensory *quale* means another sensory *quale*, *'is'* the meaning of that other *quale*,—a kinaesthetic *quale* means a blue *quale*. Surely, neither Titchener's observation nor that of anyone else ever observed so weird a happening. The mere statement is almost a *reductio ad absurdum* of the introspective account of meaning. Now suppose that an individual untutored in introspection is asked to describe the nature of his thoughts. For the sake of simplicity suppose that he depends exclusively on kinaesthesis. After the initial misunderstandings have been cleared away, he may report that he feels either movement or strain of the lips, tongue, eyes, and other parts of the body. If then he is told that these are kinaesthetic sensations, he will have got as far as the average student of psychology ever gets in introspection. If however he perseveres in his observations, he will learn to recognize some of these feelings of strain and movement as *nascent acts* in a way which will clarify the connection of the act with the meaning of the act. For example, the office of one of my colleagues is to the left of mine. A frequent way I have of thinking of him is by going through the bare initial stages of leaving my office to go to his. The eyes tend to move to the left, and there is a slight change in the tonus of many muscles. There is nothing startling or new about such descriptions. What I wish to exphasize is that the *direct observation of a meaningful act* is as genuine and direct an observation as the 'introspection' of a muscular strain. To be sure, if I had not found the historical setting of the act, I might not have been able to *observe* its true character, and it might have remained a meaningless movement to me. But, in the same way, a child who never learns to read will never see anything but black marks on a white background. Learning to note kinaesthetic 'feels' is learning the alphabet of self-observation; learning to observe *meaningful* behavior is learning to read. Only a psychophysical bias coupled with an existential point of view can stop short of a more adequate account of meaning than is furnished by kinaesthetic sensations.

That brings me to my fourth objection, which is that Lashley classes meaning with 'facts of consciousness' and accordingly considers it a *subjective* phenomenon. My position is that meaning is a characteristic of behavior and is *objective*. Of course, the terms 'subjective' and 'objective' are notoriously obscure. Lashley says: "The subjective and objective descriptions are not descriptions from two essentially different points of view, . . . but simply descriptions of the same thing with different degrees of accuracy and detail."[6] Now I have attempted to show above that Titchener's account of meaning is inaccurate and lacks detail not because of the peculiar character of the phenomena observed, but because of the biased point of view of the observer. I have shown that neither the observation of 'external objects' nor self-observation yields facts of an order essentially different from the facts yielded by any possible technique of observation. From such a point of view the subjective *does not exist* except in the mind of the mechanist and the subjectivist. So far as the 'objectivity' of mean-

[6]*Op. cit.*, 338.

ing is concerned, the meaning of an act is often directly observable and at other times it can be inferred on the basis of direct observations. In either case the account is, in principle, verifiable. And observability and verifiability are all the objectivity that science needs. (A word should be said about the verification of an account of meaning. Verification is the presentation of verifiable evidence. Such evidence, aside from the description of the act itself, is the account of the historical facts which led up to it, and correct anticipation of the consequences. An example will be found below. But even though such evidence may convince the most sceptical, it will not constitute *proof* in the sense in which that term is used in exact science. And the reason for that will appear below also.)

We may conclude this part of our discussion by considering Lashley's statement of the problem of psychology, as follows: "To find in the physical world deterministic relations between *nonqualitative* [emphasis mine] discrete entities in time and space which fulfill certain conditions of relationship laid down by subjective evidence."[6] It might be worth pointing out that receptors, neurones, or indeed any entities which are directly observable, have *qualities*. Evidently Lashley has electrons in mind and uses the body only because, to him, it is self-evident that the living organism is a physical thing and its behavior a physical process. Consequently the meaning of that behavior becomes 'subjective'. The reader's attention is directed chiefly to the logical connection between the physico-chemical bias and the subjectivity of meaning. The physico-chemical bias itself is not worth opposing, because no living psychologists are actually at work at physico-chemical problems nor will they be in the near future. The actual explanations of the mechanistic Behaviorist are being given in physiological terms. And what I object to is not the physiological terminology, but the *falsification of the observable so-called subjective facts.*

The chief problem of psychology, as I see it, is to give an adequate, correct, and verifiable account of the meaning of behavior. The total problem of physiology and psychology has been stated adequately by Watson as that of prediction and control of behavior. But he who understands the meaning of behavior so far as seems possible will be able to predict and control human conduct in the vital problems in which Lashley professes interest far more adequately than he who knows all the physics, chemistry, and mechanistic physiology which we shall know for some time to come. At any rate, that is the path which has always been taken by those who seek to understand and influence men and women in a practical way, and it is, in effect, the path taken by the psychiatrist and the psychoanalyst. Psychology as the science of a bodiless mind joined to a mindless body has contributed almost nothing, and a Behaviorism which attempts to explain meaning in terms of meaningless movements will not contribute more. Before Behaviorism can attack the genuine problems of the meaning of behavior it must clear out of its path the metaphysical problem which it has created for itself by insisting on the subjective (or epiphenomenal) character of meaning. The rest of this paper will be devoted to the discussion of

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[6]*Op. cit.*, 329.

behavior and meaning as *facts*, without asking or caring whether the facts are physical or mental, subjective or objective.

In a previous paper I put forward the claim that meaningful behavior is a directly observable objective fact and that it should be the most fundamental and basic category of psychology. I supported that claim by description and discussion of various types of meaningful behavior by means of which I attempted to make this idea intelligible and plausible.[7] Since that time various critical suggestions have reached me, three of which I mean to utilize in order to develop this idea further. (1) It was said that my treatment implies that the behavior of even the lowest forms of living organisms is meaningful or conscious behavior; (2) it was pointed out that the absence of a definition of meaningful behavior weakened the article; (3) the statement was made that these theoretical views would mean more if it were shown what relation, if any, they have to the experimental work which is being done by psychologists. In what follows I shall assume that the reader is familiar with the former article.

Taking up the first point, I start with the behavior of the *human* animal. It seems to me an indisputable fact that the meaning of human behavior is, at times, directly observable. The objection that the term 'meaning' is anthropomorphic seems ridiculous, so long as we confine ourselves to the description of human behavior. How else is human behavior to be described, if not in terms which have evolved for that very purpose? When we come to the higher vertebrates it seems equally obvious that they see, hear, smell, fight, and engage in other forms of meaningful behavior analogous to human behavior. On the other hand it should be equally obvious that there are notable differences between human and infrahuman behavior. But that is a question of detail which, with one exception, I did not discuss.[8] And, so far as I can see, one is not under logical obligation to discuss such detail. But I deny categorically that anything whatever can be deduced regarding the behavior of protozoa from the established fact that some human behavior is meaningful, except perhaps that the earliest forms of life were of such character as to make possible the subsequent evolution which as a matter of fact did take place. Such a statement is entirely different from the statement that the behavior of protozoa is meaningful. And I deny that a solution of this problem is necessary to an adequate understanding of human conduct.

Of course the issue ultimately involved at this point is a metaphysical one; and in 'Behavior and Conscious Behavior' I protested against the introduction of *any* metaphysical assumption into psychology. Weiss holds

[7] Behavior and Conscious Behavior, *Psy. Rev.*, 30, 1923, 192 ff.

[8] The exception is that my discussion implied that only animals with well developed language-habits are capable of self-conscious behavior.

that that amounts to a refusal to discuss fundamental issues;[9] and, at the expense of a brief digression, I wish to discuss his point. So far as the refusal to discuss fundamentals is concerned, I had supposed that in discussing consciousness and behavior I *was* discussing fundamental ideas. But I do deny that an arbitrary solution of a metaphysical problem yields a sound foundation for a psychology which deals with the empirical facts of human behavior. The metaphysical foundation which Weiss has used may serve as another illustration.

Weiss holds "that all human conduct and achievement reduces to *nothing but*: (*a*) different kinds of electron-proton groupings characterized according to geometrical structure; (*b*) the motions that occur when one structural or dynamic form changes into another." Accordingly the individual is an electron-proton aggregate and a function (in the mathematical sense) of the changes that are occurring in all other electron-proton aggregates.[10] That is the foundation. Then Weiss proceeds to classify the acts of individuals as (1) biophysical, *i. e.*, anatomically and physiologically similar acts, and (2) biosocial acts, which are socially equivalent because they release socially similar responses in others. Thus various ways of accepting an invitation are socially equivalent if they would be responded to in practically the same fashion by the one who extended the invitation. That is the psychological superstructure built upon the metaphysical foundation. Now the superstructure is not deducible from the foundation nor does it have any logical connection of any kind with that foundation. No arguments either for or against the accuracy, utility, or significance of Weiss' dual classification can be drawn from his electro-proton hypothesis. The classification, separated from its metaphysics, could be accepted without modification regardless of one's theory of reality. In fine, the metaphysical foundation of Weiss, far from being fundamental, is irrelevant to the rest of his psychology.

I come now to the fact that my former article lacked a formal definition of meaningful behavior. Every science, so far as its structure is logical and systematic, has at its foundations a set of *undefined terms* and *unproved assumptions* about these terms. Inasmuch as I believe that the term 'meaningful behavior' should be one of the fundamentals of psychology, I cannot define it by reducing it to simpler terms. To do so would be tantamount to the admission that these simpler terms are themselves the real fundamentals. Nevertheless it would be unwarranted to throw the term at the reader without an attempt to make clear to him the nature of the concrete phenomena which it denotes to me. Logically I should be perfectly justified in using an undefined term and making assumptions about that term, but psychologically such procedure would leave the term abstract, empty, and without relation to concrete experience. Consequently I have tried to point at, describe, and discuss the concrete phenomena which the term denotes to me. For example, in 'Behavior and Conscious Behavior,' I have discussed the behavior of seeing as one of the phenomena which the term denotes. Also I have discussed the alleged fact (logically, the unproved

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[9] A. P. Weiss, Behavior and Behaviorism, *Psy. Rev.*, 31, 1924, 35.
[10] *Op. cit.*, 39.

assumption) that the acting organism is not always aware of the meaning of its meaningful acts. In the rest of this article I shall offer additional description and discussion. In lieu of a formal definition, which, as I have shown, is logically impossible, I shall make a brief statement intended to sum up the discussion of the former article as well as the discussion which is to follow in this article. No other 'definition' of a fundamental idea is logically possible.

When a psychologist studies a human being for the purpose of understanding him and his behavior, he arrives after a time at what he believes to be a true statement of the environment of this individual or as near to such a statement as is possible. At the same time he recognizes (or should recognize) that the environment as experienced by the observed individual is more or less different from the environment experienced by the observer. In order to understand the individual the observer endeavors (or should endeavor) to make clear to himself the nature of the environment *as experienced by the individual.* As a matter of definition, I shall call the environment as experienced by the observer the true environment, or briefly the environment, and I shall call the environment as experienced by the observed individual the experienced situation, or briefly the situation, and the two terms will be used in this sense throughout this article. I am now ready to make a brief statement to take the place of a formal definition of the term 'meaningful behavior'. By means of meaningful behavior the organism enters into relations with its environment.[11] Each relation is at one and the same time the behavior of experiencing a situation and the behavior of acting with reference to the experienced situation. *By definition,* the behavior of acting with reference to a situation and the behavior of experiencing that situation is a single phenomenon; the behavior of acting with reference to a situation *is* the behavior of experiencing that situation. A concrete illustration would be an individual who establishes a relation with an environment consisting in part of a white sheet by seeing a ghost. By means of 'seeing-the-ghost' the organism enters into a relation with its environment; seeing-the-ghost is the behavior of experiencing the ghost; seeing-the-ghost not only leads to further behavior with reference to the ghost, but is itself behavior with reference to the ghost.

Let us now discuss this definition. So far as the environment is concerned, the psychologist should describe it as he sees it *after mature deliberation.* If he views it as an electron proton aggregate he should describe it in that way (without much detail,

[11]These relations which the organism enters into are by no means the only relations of the organism to the environment which can be observed.

I fear). But I insist that he should not let his view of the environment falsify his description of the situation. If I saw a ghost, it does not describe *the situation* to say that I saw a white sheet hanging from a clothes-line.[12]

Again, I have asserted that the meaningful act is at one and the same time a mode of experiencing and a mode of acting. In other words, I assert that the distinction between awareness and action is valid logically, but invalid phenomenologically. The distinction has arisen because in the practical exigencies of life we usually describe what we have experienced, not our modes or experiencing,—we discuss what we have seen, not the behavior of seeing. The distinction is valid and useful, just as the distinction between the pitch and the intensity of a sound is valid and useful even though the two are inseparable aspects of a single phenomenon. But, considering the meaningful act as a concrete phenomenon, awareness and activity are inseparable. The distinction rests upon an analysis of language and the fallacy arises because terms such as seeing, fearing, and shrinking, which should denote partial aspects of single unitary activities, are taken to denote separate activities. Seeing is (by definition) the immediate automatic response by means of which the organism establishes relations with the environment *via* the retina, not *with* or *to* the retina. And *all of this response*, the visceral part as well as the part played by the striped muscles and *by the eye*, is the act of seeing. For, *by definition*, the organism does *not* respond to a stimulation of the sense-organ, but the sense-organ is a part of the organism which reacts to the environment. Terms such as seeing, hearing, etc., as used ordinarily, denote the part played in such reactions by the sense-organ and connote the sensory *quale* of the experienced situation; terms such as shrinking, cowering, running, striking, etc., denote primarily the part played by the skeletal muscles without specific connotation regarding the qualitative character of the situation with reference to which the act takes place; and the emotional terms of nontechnical language are more or less vague both as to the sensory *quale* of the situation and as to the part played by the effectors in the response. But shrinking from danger is an act which takes place *via* some sense-organ, not to a sense-organ, and the emotional *quale* is as truly perceived as part of the experienced situa-

[12]We may note in passing that if I see a ghost I do not necessarily observe myself seeing the ghost, but the observer *necessarily* does so. We need not therefore debate the question whether the self is a 'content of consciousness'. In observing the conduct of an organism one necessarily observes the organism, and that is true also when the organism observes its own conduct. Accordingly the classical problem of the self as content reduces to the problem of the meaning of the word 'I' (or some equivalent) in various situations.

tion as is the sensory *quale*. In certain instances, and for practical purposes, it is legitimate enough to speak of first seeing danger and then shrinking from it; but a careful psychological statement should state that the activity of experiencing a situation to be shrunk from by means of the behavior of shrinking from it is preceded by the activity of experiencing a somewhat different situation, *e. g.*, a situation to be investigated anxiously, by means of the behavior of investigating it anxiously. As the behavior changes, so the situation changes. Ordinary language, legitimately enough, denotes the earlier situation by the general term danger, which means a situation which can be responded to in a variety of ways without designating the specific ways which actually are occurring.

I wish to emphasize once more that it is the behavior of *experiencing-a-situation* which constitutes the relation of the organism to its environment. The situation is what it is only by virtue of being experienced in that way. Consequently any attempt to describe this relation must include not only an account of the behavior of experiencing but also an account of what is experienced. Seeing-a-ghost is a single unitary phenomenon. The seeing cannot be abstracted from the ghost except as length is abstracted from width. Neither has phenomenological existence except as a characteristic of a phenomenon.

Before attempting to discuss more at length the way in which meaningful behavior means its stimulus or situation, I wish to review certain other general characteristics of meaningful behavior. In ordinary usage the word meaning is used to indicate (1) a characteristic of experience, and (2) a characteristic of behavior. Frequently the two usages are interchangeable. Thus I may speak of perceiving a threatening cloud or of meaning to seek shelter. But in either case my relation to the environment is my meaningful behavior toward it; and when the state of affairs is envisaged in that way the problem of the locus of meaning is solved. It is the relation of the organism to its environment which means. Consequently I have argued that the psychologist should deal with meaning only as a characteristic of behavior; and that is the usage adopted in this paper.[13]

Again, the idea of meaningful behavior has something in common with an idea in good repute with the physicist, viz., the idea of motion. No matter how far we push the process of abstraction, we cannot abstract the ideas of time and direction away from either. If we do, there is left neither motion nor behavior. The motion of a body or the behavior of an organism is not a thing which '*is*'; it is an event which happens. It is in its very essence historical and cannot be understood without refer-

[13]Cf. Behavior and Conscious Behavior, 211 ff.

ence to its history. There are times when the relevant history of a moving body can be perceived in a fraction of a second sufficiently well to determine the direction of the movement with an accuracy which suffices for practical purposes. The same thing is true of the meaning of acts. At other times extended observation and a process of inference may be necessary to determine the direction of the one or the meaning of the other. Beyond this point, however, the analogy ceases to hold; for the direction of a movement can be computed with absolute accuracy (in theory), whereas it is frequently impossible, even in theory, to state the meaning of an act with as much rational clearness as might be desirable. This difficulty will be discussed further presently.

Again, in 'Behavior and Conscious Behavior' I cited G. H. Mead to the effect that the gesture is an unfinished social act which means the completed act. Thus the beginning of an attack is *perceived* as the beginning of an attack, not as a movement the meaning of which must be inferred. I wish to make two additions. In the first place, the beginning of an attack is what it is whether or not it has been correctly perceived at the time of its occurrence. In the second place, an unfinished act need not be a *social* act in order to mean whatever it means. In any case, the meaning of an unfinished act can be stated adequately only with reference to the total act of which it is the beginning. Accordingly Mead's formula is applicable to all kinds of acts, social and non-social.[14]

In order to illustrate and elaborate these points, let us examine first a simple form of behavior and then a complicated case. The picking up of a pencil will do for the simple form. Usually that will be part of a larger activity, such as the jotting down of a memorandum which in turn is noted for some purpose or other; but let us neglect that. An account of the meaning of this act is given by the common-sense description, viz., picking up a pencil. But it is worth noting that that meaning is not a static fact or relation; that it progresses as the act progresses; and when the activity comes to an end, the meaning ceases, just as motion ceases when a moving body comes to rest. A completed act does not mean,—it has meant. If, however, the act is interrupted before completion, the incomplete act can be described adequately only with reference to what the act would have been, if it had not been interrupted, for up to the moment of interruption the act meant just that.[15]

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[14]We must be careful however to distinguish the consequences of the act from the meaning of the act. For example, the sitting hen does *not* mean to hatch chicks, and the human animal not infrequently does *not* mean to become a parent. The distinction can be made on the basis of relevant evidence and it is the task of the psychologist to find such evidence.

[15]In order to ward off a misunderstanding which occurred in the case of a critic who saw this paper in advance of publication, let me say that the discovery of what a completed act has meant may be as important psychologically as finding what an incomplete act has meant or means while it is occurring. Thus an homicide might be described as a completed act by a

Our illustration of a simple meaningful act has been reasonably clear, if we do not deprive ourselves of common sense in the interest of a mechanistic bias. It must be borne in mind, however, that a part of this clarity was achieved by abstracting a trivial act from its larger setting. A concrete act is never merely the picking up of a pencil; and if we had taken a larger setting into account, a precise and exhaustive account of meaning would not have been possible. Let us therefore examine a complex case. Some years ago I was consulted by a patient who suffered from attacks of tachycardia for which no organic cause could be found. He had been told by physicians that his case was 'mental'. Assuming the medical findings to be correct, the rapid heart-action could be nothing but one of the phenomena of strong emotion, but that told me neither the nature of the emotion nor its occasion. A study of the life-history obtained from the patient himself revealed a history of partial failures in business and of misadventures in love. At the time he came to me he was engaged in an important undertaking the outcome of which seemed doubtful to him. I concluded that he had lost confidence in himself; was afraid that he was going to fail in the undertaking in which he was engaged, and feared that he would make a failure of life. At certain times this fear degenerated into panic, and that, I concluded, was the tachycardia. Of course, the patient did not admit all this to himself. As a matter of fact, the immediate problem which confronted him was not difficult, and there was no reason why he should not look forward to a successful issue with confidence. Without attempting to explain the tachycardia to him, I succeeded in instilling a little confidence into him (perhaps only temporarily) and the tachycardia disappeared. That is a common-sense account of the case. I shall attempt now to deal more adequately with the concrete meaningful behavior as it actually occurred. The behavior in question is, of course, not merely the tachycardia but the total behavior of which that was a part.

I have said that the meaning of an incomplete act can be described only by describing the total act of which it is the beginning. If we regard the rapid heart-action as the beginning of an activity, our problem is to describe the rest of the act, *i. e.*, what would have happened if the act had been completed. In the first place and in part, the tachycardia was the beginning of an attempt to escape, by means of *running*, in the literal 'physical' sense, from a fearful situation.[16] That act, of course, was irrational; but emotional reactions frequently are. Our subsequent analysis will not and should not make the meaning of the actual act more rational, but it should make more intelligible the fact that in spite of its vagueness and irrationality the act had a *more or less* definite meaning. The tachycardia, then, was not only an initial stage of running away, but it was also an integral part of the act of *perceiving* a *fearful* situation. In 'Behavior and Conscious Behavior' I tried to show that perceiving is a form of behavior, that we *perceive* not only actualities, but possibilities and probabilities, and that such perceiving *is* the occurrence of more or less automatic and habitual forms of meaningful behavior. These principles will make the tachycardia more intelligible. In perceiving we perceive not only objects, but total situations. Conventional psychology has made a sharp distinction between cognitive and affective states of consciousness and has, arbitrarily, abstracted all affective factors from the 'percept'. But if we attempt to study perceiving as meaningful

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mechanistic Behaviorist without taking account of the meaning of the act, but from the point of view of the law and of common sense such a description would be highly inadequate. If, for example, the individual who committed the homicide meant to preserve his life, the meaning of this completed act would be highly relevant.

[16]As a matter of fact these attacks usually came on after dark, and the patient would rush out for long walks.

behavior we cannot afford to follow this line of cleavage. If, as I have suggested, we perceive with our muscles and glands as well as with our sense-organs, then the heart and the viscera can enter into the behavior of perceiving as well as the striped muscles. If perceiving is in any sense a total reaction of the organism, then the fact that there has not been an important change in visceral activity does not mean that these visceral activities are not a part of perceiving, but that the behavior of perceiving has not been modified so far as visceral activities are concerned. An illustration would be the calm perceiving of a dangerous situation as contrasted with the perceiving of a fearful situation. In the second case we do not have the cognition of danger *plus* the emotion of fear, but we have the perceiving of a terrifying situation. The visceral behavior is not the response to a fearful situation, but a situation is perceived as fearful because part of the response is a change in visceral activity. Of course, the self-conscious human adult can, and sometimes does, give his attention to his own behavior of fearing; but no doubt all of my readers will recall situations which *looked*, *sounded*, or *felt* tense or ominous or ridiculous or the like in a way which will make them realize that such descriptions are not mere figures of speech, but accurate descriptions of concrete experiences. In such situations possibilities and probabilities are *perceived*, not thought of. To be sure, the perception is vague, and when we attempt a description of these perceived possibilities and probabilities in verbal terms we find the task extremely difficult. Nevertheless there occurs in such situations more than 'a pointing from nothing to nothing',[17] and it is the task of the psychologist to give a rational account of such irrational behavior. As I have said and as we shall see, that can be accomplished by viewing the behavior as an historical event with a past and a future.

In consequence, then, of certain developments in his affairs our tachycardia patient *perceived* a terrifying situation. Let us suppose that he became acquainted with these developments through reading a letter. The response was in part visceral and persisted after the letter had been read and he had stopped thinking about it. As I have suggested, we have at all times an appreciation of the emotional aspects of our situation. Thus the individual who is grieving tastes bitter, tasteless, or revolting food, breathes an oppressive atmosphere, thinks depressive thoughts, and the like. With it all there is a constant (usually unobserved) tendency to escape from the sad situation. In the same way our patient continued to experience a fearful situation long after he had stopped thinking about it. Occasionally the visceral part of the activity became intensified into tachycardia.

For the physiological details of the visceral reaction we may accept the 'rather probable guesses' of the typical Behaviorist; for its meaning we shall consult its history. On a number of previous occasions in his life my patient had experienced 'unfavorable developments'. In each case the reaction had been partly visceral. Probably, at first, the visceral reaction did not persist. But on each occasion his fears had proved to be justified; failure had ensued. So on subsequent occasions his visceral reactions became more persistent, and more definitely meaningful. And they *meant* what his past experience had taught, viz., failure. The details of the failure were foreseen only so far as they were foreseen, *i. e.*, so far as he brought them to his own attention by means of overt or nascent gestures. Apparently very little of such foreseeing took place. The behavior was largely non-social and non-gestural. The probability of failure was not recognized as such; it was not thought of in that way; it was not *known*. The best possible way of describing the phenomenon is to say that the individual *feared* failure. If the patient had said "I fear failure," no one would deny that his

[17]Cf. K. S. Lashley, The Behavioristic Interpretation of Consciousness, *Psy. Rev.*, 30, 1923, 258.

verbal behavior meant what it did mean. *All I claim is that the statement would have been true, if he had made it.* And the fact that he failed to perceive the meaning of his own behavior should not prevent our doing so. But neither the acting individual nor any one else can ever know in precise, accurate detail what an emotional reaction means, *because it does not mean accurate and precise detail.* It is not that kind of behavior. It is, essentially, a vague *perceptual* summing up of past experience.[18]

The description of such situations and of such meaningful behavior cannot be given, at the present time, in the technical terms of any of the sciences. Certainly neither the sensations and affects of conventional psychology nor the vocabulary which Behaviorism has borrowed from physiology will serve. At the present time the only language which is adequate is the non-technical language of highly cultured nations, presumably because this vocabulary has been developed by individuals who, unlike the psychologist, have faced the real problems of human life. A usable terminology has been developed by Freud,—usable, because many of his terms sum up lengthy description of observable characteristics of human behavior into single terms. But the Freudian vocabulary is objectionable because it substitutes false rationalizations for vague meanings. Take for example the 'Oedipus complex'. Freud is right when he claims that marital unhappiness comes back in certain typical cases to the love of the wife for her father. But as a rule he is mistaken when he claims that this love is an unconscious desire for sexual union with the father. A young woman who has been petted, protected, humored, and treated as a lovable but irresponsible child by her father up to the day of her marriage, and who has enjoyed being treated in this fashion, is not likely to be happy with a husband who treats her as a responsible adult. Such a woman may love her father and be unhappy with her husband. But the important factor is a (more or less) vague dissatisfaction with the present situation and a (more or less) vague longing for a return to the earlier satisfying situation. There is no unconscious desire. The behavior means—is—the beginning of the act of leaving the husband to return to the father.

Suppose now that we were able to make a complete physiological analysis of the complex forms of behavior which we have been discussing. Very likely such an analysis would throw additional light on the cases, but for such light we must await the progress of physiology. Certainly the 'rather probable guesses' which Lashley feels he can make are not serviceable. For when Lashley says that "he is hungry and purposes to have beefsteak and onions for dinner" these words are not, as he claims, merely accepted names for the fact that certain physiological changes are occurring.[19] In that sense they are accepted only by a small (and mistaken) group of Behaviorists. By the rest of us they are accepted 'names' for what he means to do. That is, they give us the meaning of the behavior which he attempts to describe in physiological terms. But if the psychologist knows the meaning of behavior, he can dispense with rather probable guesses about its physiology. I do not mean to say that physiological observations cannot give us valuable hints about the meaning of behavior. Recent work on the physiology of deception illustrates how physiology can help psychology. I do affirm that Lashley is putting the cart before the horse when he says that the aim of

[18]Half the difficulties of psychology result from our unsuccessful attempts to force the description of vague meanings into the verbal categories evolved by logicians and the disciples of exact science. But what is vague '*is*' vague. And so long as science remains the attempt to find out what actually occurs, so long will science reject the false simplifications of the mechanist and the equally false rationalizations of the teleologist.

[19]*Op. cit.*, 350.

self-observation should be the discovery of cues to physiological problems.[20] Physiological technique is of service to psychology because it enables us to observe details of behavior ordinarily hidden from view, not because it gives us the *objective* correlates of *subjective* facts.

I come now to my last topic,—the bearing which the views expressed in 'Behavior and Conscious Behavior' and in the present paper have on experimental technique. First let me ask: "What has experimental psychology contributed to the interpretation of such cases as our tachycardia case?" So far as I can see, one does not need to be a psychologist or Behaviorist in order to understand that repeated failure is likely to discourage the individual more or less permanently; that failure in important undertakings is more likely to have this effect than failure in trivial affairs; and that business and love are two of the most important affairs in which men engage. With a history of such failures before one, one *should* look for signs of discouragement and fear. Nevertheless the ordinary cultured individual is not likely to arrive at a correct interpretation of such a case. The ordinary individual does not make it his special business to interpret human behavior except so far as seems necessary to carry on his own affairs. He takes account of the point of view of the other individual only in order to effect a sale, make a good impression, or even in order to win friendship or love. But, in the first place, many of us do not attempt to understand others even where it would be plainly to our advantage to do so. And, in the second place, even when we do so we seek a means to an end, not an end in itself. Accordingly an utterly inadequate amount of time and energy is devoted to the task of attempting to understand others. Obviously, the man whose chief interest is to reach an understanding of human conduct, who brings to bear all his knowledge and experience upon this single task, and who pursues this end even when he is not confronted by an immediate problem, will have an advantage over the ordinary cultured individual whose main interests lie elsewhere. On the other hand, it is a question which can be raised in all fairness whether the laboratory psychologist is really interested in human conduct in this vital way. Most of us have come to see that analysis of states of consciousness into sensations and affects does not lead in that direction. I have attempted to show that analysis of human behavior into meaningless physiological components is predestined to be equally futile. And whatever connection with sterile theory the fact may have, *it is a fact* that the human behavior studied by our experimentalists is trivial behavior studied under artificial conditions. The individual, as such, does not

[20] "Its (introspection's) avowed aim must be the discovery of cues to physiological problems"; *op. cit.*, 352.

concern the experimentalist. He experiments with 'subjects,' not with individuals. Individual histories and individual problems are unknown and do not enter into laboratory problems. Frankly, I cannot see that the laboratory work that has been done since the beginning of experimental psychology has added materially to our understanding of human conduct, nor has there resulted from it any considerable advance in psychological theory. We have advanced, but the advance has been due to more accurate and less biased self-observation, to the advance of biology, to the spread of knowledge about and the increase of thoughtful consideration of our major social problems, to better and more accurate knowledge of the behavior and conditions of other times and other races, and to the observations of workers in fields on the outskirts of academic psychology, such as 'mental' disease, crime, and the like.[21] If our experimentalists would care less about making psychology an 'objective science' and more about contributing to the *facts* from which progress has come and will come, they would join the other workers who observe the human animal in its natural habitat. The important behavior-patterns of human beings are social through and through. Any considerable increase in our knowledge of the formation and meaning of these patterns can come only through the study of the actual formation and functioning of these patterns and of the actual conditions under which they are formed. The home, the school, the church, court, factory, office, shop, etc., are the workshop of the psychologist. For science is not playing with apparatus in the laboratory, but the search after truth.

In conclusion and by way of brief summary, I shall enumerate what seem to me to be the essential positions of this paper, as follows.

(1) I have tried to show that a mechanistic Behaviorism cannot deal with meaning because meaning is not a mechanistic category. Lashley in particular attempts to meet this difficulty by adopting a quasi-dualistic position, and solves his problem by finding the physiological correlates of the sensations and images into which meaning is resolved by introspection. But technical introspection (from the 'existential point of view') attempts to resolve the dynamic fact of meaning into static components and accordingly must fail.

[21] Psychology has made a genuine contribution [illegible] the human individual in the mental test, but this advance has come as an [illegible] and adaptation of the old-fashioned pedagogical examination, not as a development of the laboratory [illegible]. The lack of space and [illegible] forbid an adequate discussion of this topic.

THE "GLASSY SENSATION"[1]

By E. F. Möller

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Introduction

In 1920 Schumann[2] described a hitherto unrecognized sensation, which he called the 'glassy sensation'. The name arose from the reports of *O*s who, under certain circumstances, described empty space as filled with a "glass body, in which there are very small particles,"[3] with "frozen air,"[4] or "transparent ice."[4] It was the object of the present investigation to secure an exact description of this 'sensation' and to determine the special conditions of its arousal. We have failed entirely in the latter half of our task; but, on the other hand, we have secured a large amount of material which we think points to the cooperation of the centre, if not to a central origin. We believe, that is, that the 'glassy sensation' is essentially imaginal, of the nature of a tied image, and not sensory.

[1] [illegible] Cornell University.

[2] F. Schumann, Die Repräsentation des [illegible] *Z. f. Psychol.*, 85, 1920, 224-245.

[3] *Op. cit.*, 230.

[4] *Op. cit.*, 232.

The history of the problem goes back to Hering (1879). "During the day, one sees what is called empty space between oneself and the object which one sees quite differently from at night. The increasing darkness lies not merely on the object, but also between oneself and the object, until finally it veils the object completely and fills the space alone."[5] Karpinska, working with the stereoscope, found that the object which her *Os* saw, though given objectively only by black boundary lines, was made up of an 'etwas'. "This material is something quite airy; transparent. The *Os* designate it as transparent paper, gelatine, glass, a very thin indefinable material; thickened or crystallized air. It is air and yet not air."[6] The transparent *Zwischenmedium* of Jaensch, which may be either colored or colorless, belongs as a space-filling medium to the same category.[7] It remained, however, for Schumann to undertake a special investigation of this visible intermediary, with the result that he raises it to the level of a 'sensation'.

Schumann, using the Zeiss stereoscope, presented to his *Os* some of the Martius-Matzdorff stereograms, which showed figures outlined in black upon a white ground, and some stereoscopic pictures of streets. He asked whether there was anything 'visible' between the lines of the figures or the houses on the streets. The reports showed that "the surfaces of the figures appear to be made of a glassy material . . . The content is a less compact medium, but it is like glass and has many points localized at different distances in it." A stereogram of a marketplace appeared "as if the whole was enclosed in something which was 'like quite clear ice'," and in which, too, there were small points, "like the finest dust."[8] The *Os*, upon questioning, characterized this filling as "clear mountain water," "quite transparent ice," "frozen air," "a colorless filling," which is "more or less compact," "condensed," or "thin."[9] They stated definitely that the glass did not belong to the light or to the color series, that it was a new quality different from those of the recognized visual sensations. When, repeating Jaensch's experiments, they observed the *Zwischenmedium*, they declared it to be qualitatively the same as the stereoscopic filling.[10] They also found the 'glass' with pairs of identical figures viewed in the stereoscope; and, of their own accord, reported instances in which they had observed it in nature.[11]

The conditions for the arousal of this 'sensation', Schumann finds, are (1) plasticity, which is aided by eye-movement; and (2) the localization of

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[5]E. Hering, Der Raumsinn und die Bewegungen des Auges, in Hermann's *Handb. d. Physiol.*, 3, 1879, 573.

[6]L. v. Karpinska, Experimentelle Beiträge zur Analyse der Tiefenwahrnehmung, *Z. f. Psychol.*, 57, 1910, 44f.

[7]E. Jaensch, Ueber die Wahrnehmung des Raumes, *ibid.*, Erg. Bd. 6, 1911, Kap. 6, 244-307. Cf also: W. Fuchs, Experimentelle Untersuchungen über das simultane Hintereinandersehen auf derselben Sehrichtung, *ibid.*, 91, 1923, 166; H. Hofmann, Untersuchungen über den Empfindungsbegriff, *Arch. f. d. ges. Psychol.*, 26, 1913, 112ff.; E. Rubin, Visuell wahrgenommene Figuren, 1, 1921, 61-66; G. E. Müller, Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes, *Z. f. Psychol.*, Erg. Bd. 9, 1917, 408f.; D. Katz, Die Erscheinungsweisen der Farben, *ibid.*, Erg. Bd. 7, 1911, 17f., 309ff; K. Bühler, Die Erscheinungsweisen der Farben (*Handbuch d. Psych.*, I., i.), 1922, Abschn. I., esp. 21 ff., 63 f.

[8]F. Schumann, *op. cit.*, 230.

[9]*Op. cit.*, 232.

[10]*Op. cit.*, 233.

[11]*Op. cit.*, 237.

small points in different planes. The latter condition may have its value in that it brings about roving attention, or roving attention accompanied by eye-movement, or varying degrees of compactness of the filling.[12]

This 'glassy sensation', which Schumann proposes as the sensory representative of empty space, has already found an antagonist in von Frey, who declares that the phenomenon which Schumann observed is due merely to a technical imperfection in the paper of the stereoscopic slide, and that aside from the effect of such imperfection there is nothing present in the experience.[13] Von Frey repeated Schumann's experiment with the stereoscope, using for his slides both stereoscopic and identical photographs of simple objects, printed upon dull and upon glazed paper. He remarked an increase in the size of the object which he saw in the stereoscope; an increase in existing lustre, or an addition of lustre to dull surfaces; and finally, the presence of a fine dust which filled the space in which the object stood. The dust appeared the more clearly, the duller the paper of the slide; and it arises, according to von Frey, simply because "the maximum and the minimum points of brightness arising from the grain of the paper fall on disparate, not on identical, points of the two retinas, and according to the degree of disparity are seen in different depths. The impression is favored by skillful lighting, if the roughness of the paper thus comes out clearly."[14] This dust-filled space von Frey identifies with the glass of Schumann. He also finds it in nature, when the air is filled with smoke, fog, or dust. That the evidence points to a new 'sensation' he denies, (1) because the fog-product of stereoscopic vision is associated with a definite technical imperfection of the picture; (2) because differences of intensity, which should enable one to judge the depth of the space perceived through the sensation in question, do not exist; (3) because, when the usual criteria for the estimation of distance are not present, as, for instance, in the clear air of high mountains, serious errors are made in the estimation of distance; and (4) because "truly transparent bodies are not perceived except through polish or blemishes on the surface or inside the body, or on the ground of known optical criteria."[15]

Both Schumann and von Frey find suspended in the space within and about their experimental objects certain small flecks or dust-like particles. But Schumann declares that "if these points are not in a plane, but are arranged at different depths, they appear to lie in a glass body . . . If they are all localized in the same plane, the arousal of a plane (glass) surface is aided . . . Sometimes there appears before the whole image a glass plate with little flecks upon it."[16] Schumann thus observes, in addition to the flecks, a glassy medium, in or upon which these bits of dust are contained. Between the flecks there is the glass of the 'glass body'. Von Frey, on the other hand, admits only the flecks, between which he sees nothing. "The object . . . stands in a space which is filled with fine dust . . . This dust acts as an air perspective . . The 'air or glass impression' of which Schumann speaks is only another name for what I have called 'dust and lustre'."[17]

Schumann and von Frey are agreed as to the flecks; they disagree whether there is anything visible between or around the flecks. On this main point the results of our observations sustain Schumann.

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[12] *Op. cit.*, 238.

[13] M. v. Frey, Uber die sog. [illegible] des leeren Raumes, *Z. f. Biol.*, 73, 1921, 262-266.

[14] *Op. cit.*, 264.

[15] *Op. cit.*, 266.

[16] F. Schumann, *op. cit.*, 239.

[17] M. v. Frey, *op. cit.*, 264.

Our experiments naturally group themselves under fo heads: (*A*) experiments with the stereoscope; (*B*) experimen with skeleton forms; (*C*) experiments with mirrored form (*D*) miscellaneous experiments. A detailed account of meth and apparatus appears under every head. These groups do n represent the chronological order of the investigation, nor can v assert that the order of series within the groups followed a pr determined, logical plan. The phenomenon under investigati is both multiform and elusive, and we often shifted the course our work to follow up new cues as they appeared.

The general instruction given to the *O*s read: "You are to describe wh you see. Your description will be fourfold, including a characterization (*a*) the figure; (*b*) boundaries of the figure; (*c*) contents of the figure; a (*d*) background-differences in and out of the figure." We shall indicate a change made in this instruction in cases where we found such a chan necessary.

(*A*) *Experiments with the Stereoscope*

For these experiments we used; (1) the ordinary Brewste Holmes Stereoscope; (2) the Zeiss Precision Stereoscope; a (3) the Ludwig Tropostereoscope. We performed these exper ments in a dark room, where the light which came from a reflect containing a 200 watt Mazda bulb filtered through a Gage 'da light' glass. The reflector was fastened to a standard behind t *O*, at a height of 2 m. from the floor. Before allowing them start upon the actual experiments, we trained the *O*s in stere scopic vision by means of the Hausmann stereograms.[18]

(1) *Experiments with the Brewster-Holmes Stereoscope*

Series a. Each one of the 6 slides in this series showed a truncated co or pyramid outlined stereoscopically in black upon a gray background, which the texture varied from a very smooth paper to a cardboard wh had been roughly scraped with a knife. This series was presented 3 tim in serial order, beginning with the roughest background (Slide 1). If, von Frey says, the flecks in the empty space are a function of the bac ground, then we thought that we should receive reports of a snowy, fla filling over the rough background and a fine dust over the smooth bac ground; that is, the size of the flecks should vary as the roughness of t background varies. The reports should also indicate the presence or a sence of a glassy medium in addition to the flecks. The *O*s were: A, B By and H.[19]

We asked for a characterization of the figure only to determine the a tainment of the proper stereoscopic effect; therefore we shall not inclu these reports in our results. In the discussion, S refers to bounding su faces, C to content of the figure, and D to the background-differences; a the number to the slide in reference to which the report was made.

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[18] *Dr. W. Hausmanns 26 Stereoscopen-Bilder zur Prüfung auf binocula Sehen und zu Uebungen für Schielende,* Leipzig, 1913.

[19] Dr. L. B. Hoisington (H) and Dr. H. G. Bishop (Bi) were members the department of psychology; Dr. G. Adams (A) and Mr. F. L. Bixby (B were graduate students in the department.

Surfaces. A (1) did not see the truncated pyramid. She reported, however, that the large square, which would have formed the base of the pyramid, was "glazed." . . . "It looks like isinglass." (2) In the one case where this was seen as a truncated pyramid "the large square is glazed, and the glaze forms the sides." In the other two presentations, "the small square is in front of the large square. It is more isinglassy than the large one. I can't tell whether the glaze belongs between the two squares or in the large one." (3) "The small circle is transparent; the sides are dense and glassy looking with streaks and splotches in them." (3) "The small circle is glassier than the rest. It is flaky and rough, but the glass is over everything and has the character of the background in it." (4) "In indirect vision, the sides are glassy, transparent and not of the same substance as the contents. The sides dissolve and disappear when I look for them. The small circle appears glassy and not transparent. The big circle is glassy, but the little circle is more so." (5) "The sides are more transparent than the small circle, which is more glassy-looking." (6) "The small circle is filled with something transparent, jelly-like, flaky, not definitely streaked. I don't know how far back it goes. The transparent glassy stuff has the character of the background in it in cracks and ice-splinters."

Bi is unable to find any surfaces for (1) and (2). (3) "A conical sheet of some kind links the two circles together, yet when I look for it I can't distinguish it from this glass mass." (4) "The small circle has a rigid, dancing, glassy surface that is a little blurry. The area of the small circle is like a transparent after-image." (5) "I think of the glass and the small circle as having a front, but I don't see any." (6) "I can force a glass surface to stand out."

By for all 6 slides reports: "Casually the sides and the bottom are made of the background. Critically the sides are made of glass." The surfaces are "a thin film of a shiny, shimmery, transparent, colorless stuff." In all except (2) "the sides appear to reflect light, like glass." Except for (5) "the base of the figure is made up of the color and texture of the background." The influence of the background on the glass is remarked in (1); "the shimmery stuff seems conditioned on the texture of the background. It covers everything." (4) "The surfaces are waves of colorless, transparent stuff. It's like little holes in the air." (6) "The glass is more markedly the texture of the card projected to fill the sides and the front opening. The figure is entirely surrounded by a film with very fine waves in it."

H (1) "The gray of the background seems to fill up the sides, and yet at the same time it is back. It does not fill up the front end." (2) "The gray lends itself to the sides, yet they are transparent." (3) "The sides are dense, crystalline, transparent, and glassy. The gray lends itself to the sides, yet seems to belong back." (4) "The sides are solid, transparent, and glassy. The gray lends itself, but it is certainly beyond." (5) "The small circle is perfectly transparent. The gray is there as though reflected in the hard, smooth sides, and seems to give a little of its character to the cone." (6) "The gray seems to form a complete surface, yet there is something beyond it. The background lends itself to the sides, which are glassy-like, hard, dense and transparent. It lends itself more definitely to the sides than to the end. It is impossible to be sure you see through the side at all. It is more like a reflection, because the side is glassy and smooth; yet you get this gray, which is rough and beyond."

Contents. For A the content of the figure is "glassy" or "icy", but "as isinglass is glassy. 'Glass' itself is too clear." (3) "The glassiness takes on [illegible]." (4) "This glass is clearer and more like ice than the others." (5) "A transparent [illegible] shake of jelly." (6) "A soft glassy thing which holds the small circle out in front: flaky and not definitely streaked." *O* repeatedly states that 'glass' is not a good descriptive word. (5) "[illegible] is not a good descriptive word because it is too clear. Ice is better, because you can see ice and see through it too."

Bi reports all the content as "glass, which the background seems to affect, though I don't really think it does." It is compared in quality to "dim high-lights on slightly tarnished silver," "a hard, glinty, glittery, sparkling, transparent stuff." (3) "The glass sparkles. It is as if the texture of everything were fine instead of coarse. Since you see the background through the glass it has the effect of making the glass coarser or finer, but it only seems to. The glass is a very irregular crystalline formation, like zinc crystallized on iron, or like heat waves caught." (4) "Hard and unchanging and resistant like ice." (5) "The background is finer but the texture of the glass is not, it is constant." (6) "If I were less careful, I'd say this was a finer texture of glass. It is like seeing the bottom of a stream through ice. It films the background."

By reports in every case: "The glassy stuff has the texture of the background in it." The glass is "shimmery, transparent, colorless, wavy." (2) "The waves are finer and seem to be part of the texture of the thing." (4) "The glass inside seems to be more wavy than the sides." (5) "This shimmery stuff fills the cone. It waves up and down like a frozen shimmer. It is a colorless transparent something and seems to be texture even more markedly; like bubbles or fine holes punched in the air." (6) "The filling is a gelatinous substance. It has fine waves running crosswise like the grain of the background running that way."

H finds that "the filling is some sort of a transparent solid medium, yet the gray seems to lend itself to the body of the cone." "The gray belongs to the figure, but seems to lend substance to it." In one case (6) H reports: "The texture of the background is like the texture of the pyramid itself," and "cues for solidity are some bits of black quality in the figure."

Background Differences. A reports that "the background outside of the larger circle is a rough surface. There is no background inside the figure."

Bi says: "The background seen through the glass appears blurry as if out of focus." (3) "It makes the background look as warm air makes it look, only this doesn't go in waves, it is static."

By "The background inside the figure is smoother and lighter in tint than that outside."

H "The gray background shows sharp shifts from dark to light quality. The background through the sides of the figure is more uniform. There are still variations, but they are smoothed out. The shift of brightness is not so sharply defined. The gray seen through the little end is the same as that around the outside."

This was the first series taken, and it is evident that the *O*s are groping for descriptive words. We note (1) that all the *O*s saw more or less of what we may call surface glass and content glass; (2) that, curiously enough, no *O* directly reports dust particles or flecks, though we might perhaps infer their presence from A (S 3,6), (C 3,5,6); Bi (S 4), (C 3,5,6); By (S 1,4,6), (C 2,4,5,6); and H (S 6), (C 6); and (3) that the glass of the figure in some way has an effect upon the background which is seen through it. For A and Bi the sides are elusive; for By they differ with casual and with critical observation; and for H they are transparent yet somehow gray.

Series b. We introduced small crosses or circles as points of reference[20] upon the slides of Series *a*, which we presented in haphazard order. The *O*s were: A, Bi and H.

Surfaces. A (4) "Indistinct." (6) "You assume that there are sides holding this glassy stuff in."

Bi (2) "Nothing joining the circles, but I can read in a conical surface which isn't there." (6) "A hint of a conical surface, not visible, but somewhat in terms of eye-kinaesthesis."

[20] H. v. Helmholtz, *Handb. d. physiol. Optik*, 1896, 917f.

H (2) "The front surface is hard, glinty, clear, more transparent, more alive, and less gray than the sides. The sides are slightly grayish, clear, hard, and not perfectly transparent."

Contents. A (3) "Glassy." (4) "Jelly-like."

Bi (1) "The whole slide is covered with glass so that there was no content proper for the figure." (4) "The glass is lustrous, extremely rigid, it blurs the background, and it diminishes outward to the end of the slide."

H (2) "The figure is solid. Some dark spots seem to float in the medium." (5) "Like the sides." (6) "Glassy-like and transparent."

Background Differences. These differences were reported as like those in Series *a*. Except where there is a change in the nature of the report, we shall not include this division in our future discussion.

For two *O*s, A and Bi, the presence of a surface glass is doubtful, while H finds it much as it existed in Series *a*. There is a glassy content, but again there is no definite mention of flecks, except for the dark spots which H reports as floating in the medium (C 2).

Series c. In order to determine the effect of the tint of the background upon the glass, we used a single transparent colorless celluloid slide upon which we outlined a truncated cone stereoscopically in black ink, and behind which at a distance of 1.5 m. we set up a cardboard background, which we varied. We used Bk, W and gray cardboard. The *O*s were A, Bi and H.

Surfaces. A (1) "Indistinct."

Bi introduces a 'fog'. (3) "The small circle is full of fog, and the conical surface is a fog (made of the background) that stands at the proper angle to be a cone."

H (1) "The front surface is hard, glinty, clearer, more alive, less gray, and more transparent than the sides." (2) "The sides are slightly grayish."

Contents. A (1) "Seems frozen." (2) "Whiter, stiller, and harder than the background; jelly-like." (3) "Not slick." The word 'glass' is not mentioned.

Bi (2) "Filled with the fog that makes up the background. The fog is shaped like a bowl. The inside volume is impenetrable. It's like a snow cone set in a snow background."

H (1) "Transparent; the same as the sides."

The change of tint itself apparently does not affect the figure, while the nature of the background may do so. The glass is not present for A and Bi. A sees a white jelly-like figure, with indistinct sides, while for Bi it is a foggy volume. The figure for H is unchanged.

Series d. The stimuli in this series consisted of a set of stereoscopic views; St. Martin's Cross, Iona; Ehrenbreitstein, on the Rhine; The Bold Axenstrasse, Lake of Lucerne; Ruins of St. Werner; and the Ruins of the Granite temple, the Sphinx, and the Great Pyramid. The instruction read; "You are to describe what you see, noting particularly the space between things." The *O*s were H, Bi, A and By.

A describes the space between the objects as "glassy." (1) "Like something frozen hard in between things, as if the things were set in ice." (2) "The glass isn't so clear that you can't see it, and it makes the third dimension more prominent than it would otherwise be." (3) "The glass holds the figure, which may be flat, apart."

Bi (2) "The hard glassiness fills up the space. The space is glittery." (3) "Grayish toward the back." (5) "Like a perfectly clear gelatine, where the only quality you get is high lights."

By (2) "The whole thing is very glassy, as if the picture were set in a glass like a paperweight." (4) "Very glassy. I can see the shining of light and imperfections. This may be in the lens." (5) "I can't say what the space between things looks like."

H (1) "The space is most definite in the foreground, where it is glassy, clear and neutrally colored. It has no definite front boundary." (3) "Bits of light and dark gray, localized between myself and the object looked at, give body, substantiality, reality to space." (4) "I don't believe I see anything in particular. It's space, good tridimensionality, but I can't say that I see anything between the things."

The *O*s indicate a wealth of glass. From A (1,2) and Bi (1,2) the presence of flecks may be inferred, while By mentions imperfections (4), and H attributes the reality of the space to bits of dark gray (3).

General Summary

The reports are very consistent. Except for A and Bi in Series *c*, all the *O*s see glass as content and as surface, although both A and Bi find the sides difficult to observe. The *O*s do not report flecks directly, but we may infer their presence in numerous cases.

(2) *Experiments with the Zeiss Stereoscope*

Series a. This series was a repetition of Series *a* with the Brewster-Holmes stereoscope. The construction of the instrument allowed us to add three finer backgrounds; a piece of transparent celluloid in the substance of which were small specks, placed upon the milk-glass plate which is part of the equipment of the Zeiss instrument; the milk-glass plate itself; and a glass vessel filled with milk. The imperfect piece of celluloid, upon which a cone was outlined stereoscopically in black ink, gave a background which was more uniform in texture than the smoothest paper. More uniform than this was the milk-glass plate; while the most perfect background of all, as far as uniformity was concerned, was the glass vessel of milk. With the last two backgrounds the cones appeared in outline on perfectly clear colorless celluloid. The stimuli were presented in serial order to two *O*s (By and Bs) and in haphazard order to one *O* (Bi).[21]

Surfaces. Bi does not see a surface. In every case he reports: "The surface is there by implication, but I don't see any sides."

By (1) "Not directly observable; a filmy, colorless, rather coarse-textured thing that's like the black inside a black tube, because, when you look at it to describe it, it's all around where you're looking, but not where you're looking." The surfaces differ only in texture; (2) "like frozen waves, a finer texture than the first;" (6) "only slightly discernible;" (8) "a colorless, transparent, textureless medium."

Bs (1) "The sides are grayish-looking; rough and fuzzy surfaces." (3) "Glassy; about transparent. I seem to see through the sides, and I see this other stuff through them." (6) "Like the background to a certain degree, but transparent and more delicate." (7) "You look through the glassy, smooth, transparent sides to the filling." (8) "The side is sort of glassy-looking; slick-looking like blanc-mange, and you look through it to the filling."

Contents. Bi does not report a content of the figure, but a content of the entire visual field. Except for (8) and (9), the report for (3) is typical: "The glittery glass is everywhere in front of the background. The black lines are like wires set in the glass. The glass is very rigid, and it is thicker through the small circle, and gets thinner out toward the edge of the field." "The glass is blurrish, like dust in the air, but it isn't that, though it reminds me of it." The reports for (8) and (9) are alike: "No glass; a cone-like thing simply suspended in a mist, and filled with the mist."

By (1) "The same sort of thing as the surfaces, except that it's a coarser texture; fills up the whole inside of the figure, which is a hollow shell." The content differs from slide to slide only in texture. (3) "The hollow shell is filled with a colorless mist of finer texture than the other figures;" (4) "a

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[21]Miss F. Burns (Bs) was a graduate student in psychology.

mist like the sides; has little drops in it; denser and more compact than the sides, and almost fluid or bulky, instead of filmy;" (7) "a shell filled with a transparent medium like the surfaces; always a marked difference between surfaces and filling; the same general stuff, but the inside is bulky or filmy, while the outside is a very smooth surface;" (8) "seems to have a very fine texture about it;" (9) "a colorless medium, which is smooth, bulky and transparent."

Bs (1) "Like the glass of a heavy paper-weight; thick, glassy, air-like. The fuzzy stuff (of the background) makes it have an icy appearance, as when you see through a thin icicle, or like what's between the surface of the water and the image of an object in it." (2) "The same glassy stuff but not so thick or heavy as before; less dense; like ice that's full of cracks, but is not falling apart." (3) "The same glassy stuff; concentrated through the center, and thinner through the sides." (4) "The same glassy stuff, but more transparent, clearer, brighter, than before; like looking into a clear liquid." (6) "Not as heavy or as thick as the others; less bulky." (7) "Just like clear ice." (8) "Grayish; looks like jellied dishwater." (9) "Like the other one, only it's white and looks heavy like blanc-mange."

Background Differences. Bi for (8) and (9) reports no background, only a mist. In all other cases he reports, as in (2): "The background is behind the big base. It is not blurry itself, but is seen through a blur;" or in (6) "The blur is so great, like a shimmer over a hot stove, that the background isn't localizable. The blur takes place everywhere, but is so marked in the little circle that you don't even attempt to see the background."

By (1) "Through the figure the background appears filmy and a little lighter than the outside background. You see this gray film through a transparent frozen-wave medium. The outside is a rough surface." This description is general for this *O*. The only change is in the degree of roughness of the surface, which corresponds with the degree of roughness of the slide.

Bs (3) "What I see through the sides is lighter and more delicate in texture than the outside background. Through the center, the background is darker, heavier and grayer than through the sides. It looks as if there was a glassy stuff over it, when I look through the center." This description holds for all observations.

Series b. In this Series we kept the slide constant, but altered the illumination by changing the level of the light. The *Os* were Bi, By and Bs.

Surfaces. Bi (1) "I can more or less see the curved surface or not, as I please. The glass extends out in a shapeless mass, or I can cut it down more or less to the surface of the cone." (3) "A little harder to see a surface this time."

By (1) "A transparent colorless material. Texture seems to be part of the background, rather than part of the sides, which are smooth and shiny." (2) "The same kind of transparent substance, smooth and rather thin;" (3) "colorless transparent stuff; very thin and smooth."

Bs (1) "Glass; transparent, and you see the stuff in the cone through them." All of Bs' reports are like this one.

Contents. Bi (1) "The whole slide is full of glass and the glass is denser in the middle; denser in the sense of blurrier. It's more conspicuous there." (2) "Everything is glassy, but not so glassy as before. The greater amount is in the middle again. This one fogs over, loses its glass and becomes a vapor or a fog." (3) "Like the last one, though it fogs more readily."

By always described the figure as "a hollow shell" filled with a "transparent stuff like the sides." In (2) this content is "less dense than in the first figure," and in (3) it is "still less dense."

Bs (1) "Glassy or icy-looking; thicker than the stuff in the sides; like what you see in paper-weights." (2) "Not quite as bulky or thick as the

other one. It looks like frozen air. It has the smooth, clear appearance of white gelatine." (3) "Less glassy-looking, less frozen looking than before."
Background Differences. As reported in Series *a.*

The surface glass is unchanged by a change of illumination. The content, however, for Bi becomes less glassy and more like a fog; for By less dense; and for Bs less glassy or frozen-looking, as the illumination decreases in intensity.

Series c. With the instruction "You are to describe what you see," we presented white slides (cardboard of very fine grain and dull surface) which we had peppered with minute dots. In some cases the dots were arranged in a pattern, while in others their arrangement was left to chance. The *Os* were Bi, By and Bs.

Bi (2) "It's like looking down on a city at night. The dots are all scattered around, some near, some far, in a glassy volume. It's what looking down on snowflakes from above when they're falling would be, only I've never done that and, of course, this doesn't move."

By (1) "The dots float around in a film, in front of the white quality, which lies away behind them. They are not all in the same plane." (3) "Instead of being a white background with black dots on it, it's black dots floating away above the white. No dots are in the white. The white is still beyond that."

Bs (2) "Some dots seem closer than others." (3) "They are not all in the same plane, but one stands out higher or lower than the one next to it."

The dots, which appear in different depths, are said by Bi and By to lie in a transparent medium. Bi calls this a glassy volume, By a film; there is therefore, something between the dots. Bs, on the other hand, does not mention a medium of any sort, but merely sees the dots in different depths.

Series d. Following von Frey, we photographed a number of simple objects and presented to the *Os* (Bi, By) identical instead of stereoscopic pictures. We used the simple and complex geometrical forms which are described later under skeleton forms,[22] transparent glass paper-weights, a rectangular chalk-box, a small toy pig, and a small toy horse. The pictured objects measured not more than 3 cm. along the longer side. There were 24 slides in all, 12 printed upon dull, 12 upon glazed paper. The skeleton forms appeared outlined in black on white and on gray backgrounds. The paperweights, the box, and the toy animals stood upon a light gray surface before a dark gray wall. Shadows were present only in the pictures of the toy animals.

Bi. This report (rectangular parallelopipedon) is typical of the reports for skeleton forms: "The divisions of the instruction don't seem to apply here. The figure isn't tridimensional, but flat. It is set in the glass, which you would have just as well without the figure. It doesn't make up the figure." For the solid forms the report was (pig): "A pig in ice; a lot of glittery glassiness there, but it doesn't belong to the pig. He is set in the glass, though he doesn't belong to the glass. His sides are made up of dark and light shading." Bi does not report a difference in glassiness for the dull and the glazed slides.

By. The following report (triangular pyramid) is typical for the skeleton forms: "An outline figure; without surfaces or content; like a picture of a figure; not tridimensional. The whole field of vision is filled up with a glassy stuff which is quite dense. None of this glass is inherently bound up with the figure. The glass is very fine and colorless." (Horse) "Between me and the animal there is a lot of glass which is very dense, colorless, al-

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[22]See p. 271.

most lustrous, with little white flecks suspended in it. But the horse looks like a picture, not like a tridimensional horse. The glass doesn't belong to the figure." The glass of the dull background is described as "lighter in shade and not so thick, less body to it, than with the shiny background."

Although the figure itself is flat, there is no lack of glass in the field. The *Os* agree that this glass is present over the visual field without reference to the figure, which is set in it. By definitely reports flecks, and their presence may again be inferred in the "glittery" glass of Bi. The character of the glass over the dull and the glazed backgrounds is unchanged for Bi, but By finds a difference.

Series c. We presented to the *Os* (Bi and By) some of the slides of Series *a* and asked them, when they had reported the "shimmery or glittery" glass, to describe what happened when they teased this glass with a fine steel knitting needle (size 20), held in the right hand.

Bi "It doesn't change it. It looks the same. The needle is just in the glass."

By "It destroys the whole figure, because I get to looking at the needle. I don't think it destroys the glass though. I think the glass stays."

Although the figure for By is destroyed, for both *Os* the glass itself is apparently unchanged. The needle is simply thrust into it.

General Summary

These reports are very much like those of (1). Glass is present for all the *Os* in all observations, except for Bi and Bs, Series *a* (C 8, 9) and Bs, Series *c*. Bi and By do not actually see surfaces, though for By they are 'there' but not directly observable, and for Bi they are 'there' by implication. Bs reports a transparent side. That the background affects the content is again indicated by Bi, Series *a* (C 8,9) and Bs, Series *a* (C 8,9), as well as in Series *b* by all the *Os*. Again there is no direct report of flecks.

(3) *Experiments with the Tropostereoscope*[22a]

Series a. We used red glass discs upon which pairs of white circles were so placed that with different settings of the instrument a truncated cone of varying height appeared. There were 8 positions in which the cone pointed toward the *O* and 6 in which it pointed away from the *O*. The longest cones tipped slightly up or down. The *Os* were A, Bi, By and H.

Surfaces. A. Whether the cone is truncated toward or away from her A's reports are in general the same. (2, truncated toward *O*) "I can't say much about the sides. They must be there, but I can't see them." (6, truncated away) "Something holds the two ends together, but I don't know what it is."

Bi (1, truncated toward *O*) "The natural way seems to be for the red to fill up all the space between the two circles." (4) "As if I took tar and stretched it out with a little ring. Then the small circle would be flat, and the other any near-conical surface." (8) "The boundaries are like rubber, if you had a little ring in it and stretched it out." (1, truncated away) "The red bridges the difference between the two circles, as if I had a curtain and just pushed it back." (6) "A dense red film forms the walls of the tube."

[22a] Since this work was completed, we have taken some observations with the Sanford model of Helmholtz' telestereoscope. We found nothing new, and therefore did not extend the observations to a complete series. The instrument does not furnish compulsory conditions for the perception of glass.

By (3, truncated toward *O*) "The sides are a colorless film. You don't seem to see them as much as you are aware of them." (4) "Now I get a very dusty sort of thing, a film. It is boundary, and is fine and small. Dust is a bad word." (1, truncated away) "I get two circles and a red film which is everywhere between the circles."

H Truncated toward *O*, the side is generally described as "quite sharply defined"; (3) "crystal-like"; (1) "light and flinty." (2) "The front surface is glassy." (6) "Transparent or nearly transparent." (4, truncated away) "The sides are very definite on the inside, but the outside is unknown to me. I see the inside, not the outside." (5) "Rather hard and glassy-like."

Contents. (1, truncated toward *O*) "The red is at the bottom and you see into the figure." (2) "The content couldn't move, it is opaque." (5) "Jelly-like, but softer than jelly." Truncated away from *O*; "No content."

Bi (4, truncated toward *O*) "The red shapes itself into a film cone." (6) "A vaporous, thin, sparse film." (8) "The film is not hard or glassy, but soft, steamy and vaporous." Truncated away from *O* the figure is empty. (4) "No sparkling glass here, but a fluffy film color, that looks soft instead of hard as the glass did."

By (2, truncated toward *O*) "One minute I think it is filled with a colorless substance, and the next I think it is filled with a red substance. The filling is a smooth, transparent medium, without those little waves in it." (4, truncated away) "No figure. I get two circles in a red film which is everywhere between the circles."

H. Whether it is truncated toward or away from him H sees a solid figure. (1) "Slightly pinkish perhaps, though it might be perfectly transparent with the red beyond." (2) "The figure reflects a color rather than being colored itself." (6) "Glassy-like; a bulky mass." (3, truncated away) "Lively, and I get this red showing through it."

Background Differences are not reported. The background for all the *O*s is simply a film.

Only one *O* (Bi) definitely reports a colored filling. A and By again find the indefinite colorless side, and H continues to see transparent, glass-like bounding planes. For Bi the sides are a red film. We note that Bi (C 4) remarks the absence of glass and that A and By do not report its presence, while H describes both surface and content glass.

Series b. We used a combination of two metal discs each of which had a 3 mm. strip cut out along a radius, and two blue glass discs, to get a blue line upon a black background. With different settings of the instrument, the line varied its position in relation to *O*. It tilted away from or toward in 4 positions and in the fifth position became a horizontal bar. The *O*s were A, Bi, By and H.

Surfaces. A and Bi do not see a surface. "The blue line is a film." Its edges are for A (1) "not sharp; blurred but easy to find"; (4) "soft"; and for Bi (3) "blurred, but fairly sharp"; (4) "the part near me is sharper than the part away from me."

By reports every time that "the line is distinctly surface." (1) "The outlines are not well defined. They are blurred by the suggestion of a mist which is over the thing." (4) "The near boundaries are well defined, and the near end is more vivid in color. But the far boundaries are blurred. It fades away into quality almost without shape. The back end is soft and mellow."

H (1) "As a bar of light, the front surface is indefinite; as a rod it has a pretty good, fairly definite front surface." (5) "The boundaries around the thing are fairly sharp, but a little fuzzy. There is a sort of halo around it."

Contents. A (2) "A blue luminous stuff." (4) "It is vapory, spongy, and it has no dimensions."

Bi (1) "The blue film." (3) "The blue film is more localizable than the black, because it doesn't have any room to get bulky."

By (1) "No contents. The line is just suspended in blackness. There is a colorless shimmery mist through which you look at the line, but you can't see whether this mist is a wall or has depth."

H describes two contents. As a rod (1) the content is "a rather luminous glassy mass"; (3) "hard, smooth, glowy and glassy-like"; (4) "blue, quite dense, translucent, and glassy-like." As a bar of light (3) the content is "softer and fluffier than when it is a bar"; (5) "less obviously tridimensional."

Background Differences as in Series *a*.

A and Bi do not see a surface, and H finds it "indefinite" or "fairly definite" (S1). The *O*s (except By, for whom the line is a flat surface and therefore without content) seem to agree as to content: *i. e.*, the non-dimensional stuff of A is fundamentally the same as the film of Bi and the fluffy filling of H. H, however, reports a change to a harder content under a certain perceptive attitude (C 3). H is the only *O* who reports glass (C1,3,4) though A says that the content is "luminous" (C 3) and By mentions a "shimmery mist" (C 1).

Series c. We used 3 yellow dots in the same manner as we used the blue line in Series *b*. The positions of th... of dots in regard to *O* were identical with those of the line. The *O*s were A, Bi, By and H.

Surfaces. A (2) "Too glowy to be a surface, yet there is a suggestion of surface there." (3) "A film, but almost a surface; too cloudy and too luminous to be a surface."

Bi (5) "The yellow is a film; without form except that it is flatter, more localizable, and more surfacy than the black."

By (1) "The dots have a smooth white surface." (5) "At first the dots are well defined, surfacy, lustrous dots, but then they grow dimmer and less well defined. Then they are patches of white without lustre, seen through a mist." (6) "In front of each dot there is a colorless film which isn't there at first. I don't really see the film, but the dots get ill defined as if you were looking at them through a mist or something."

H (3) "The dots are flat bits of brightness." (5) "The dots have a fairly definite surface, which is hard, glassy-like and dense." (6) "The boundary between the yellow and the black is fuzzy."

Contents. A (1) "Luminous yellow." (2) "I can't see into it as well as I can into most films." (3) "A film, but almost a surface; vaporous; looks like light shining through it."

Bi (1) "A yellow film fills the holes." (2) "Except for the quality, it is just like the black." (5) "The yellow is without form."

By (1) "I can't see any content. It's as though the spheres were made of paper."

H (1) "A glowy mass." (2) "It has the density of a bulk color, but I can't see through it." (4) "It ought to be bulky but it isn't, because though it is bulky it isn't transparent;" "(3) "translucent and alive; jelly-like, fiery and brilliant, but not a flamey, flashy, leaping flame"; (6) "glass-like."

Only one *O* (By) does not see a content. For A and Bi this content is a yellow film, with a hint of surface; for H it is a glowy mass with a definite surface. Both content and surface are glassy for this *O*. By sees only a smooth surface which becomes ill-defined, suggesting to him that there is a mist over it.

General Summary

In these experiments only one *O*, H, reports glass. By mentions a mist in Series *b* (C) and Series *c* (S) which might be considered glassy, in that he says it "shimmers." In the same way the "luminous content" of A may also indicate the presence of glass.

(*B*) *Experiments with Skeleton Forms*

The forms which we used in these experiments were (1) a set of cones and pyramids, (2) a set of simple and of complex geometrical skeleton forms, and (3) a set of irregular ridges. To avoid as far as possible the formation of contrast lines, we painted the outlines a dull gray. We performed the experiments in the dark room where we suspended the figure by means of an invisible thread from a carrier. The *O* viewed the object through a 25 × 35 cm. opening. Except in those cases where we wished to determine the effect of changed illumination, (1,*h*;2,*b*;3,*a*), the light—a 200 watt Mazda lamp, reflected through a Gage 'daylight' glass which was faced with a double sheet of tissue paper—stood in front of the figure, toward the right, and well above it, so that the figure was uniformly and moderately lighted.

(1) *Experiments with Skeleton Cones and Pyramids*

Series a. We had 5 circles and 5 squares made of 1/16 wire. The diameters of the circles and the sides of the squares ran from 10 to 30 cm. by 5 cm. intervals. Thus it was possible to form a truncated cone or a truncated pyramid by hanging in the carrier one circle in front of another circle or one square in front of another square. The length of the figure and the degree of truncation depended upon the size of the circles and squares and their separation, which could be varied from 15 to 90 cm. *O* sat 4.5 m. from the opening in which the figure appeared, and 5.5 m. from the base of the figure. We presented 6 figures, with a black valvet background. A, Bi, and H acted as *O*s.

Surfaces. A (1) "The sides seem to be there until you look for them, when they dissolve so that you can't find them at all; a soft film, but I can't say whether it is transparent or black. The film is static, but being static doesn't make it more definitely where it is, but it seems there between the circumferences of the two circles and I can't get away from it." (3) "I can see where the sides should be. The small enclosure is almost coal black. It looks like very dark liquid or clear rock. It's congealed." (4) "The sides are lighter and more filmy than the smaller square. This square is hard in that it looks glazed, though it's a kind of film, too." (6) "I can't tell whether the sides are film, or whether the whole thing is film." (2) "The small circle looks like the mirrored reflection of black velvet which shows the glaze of the mirror and the velvet too."

Bi (1) "The sides are a sort of black film, as if the black film of the background were pulled up to the small circle." (3) "As formless as an after-image." (5) "In the little circle it's like looking into a black box." (6) "Most of the time the two bands are tied together by a dark filmy grayness, so that a film lies outside the little circle, and inside the big one. Though it isn't a surface, it seems to be stretched there."

By (1) "You would say that they are black, but when you look there is no side, yet it has the meaning of cone." (2) "The sides are black without surface or texture, they are just extent." By twice reports (1,2) "no front plane"; and except for one other figure (3) he says: "A mist comes up and fills the window so that the whole thing lies behind the transparent shimmery stuff, and I can't tell about anything on account of this." In (3) he sees "the front end more surfacy, blacker, and more shiny than the other surfaces."

H (1) "The sides are a practically transparent gray." (2) "The surface belongs more to the sides than to the top and bottom. The side boundaries are very definite." (3) "The front surface is less certain, less clearly seen

than the side surface, yet something in the front end gives a hint of surface." (4) "It has a definite beginning-place on the outside, and the front is more absolutely transparent than the side surface."

Contents. A (1) "As if the whole figure were made of a thick ink that had congealed." (3) "The thing is not transparent but opaque, and yet not perfectly opáque; misty opaque so that I can see inside a certain way." (5) "The contents seen through the smaller base are bulky, almost liquid looking. Seen through the sides they are doubtful. It might be a hollow figure covered with film."

Bi is not sure of the content. (6) "Blackness, like looking into a black box"; but (5) "it may be hollow or it may be filled with black, but I don't see anything."

By (1) "Just blackness, as if you were looking inside a tube or cone." (2) "The black of the content is smoother than that of the blundaries; textureless and pre-dimensional." (4) "A cloud of intense black." (6) "It seems hollow, but filled with black."

H is very doubtful about the contents. (1) "I know nothing about the inside of the sides. It doesn't stop anywhere, and yet I see it. Perhaps it stops at the bounding surface." (2) "My impression is that the side stuff fills the whole mass of the figure, but I see it at the margin." (3) "The whole thing seems filled up, yet all I can see as quality belongs to the surface. It means filled, but I can't see it."

Background Differences. A always reports "a filmy surface unlocalized with reference to the figure." Bi always reports "a black film." By always reports "unlocalized blackness." H says that the background is not definitely localized. (1) "The background outside the figure has a vague hint of something more substantial, more real about it." (3) "Through the small circle the background is very black. Through the side it is blurred, but still pretty black as background." (6) "A perceptual divorcement of gray and black. The black is very black (background) though actually blurred over with the gray (figure)."

As in the stereoscopic experiments, A and By report the indefinite sides. Bi pulls the film up from the background, and H alone sees definite surfaces. There is a difference between the sides and the front surfaces, and the content is different for different *O*s, though Bi and By agree that it is blackness if it is there; and A sees the content differently through the sides and the smaller form. H is the only *O* who connects background and figure, and he notes the curious relation of the two in D 6. A suggests glassiness or iciness in the "glaze" and the "congealed surface" of the small form (S 2,3,4) and perhaps in "congealed ink" (C 1.) The transparent shimmery stuff which fills the window for By (S) and the shiny surface (S 3) may also indicate the presence of glass.

Series b. We used figures from Series *a* with black velvet, gray cardboard and black cardboard backgrounds. *O*s were A, Bi, By and H.

Surfaces. A (1) "I can see where the sides should be, but I can't see them."

Bi (1) "The small ring is filled with a black film, which is unlocalizable in that it is not a surface. It's cupped, too, instead of being flat." (2) "The gray is not observably in the ring, but it's not seen out of it either." (3) "The implication is that the film filling the small ring comes from the background." The only difference in the reports is that the tint of the film varies with the background. The "black film" in (1) is a "gray film" in (2). (3) "The sides aren't there as surface. It is like a cone turned out of a film that is so vague that it hasn't very definite limits laterally."

By. Again the only difference from report to report is in change of tint. (1) "As a whole the figure is a truncated figure with black smooth sides; but when I try to describe them, there is nothing there." (3) "A suggestion of

looking through some sort of a transparent medium which is observable on the sides of the figure and over the bases. It is shimmery, colorless, and of fine texture. It seems to move." (4) "This medium is not exactly a mist, but more a transparent film." "At times the front end is a shiny black surface, which is a very deep and intense black." This last report came only once, for (1).

H (1) "The front end is slightly grayish, but less grayish than the side. It is poorly defined." (4) "Less stuff given in the front end." Even with a black background the sides are reported gray. (1) "The sides are filled up better than the front end. The gray is certainly along that side." (2) "The gray of the side is a sort of bulky volume; a transparent gray which is sharply defined on the outside." (3) "The thing is gray and surface, and somehow the surface is dominant."

Contents. A (1) "A film. I think it is light gray, but I can't be sure, because I see the background through it." (2) "The film-content looks congealed like ice, though there is nothing glazed about it. The particles are held together, fixed; a frozen mist, softer than ice or glass; like transparent ice-cream." (2) "Through the small circle this film is glassier, harder, more jelly-like and bulkier than when it is seen through the sides." (3) "Through the small circle the film is like a liquid mass." (4) "Ice and glass are too smooth for the content. You see what is behind it through it."

Bi. The contents differ only with regard to tint. With a black background the filling is black; with gray, gray. (1) "The contents aren't there any more than the surface of the circle. The black is everywhere, except outside the conical surface." (2) "The gray filling is the smokiest kind of dense fog." (3) "Over the black I get a shimmery effect like a dance of heat waves, and over the rings it is like a lot of fine threads. It is wiggly and mobile and is the nearest I come to the stereoscopic glass."

By (1) "I can't tell about the content because of this mist across the front." (3) "It may be filled with black quality like a tube, or it may be just a plane front; you can't tell." (4) "It may be filled with a colorless, misty medium through which you see a black medium."

H (1) "Gray, which is cloudy-like, and lighter in some parts than in others; mobile and alive." (3) "Cloudy; I mean that the gray itself is denser and rarer in some parts, though the denser parts appear lighter in quality and tint." (4) "Certainly the figure comes to me as solid and the contents are furred. I am inclined to take it as a bulky mass, because all the gray that I see belongs to the figure and not beyond; yet all the gray is transparent."

A, Bi and By all have difficulty with the sides. A sees where they should be; Bi sees a film, but nothing as surface; and By sees the sides as gray, black or colorless, but when he examines them they disappear. Bi apparently fills the small form from the background from which, too, the side film takes its tint. By, when he sees the sides as tinted, also takes their tint from the background. In only one case, in one observation, does he find a surface for the small circle (S 1). H reports a definite gray surface which is not affected by a change of background; and the small circle is like the sides except that it is poorly defined and less gray. A does not know whether her film-content is itself tinted or whether it gets its tint from the background which she sees through it. For Bi, the foggy content, which is just as indefinite as the sides, varies its tint as the background varies. The mist which fills the window makes the content difficult for By to describe. He cannot say whether what he sees is really a bulk (which is either without tint or with the tint of the background) or a plane. The content for H is always gray, but in the same figure the gray is not uniform. A reports a kind of glass in which there are particles which are held together. It appears different through the small circle and through the sides. Bi reports

an approach to stereoscopic glass (C 3) with the same simile which he used in the stereoscopic observations. Somewhat like this is the shimmery medium which By describes, not however as content (S 3).

Series c. We used 8 skeleton figures, each one with part of the outline blotted out.[23] We obtained this effect by wrapping a section of the wire with a black velvet band. The bands were 2.5, 8.75, and 25 cm. wide. The *O*s were A, Bi, By and H.

Surfaces. A (1) "Something connects the two circles but I don't see it as boundary." (2) "The sides are like mist laid around the sides. I don't really see it as boundary. The front plane is almost like glass." (3) "Where the piece is cut out it is dull, heavy, and dark, quite different from the figure, which is light and soft." (4) "The planes are filled with something like gelatine paper." (7) "It is just there and I can't see where it begins and ends." (8) "If I take the figure as a hollow tube, the sides are filmy and gauze-like, but if I take it as a cone with a piece cut out of it I don't see any side plane." "The disc seems cut out of the small circle and makes this circle filled with something more surfacy but yet it is not quite a surface."

Bi (1) "Sometimes the sides are black and solid, sometimes they thin out and become less dense." (4) "The little circle gets so solid that it bulges. This solidifying does not apply to the content as seen through the smaller circle but only to the surface of the circle." (7) "Where the piece is cut out the contour of the cone surface is interrupted. It destroys that part of the surface, but the rest of the cone is all right." (8) "Nothing definite in the cut-out part except black."

H (1) "The sides are neutral gray in tint." (3) "Though a piece is out of the boundary, the side is uniform wiht adjacent parts of the cone." (5) "At the gap the gray breaks off along a fairly definite line, though there is not a definite line there. It fades quickly in a fairly straight line into black." (7) "If I don't fixate the cut part, the figure shows a gap as if this side were simply cut off. There is less of the gray in the front end than in the sloping surface, but it is of the same brightness." (8) "If the gap is filled in, the gray in that part is like the rest of the boundaries, but it is not a constant gray. It changes in amount, softness and density, and it is lively. When I get something like a glassy, flinty, hard, almost reflecting surface there, the gray is more stable and less transparent." (1) "The front end is a little harder and more glassy and not quite as soft or as suggestive of depth as the sides."

Contents. A (2) "I can't say what is behind the mist-sides, if anything. It is a solid cone, and I see into the content a little way. The content in the small circle is much more solid looking and more liquid looking." (3) "Through the small circle the content is almost liquid." (4) "I see solidity without seeing solid content. It is liquid and viscous." (6) "Just about filmy." (7) "A soft mist-like stuff, lighter than the background and the stuff in the smaller circle."

Bi (2) "I don't know about the inside, I see the outside, yet through the little circle I get the impression that I am looking into a lightless space." (5) "The little circle always has black up to the mouth of the cone." (6) "The black comes up to the little circle like night to a window."

H (1) "I take it to be solid yet there is the same old dilemma; I don't see the stuff inside the figure." (3) "The figure with the chunk out is just as much filled as the one with complete boundaries, only part of it is lopped off." (5) "Casually it is hollow. It solidifies in terms of the front surface." (8) "I never have seen anything inside the figure. I just take it to be solid, because of the transparent sides that I can't see any margin to."

[23]We owe the suggestion of this variation of the experiment to Professor H. C. Warren of Princeton University.

It is surprising that the elimination of part of a boundary has so little effect on the figure. We rather expected that the *O* would look into the content of the figure through the gap thus provided; but in no case did this happen. The surfaces for A continue to be indefinite and misty, and the front circle again approaches glass. While the piece cut out makes the front form more surfacy, it still is not quite a surface (S 7). A reports a marked difference in the sides when she takes the figure as a cone and when she takes it as a tube (S 8). Bi departs from his usual report when he describes the sides and the small form as solid planes (S 1,4). H still sees the gray sides and the slightly less gray front form, to which he now ascribes a glassiness (S 1). H is the only *O* who sometimes fills in the gap in the side of the figure, so that there is a continuous surface; and even then, there is a difference between the grays (S 8). All three *Os* find that where the piece is cut out the figure simply does not exist. The reports of content are practically unchanged. A still sees the content differently through the small circle and through the sides. Bi again reports "blackness" although he is not yet sure that this is really content (C 2). H has his constant difficulty; he does not see a content, but because the sides have no inner margin, he takes the figure as solid (C 8).

Series d. In this series we used only one cone (a 10 and a 25 cm. circle with a separation of 45 cm. before a black background) into which we introduced a fixation point successively in the following positions: in the front circle; in the back circle; on the background; in the back circle which had a section blotted out by an 8.75 cm. black band, at the same time that the front circle had a section blotted out by a 2.5 cm. black band; and in the front circle which had a section blotted out by a 2.5 cm. black band. We added to the instruction "Please fixate the point which has been provided." *Os* were A, Bi and H.

Surfaces. A reports once: (3) "The sides look as though they should be glassy, as though I were seeing the background through them. It has a glazed character in spots, *i. e.*, it looks like glass and means glass." For the rest of the figures A says: (1) "I can't see the boundaries. I see where they should be." (2) "The sides look filmy, but they are hard to see as boundaries." (3) "The small circle is glassy. It doesn't seem to belong to the sides. It has the glazed stuck-together look of glass, not glass as a bulk, but as a thin sheet through which I see the fixation point." (4) "The small circle is seen as the surface of a solid. It is denser and more congealed than the side." (5) "Where the plug is cut out it is black."

Bi (2) "Nothing substantial links the two circles together; a transparent shell that lets the black show through it." (3) "Not side surface, but I can force a gray shell which is visible but not substantial." (4) "I can't quite see where the gray surface (the unsubstantial shell) meets the black, where the part is missing. Where the part is missing it looks like background and not like surface of a cone." (5) "A light and a dark surface meet (gap and sides), but it is not quite like a black insertion in the surface; more like a tearing out, and the blackness is there."

H (1) "The sides and the front end are gray. The front end is filled with gray which is thin, flinty, glassy, yet a little soft. It tends toward a bulk." (2) "The sides are flinty and dense." (3) "If I take it as a cone the sides are softer, fluffier, more filmy, and not so bulky or glassy as if I take it as a tube." (5) "With the fixation point the sides seem less suggestive of tridimensionality. They are more definitely gray, glazed surfaces. Where the circles are broken there is no gray, just a black background, which does not fill the gap but is beyond. The front end is filled with gray, but it is transparent; glassy, flinty, and dense."

Contents. A (1) "Filmy." (2) "I take it as a solid cone and it would be, only there isn't enough stuff inside to fill it up well. There is something there but not enough." (3) "Through the small circle it is hollow. Through

the sides it is filled with something glazed, transparent, and formless." (4) "I don't see it as a bulk though, it just fills in. The small circle seems to be the surface of a solid."

Bi. For this *O* the figure is filled only when the fixation point is in the front circle. (1) "A bulky figure made up of homogeneous black that has limits but not a surface." (2) "The fixation point hollows out the cone." (3) "The fixation point pulls the black back with it."

H (1) "It is variable; it is solid and it isn't." (4) "Most of the time it means solid, but I can't see any content. I see only surface gray, and I think it is hollow." (5) "All I see is surface, but it means a complete solid."

Except for the one case (S3) where she reports sides that look like glass, A finds the same trouble in seeing the boundaries, though she sees where they should be. The small circle is again different from the sides and appears as a thin sheet of glass (S 4). Bi returns to the indefinite sides, with an unsubstantial gray shell. H still sees the sides and the front end as gray. His figure appears either as a cone or as a tube. As a tube, the sides are glassier (S 3). Although the gray surfaces (both sides and front end) are soft, they are also flinty and glassy. In one case (C3), where the fixation point is on the background, A says that the figure is hollow through the small circle, while through the sides it is filled. She takes the figure as solid, though she cannot describe much of the content. Bi reports the indefinite blackness for film only when the fixation point is in the front circle. In other cases he finds that the point hollows out the figure. H is undecided about the content. He still sees only surface, and yet the figure means solidity. For all *O*s the cone simply does not exist in the place where a piece is blotted out.

Series e. We repeated Series *d* with a 10 cm. circle suspended 90 cm. in front of a 30 cm. circle before a black velvet background. The *O*s (A, Bi and H) sat at a distance of 2 m. from the opening in which the figure appeared.

Surfaces. A (1) "I see where the sides should be, but I really don't see them as plane surfaces. The small circle is a film, but it is hard, smooth, more definitely in one plane and more suggestive of a solid." (2) "The small circle is transparent and glassy." (3) "If I look through the small circle the cone is a hollow cone of glass. It is good glass, too." (5) "A dense, dark, film strip stretches from the small circle to the large circle. Still, I don't see this as a side surface." (6) "The sides are filmy, vapory; not distinct at all." (7) "The small circle is filled with something really glassy; transparent and slick like glass." (10) "Where the piece is out there is a black blotch as if the background came up and filled the side in."

Bi says that the sides are either gray or black. He reports the presence of a "gray halo" for half the presentations. (1) "The sides are a film of blackness." (2) "The side wall is a black shell, like paper in shape only." (5) "There is a gray halo that forms a surface except over the gap. The front circle is almost full of black." (8) "It is a gray shell." (9) "Where the ring is broken there is just blackness, like a piece torn out of the wall."

H (1) "The sides are not definitely seen. A certain amount of gray lies in between the circles very indefinitely. It seems at times to go into the back circle." (3) "The sides are grayish but not uniformly so." (5) "The front end is definitely filled in with gray. The sides are not very thick neutral gray, but they mean a definite surface." (6) "With sharp fixation there are no sides, but with less active fixation there is a certain amount of gray filling, which takes on a directional aspect more or less between the two circles." (8) "There are hints of gray in the sides; a gray that is not dense or hard, but fluffy and soft. By 'hint' I mean that the relation between the two circles is a meaning of relation rather than a matter of seeing. If I take it as a tube wall it is more definitely and uniformly gray. It is harder,

glassier, less fluffy and more definitely in a plane." (10) "It is slightly cloudy and not very definitely in a plane, but it carries the meaning of side." (9) "The big circle is more or less filled out in its own plane; just a blur of neutral gray."

Contents. A. Where the fixation point is not in the front circle *O* says: (3) "Through the small circle the figure is hollow. Through the sides the content is filmy, glassy, hard, and transparent." With the fixation point in front: (1) "Through the small circle the content is filmy, more suggestive of a solid, smoother, harder, and more definitely in one place than through the sides." (5) "Through the sides it is filmy and unlocalized." (6) "Through the small circle it is like the sides but not so soft; more sheen to it; a sort of slickness." (10) "Vapory and filmy."

Bi. When the fixation point is at the back the cone is empty. (2) "For a second, there may be the sheer empty transparency of stereoscopic vision —that glassy filling; but this doesn't last." (7) "The fixation point pulls the black back out of the cone." When the fixation point is in front the figure is (6) "a black jelly," (9) "just blackness," (10) "a black solid."

H (5) "I take it as solid, but everything seen is localized in the surface." (8) "I can't say that it is hollow or that it is filled."

The reports are very much like those of Series *d*. A sees sides but not surfaces, and the disc is glassy. Where the fixation point is on the background (S 3), she again sees the cone through the small circle as a hollow glass shell. The sides for Bi, while they are gray or black, are films and not surfaces. The sides for H are not so definite as before, but they are still gray, and mean a surface (S 5). When the figure is taken as a tube they are less fluffy and more glassy than when it is taken as a cone. The small circle is still definitely a gray surface, and in one case (9) H says that the large circle is more or less filled. The break in the side is filled by the background, that is, the cone does not exist at that point, for any *O*. Even where the figure is empty through the small circle (C3) A sees a content through the sides. In the other cases there is the same difference in content through the small circle and through the sides, and even though it is filmy, it is also glassy or "slick" (C 3,6). Bi finds a black content only when the fixation point is in the front circle. He still says that the point, when it is set back in the figure, pulls out the filling, though he now gets a glimpse of a glassy filling line like that of stereoscopic vision (C2). H's content is unchanged. He sees only surface, but he takes the figure as solid.

Series f. We now introduced, by suspension from the carrier, 6 barely perceptible flecks, made of beeswax and painted gray, into the cone in such a way that some were scattered inside the cone and some on the surfaces. The *Os* (A, Bi and H), again seated 4.5 m. from the opening in which the figure appeared, saw the figure against a black velvet background.

Surfaces. A (1) "The sides more or less follow these little dots which round out the boundary. Where the dots aren't around, the boundary is vague, and I can't tell where it is. It is vapory." (2) "Where the dots are near the circles, I can almost see the boundary. Where the dot comes into the boundary it is less transparent, though there isn't more stuff there."

Bi (1) "Black, but this black is not localized definitely on most of the circumference." (2) "Sometimes I think I see the outside of the cone as a little lighter than the inside."

H (1) "The side is neutral gray. It is translucent, and two little spots seem to lie in, to be a part of, a bounding surface." (2) "The side is very sharply limited, bulky but soft, and rather transparent and glassy. There is a small amount of gray there, so that it's a very weak positive experience of grayness." (3) "Though it is transparent, the side blots out, obscures the background." (1) "The front plane is slightly grayish, but not as gray

or as soft as the side; little more like a surface, though more transparent than the side." In only one case (3) H says: "no back or front plane."

Contents. A reports once (3) "Through the small circle the content is like the mist that's outside, but it's more dense, colorless and transparent. Through the sides, it's more misty, less transparent, and gray." Usually A says "a glassy liquid mass."

Bi (1) "The cone has substance which is more like the clear glassiness of the stereoscope. This jelly is not as sparkling and glittery as that in the stereoscope, but it's the same bulkiness." (2) "A black jelly (not a film black), or a clear thing that I see the black through." (3) "The cloudy black has come back again; clear up inside the cone. What I see in the small circle is much blacker than what I see as surface." (In this case the fixation point was in front of the figure.)

H sees only one figure (where there is but one dot in the cone), solid. (2) "Perceptually it is taken as a solid, but I really don't see anything inside certainly; a suggestion that the gray from the sides extends indefinitely inward, but I don't really see it." (1) "The flecks inside the figure make it more definitely empty."

For A the flecks seem to help in the formation of the sides and in their localization; while for Bi and H they do not affect the surfaces. For Bi the surfaces are of indistinct black, for H they are gray, and at the same time rather transparent and glassy. The front plane for H is different from the sides, in that it is "less gray, less soft, and more like a surface" (S 1). A and Bi definitely report content glassiness (C 1), and the presence of a glassy medium might also be inferred for H (C 2), since he describes the surfaces from which this gray extends as glassy (S 2). A and Bi agree upon the figures whose content they describe as glassy, and when the flecks are scattered in the fore part of the figure, both report a misty or cloudy filling, through for A this mist is colorless through the small circle, and gray through the sides, and for Bi it is a cloudy black. For Bi "the dots keep the black film out," and for H they make the figure "more definitely empty." A does not report any effect of the dots, but since her reports are very like Bi's, we may suppose that the effect was the same for her as for him. Even when H sees a solid figure, he says that this is perceptual, and that there is really nothing given. It is to be noted that the "glassy filling," which Bi now finds when the murky blackness is kept out of the figure, appeared fleetingly in Series *e*, Bi (C 3), under like conditions.

Series g. By turning *O*'s chair slightly to the side, and asking him to fixate a point placed directly in his line of regard and therefore to the side of the cone, we obtained (adding to the regular instruction: "Please fixate sharply the point which has been provided") a report of the cone as it appeared in indirect vision. The distance of the point from *O* varied, so that in some cases the cone was beyond the point of fixation, and in other cases the fixation point was beyond the cone. *O*s were Bi, By and Bs.

Surfaces. Bi does not see a figure. (1) "I would infer that there is a cone over there, but I don't see it. All I see are two rings with space between them."

By (3) "The thing does have sides because it's a cone, and it has sides to it as a cone should have, but when you try to describe them it hasn't any, but even then it still has the cone character. These sides are black." (2) "Black sides if you don't look for them."

[illegible]

is a haze or a cloud." (2) "The front hole is black, and it looks deep and bulky, like heavy black smoke, but it's really just around the top part. It doesn't fill the whole cone."

Contents. Although he does not get a figure, Bi says. (2) "They look like hoops hung in this stereoscopic jelly. This jelly is perfectly clear with

respect to the murkiness in it. I think it's black or else it's clear like water and I see the black behind it." (3) "I wouldn't attribute the black to the jelly. I think the jelly is colorless glassiness, and the black is behind."

By (1) "I don't know whether it's filled or empty. It may have a deep rich black inside, like the blackness in a tube, or there may be just a plane surface across the top. This surface is, in this case, black." (2) "The black of the contents, if it has any contents, is deeper and richer than the black of the sides."

Bs (1) "I can't say whether it's filled or empty." (2) "It seems filled sometimes with a blackness, but that gets away from me when I try to describe it and then it seems empty."

Bs sees the side surface as a gray cloud and the front surface as a black smoke. Bi does not see a figure at all, although he could infer a cone; and By sees black sides which will not bear inspection. Neither Bs nor By can say whether the figure is filled or empty. Both suggest a filling of 'blackness' which disappears for Bs when she tries to examine it, and which for By may be merely a black surface. If there is a content, both *Os* report it as blacker than the sides. Bi is the only *O* who reports glass. While he does not get a figure, he sees the circles hung in a 'stereoscopic jelly' (C 2). He is not sure whether the clear jelly itself is black or whether the black is behind it, but he is inclined to believe that the jelly is a colorless glass.

Series h. The *Os* (A, Bi and H) sat at a distance of 4.5 m. from the opening in which the figure appeared upon a black background. We used the figures of Series *a*, but altered the illumination, so that the figures were illuminated from different positions and with different lights. They were directly illuminated from the front, from below, and between the circles and at the front by the light from a 200 watt Mazda lamp, which was diffused by means of a single layer of white tissue paper; and they also appeared in daylight. Lighting from the back or from the side proved unsatisfactory, as then part of a ring or an entire ring was in too deep shadow for observation. In some cases we introduced a fixation point, as in Series *d*, and then added to the instructions "Please fixate the point which has been provided."

Surfaces. A (1) "Not really there though I see where they ought to be." (3) "If it had boundaries, it would be slick, I think."

Bi (1) "The sides are still there, though they're not so conspicuous as before." In only one case *O* reports (4) "a black shell."

H (1) "The sides are a neutral gray, very thin and transparent. Perceptually it is a surface, but not one that I could put my finger on. The side where I look is always grayer, but when I look to see if it's there, then it's indefinite." (2) "The little circle is glassy-like. It's like the sides, a transparent, grayish surface, yet not a surface." (4) "The side is a little loose, indefinite in localization, but taken as a figure it's definite enough."

Contents. A (1) "Through the sides the contents are filmy, but slick-looking. Through the small circle they are liquid looking with something slick about it." (3) "It may be colored but, if it is, it's perfectly transparent."

Bi (1) "A black filling which is soft, foggy, and bulky." (2) "More like black air than like black smoke." In case (6), where the fixation point is at the back, *O* reports that the figure is a "hollow shell" and adds: "By hollow, I just mean free from black. It is a clear filling."

H sees only those figures as solid which are without fixation points. The others are empty. (1) "I take it as solid, but this is pure meaning. I'm sure there is no real experience given that is localized in the figure. Anything approximately localized appears at the surface."

A returns her usual report. She sees where the sides ought to be, and adds that if they were there they would be 'slick'. Bi and H also describe sides, which for Bi are black and for H gray; but H says that the surface,

while definite enough for the figure, is not one "that I could put my finger on." H is the only *O* who describes a glassy surface (S 2), though A uses the term 'slick' (S 3). The content for A is again different through the smaller circle and through the sides. Through both the content is slick, but through the sides it is a slick film, and through the small circle a slick liquid. A cannot say definitely that the content is tinted, because it is transparent. Bi says that the content is a black filling, like black air; and again, where the point lies at the back, reports a clear figure, *i. e.*, a figure "free from black." For H the points empty the figures, and though where there are no points he takes the figure as solid, he says that the experience which he localizes is at the surface.

(2) *Experiments with Geometrical Skeleton Forms*

These figures, made of fine wire, were outlines of tridimensional geometrical forms: a cube, a rectangular, a rhomboidal, and a rhombic parallelopipedon; a pyramid with a triangular base; a pyramid with a square base; two pyramids with triangular bases fastened base to base; two pyramids with square bases fastened base to base; and two pyramids with triangular bases fastened apex to apex, to form an outline hourglass figure. The figures measured from 10 to 20 cm. on a side. The *Os* (A, Bi and H) sat at a distance of 2 m. from the opening behind which the figures appeared.

Series a, b, c. These series were repetitions of Series *a*, *b* and *h* of Part (1), *i. e.*, simple presentations of figures on black velvet background, on different backgrounds, and in different lighting.

Series a. Surfaces. A (2) "Light gray, but I can't tell whether I see them or the background through them." (3) "A sheet or a very fine conweb, which is soft and filmy looking; like glass, but softer than glass." (4) "Though I see the sides as planes, they don't look like surfaces."

Bi (1) "The forward sides never fill up, but the black fills in the faces seen through the other sides, as a window is filled with the black of night." (2) "Sometimes I get a shimmery something over the lines and filling the base. It's active and dancing, but not sparkling or clear." (4) "The other (front) sides are just open. They are nothing but rims around spaces which might be full of something."

H (2) "The sides are all filled; definitely gray and glassy-like." (3) "The top and bottom are somehow less definite than the sides." (4) "Casually the back appears filled; critically, I can't say. All the gray is in front of it."

Contents. A (1) "I am not sure about any of the contents. The figures seem solid, but I can't tell whether there's anything behind the bounding planes or not." (5) "Soft like a film, but there's more in it, and it's stable like jelly. At the same time it's a little glazed, like heat rising off a cornfield, except that it isn't as light."

Bi (5) "I can never fill the figure with anything."

H (4) "I take it to be solid, yet so far as actually seeing the solid, this is not true." (3) "Ordinarily I don't see anything inside the figure, yet the side doesn't leave off anywhere."

Both A and H see the sides as planes. H says they are gray, but A is doubtful whether they are themselves tinted, or are tinted by the background. Bi sees planes only in those sides [illegible] sides, and then not as surfaces, but as filled with the film of the background. For A and H the figure seems solid, although neither *O* is satisfied that he actually sees content. The figure is empty for Bi. All of the *Os* [illegible] glass; A and H as surface and probably as content (if there is content), and Bi as surface over the base of the figure.

Series b. Surfaces. A (1) "The sides are made of glass but they're softer than glass." (6) "They may be gray, but I believe they are colorless and I see the background through them." (8) "This soft cobwebby stuff fills the planes; a film, not a surface."

Bi (2) "Taken naturally there are no bounding planes. The sides which are seen through other sides fill up with the background." (6) "It is possible to see the base filled with something shimmery like the glassy stuff, but it's quite transparent, like jelly or clear glass, or slightly smoked glass, because it looks blurry."

H (3) "The sides are gray; a transparent gray and not a hard definite surface." (5) "The top and bottom are softer, less dense, less gray, than the sides." (8) "I can't say about the back surfaces. The thing seems glassy, and I see the back through the other surfaces."

Contents. A (2) "I can't be sure about the filling. It may be or may not be filled." (4) "I seem to see something hazy as filling, as if you had something lively and stopped it before it had time to die." (5) "Almost like a block of ice, only it's not hard enough or thick enough to be an ice-filling. It still bothers me to call this content."

Bi (1) "The background really envelopes the whole figure. The figure is hung up in this thin smoke, so it's full of it, but it has no content as figure."

H (2) "I take it as a solid figure, which is filled, but I don't definitely see that stuff inside." (3) "It just gives the impression of glassy thickness."

The results are like those of Series *a*, except that Bi, with the gray background, sees the figure hanging in a thin smoke which fills it, but not as content of the figure.

Series c. Surfaces. A (1) "Like ice, but not so hard or thick as ice." (3) "I think that as an object the planes fill in, but I don't believe they do it so well otherwise." (5) "A congealed mist, which is stable as fine gelatine, but softer." (7) "First the figure was in outline, but when I found out what it was, I saw planes."

Bi (4) "The background fills the back faces, but the other faces are hollow." (7) "I can get a shimmery something like clear glass, or glass which is slightly smoked, but it's very light. It seems to fill the base."

H (1) "The sides are very gray. Some of the surfaces are more hard and glassy-like than others." (4) "The back surface is seen through the other surfaces and has a purely inferential grayness." For the complex figures *O* says: (7) "There is a dividing plane between the two figures which is glassier, denser, perhaps thinner in the sense of thickness, than the other sides. It is transparent. The sides are softer."

Contents. A (2) "I can't be sure that this is a solid figure. It seems solid all right, but if it were, I'd only be seeing it through itself." (5) "Like a block of very soft air, just so dense that I can see it." (6) "Glassy and congealed, as if the thing inside were 'caught in the going'."

Bi (1) "The figure is empty, but sometimes it's like a solid block of black." (5) "When it has a content, this content is really lighter than the background; really light gray, though I take it as black." (6) "Sometimes like a block of jelly, that doesn't shine like jelly; no highlights, and it has the effect of being transparent, though I don't see the background behind it."

H (3) "The figure is seen as solid. The inner surface or edge is very indefinite, but I don't really see anything inside." (5) "The sides are glassy, gray, and transparent, and they don't stop inside, so the content is like them, but I don't see it definitely as content." (7) "The gray I see belongs to the figure, it doesn't project beyond it."

These reports are in general like those of Series *a*. Bi, however, describes both an empty and a filled figure. The solid figure is black, and evidently not glassy. This report, since it was given with objects some of which were identical with those of Series *a*, probably indicates a change of attitude.

(3) *Experiments with Irregular Ridges*

We suspended two large sheets of gray cardboard 45 cm. apart in the carrier, in a vertical position, perpendicular to *O*'s line of regard. The upper edge of both, which had been cut into irregular ridges, was the only one visible to *O*. The ridges of the two sheets were asymmetrical; the front sheet hung 10 cm. below the back, which in turn hung 10 cm. from the top of the opening, and 45 cm. before a black velvet background. As *O* looked into the opening, he saw two sets of gray ridges, one higher than the other and at some distance behind it, and behind this, a black background.

Series a. This series was a repetition of (1), Series *a* and *h* combined, *i. e.*, simple presentation of the figures and presentation with different lightings. *O*s were A, Bi and H.

A always reports as in (3) "I simply see extended nothingness. I don't see it filled in at all."

Bi (1) "Between the ridges there is a haze. It's foggy and penetrable, a somewhat transparent film. It shades off and up, so that it's thinner at the top." The other reports are the same, except that the haze is lighter or darker with different intensities of light.

H (1) "The space is filled with gray, which runs in a band (but this band isn't a surface) between the irregularities of the gray mountains." This gray for H becomes lighter or darker with different intensities of light, but the report itself, except for this, is unchanged.

A change of light did not materially affect the filling in those cases where it was present. The haze of Bi and the gray of H simply become lighter or darker. A does not see a filling. The space is not described as glassy by any *O*.

General Summary

A, By and Bi always find indefinite sides, except for A (1) Series *f*, Bi (1) Series *c*, and all *O*s (2) Series *a*, *b*, *c* and (3) Series *a*. For H, who did not observe in Series *g* and *h*, the surfaces are always gray, and for Bs in the one series in which she observed, (1) Series *g*, they were gray. A sees plane surfaces in (1) Series *f*, and in (2) Series *a*, *b* and *c*. Bi sees definite surfaces in one series, (1) Series *c*, and occasionally also in (2) Series *a*, *b* and *c*. All of the *O*s see some of the figures as solid, though the content differs for different *O*s. A sees the content differently through the sides and through the small circle, and both A and H tend to become doubtful of the reality of a content. In (1) there are definite reports of glass both as surface and as content, and also cases where its presence may be inferred. In (2) and (3) no glass is reported.

(*C*) *Experiments with Mirrored Forms*

The suggestion for this group of experiments came from a report in which *O* described what she saw in the skeleton figure as "like the mirrored reflection of black velvet which shows the glaze of the mirror and the velvet too."[24] We used (1) skeleton cones, (2) irregular ridges, and (3) simple objects.

(1) *Experiments with Skeleton Cones*

Two m. in front of the carrier in which the figure hung, we placed a 45 × 50 cm. plate glass French mirror. The reflection passed from this

[24] See p. 262.

mirror to another similar mirror of the same dimensions, which stood at an angle of 45° to the first, and into which the *Os* (A, Bi and H) looked. We presented series of skeleton cones corresponding to (*B*) (1) Series *a*, *d*, *f*, *h* (simple presentation, introduction of a fixation point, introduction of flecks, and different lighting).

Series a. Surfaces. A (1) "Both the little circle and the sides are misty, and the mist is glassy." (2) "Not much difference in the texture of the little circle and the sides, except that the sides are more like mist"; (5) "indefinite."

Bi (1) "Black; harder and solider, and not so soft and filmy as the unmirrored figures were; black but grayish." (3) "Though I say these surfaces are black, they are always slightly grayish, and they're never soft and fluffy as the unmirrored ones were."

H (3) "The sides are a transparent neutral gray; slightly fluffy." (5) "The near end is also a transparent gray; glassy-like, more solid, more bulky than the sides."

Contents. A (2) "Through the small circle, the contents are liquid looking, and through the sides they are misty." (3) "Looks liquid through the small circle; closer to a bulk than to a film; more like jelly than glass."

Bi (1) "Filled with black." (2) "The black filling is fluffy, but sometimes it changes, and the figure seems filled with black jelly."

H (1) "I take the thing to be more or less solid, but what I see is a completely bounded figure." (5) "I really don't see a content, though I take the figure as solid."

The reports of both A and H are like those in the stereoscopic and the real figure experiments. The boundaries for A are indefinite and misty, and for H are transparent gray. A does not find as much difference between the surface of the small circle and the sides as she did in the other experiments, but she notes the same difference in content through the circle and through the sides. H takes the figure as solid, though he sees only boundaries. The grayish black surfaces which Bi sees differ with respect to hardness from the unmirrored figures. The content for him is either a fluffy black, or a black jelly. A describes the mist as glassy (S 1) when it forms the indefinite boundary, and H (S 5) finds the smaller circle more glassy than the sides.

Series b. Surfaces. A (1) "The side is vague and misty. It is transparent, without any color. The little circle is definitely transparent like glass; definitely glassy, like isinglass." (3) "The sides are misty, but a bit glassy. The small circle is like isinglass, or like mist frozen into glass."

Bi (1) "The side surface is grayish and harder than in the unmirrored figures. It seems more solid and not so soft." (2) "When the fixation is at the back, it pulls the black out of the figure and leaves a gray surface shell."

H (2) "The sides are gray; not sharp surfaces, but the gray forms a sort of plane between the circles." (3) "The near end is gray, too, and is more glassy-like than the sides."

Content. A (1) "Liquid-looking through the small circle and bulky and misty through the sides."

Bi (2) "When the fixation is at the back, it pulls the black out of the figure." (3) "Black and fluffy, but still hard; sometimes like a black jelly."

H (3) "The figure looks solid, yet I can't say that I see any content. What I see is boundaries."

The fixation point does not affect the figure for A or for H. The smaller circle and the vague side are different for A both as to surface and as to the content seen through them. She reports a surface glass, which is apparently more definite in the smaller circle (S 1). The surfaces for H are gray; the familiar difficulty is present as to content; and the near end is said to be

"more glassy-like than the sides" (S 3). As in Series *a*, Bi finds the gray surface harder than that of the unmirrored figures, The fixation point, when it is not in front of the figure, pulls the black filling back with it.

Series c. Surfaces. A (1) "The sides are not definite, but where the flecks are there does seem to be a more definite boundary."

Bi (2) "Sometimes it seems as if there were a conical shell uniting the two circles, but I don't see it except as a lightish graylike stuff, which isn't really a surface."

H (1) "The side is a very thin veil of gray. It is almost transparent. The front plane is gray, and it is more transparent than the sides."

Contents. A (1) "Through the small circle more liquid than through the sides. Through the sides it is film-like." (2) "Some of the flecks are in this film-content. I see it as black content (through the sides) but I can't say whether it's black itself, or perfectly transparent, with the black behind it."

Bi (1) "A transparent jelly-like stuff; icy, airy; not black ice; but it is not quite clear. If I miss the flecks it tends to fill up with cloudiness." (2) 'The black murky stuff does not come nearer than the back fleck. The figure seems carved out of glass."

H (2) "The flecks emphasize the emptiness; no hint of content, even in meaningful terms."

The reports are very much like those of (*B*) (1) Series *f*. The flecks help A to form bounding surfaces, but do not affect the surfaces for Bi, who finds them indefinite, "not really surfaces," or for H, who sees the gray planes. A sees a liquid content through the small circle, and a film-content through the sides. She cannot say whether it is colorless or black. The flecks for Bi keep back the cloudy black, and leave the cone filled with a transparent jelly. H again notes that the flecks empty the cone.

Series d. Surfaces. A (1) "The smaller circle is slick like glass, but the sides are more film-like." (4) "The front circle looks darker than the sides. All the boundaries are very indefinite though."

Bi (2) "Something like a gray shell there, but it's not a surface." (3) "The filling has no definite surface limit. The little circle seems darker than the rest of the figure."

H (1) "Both the side and the front planes are a neutral gray." (3) "The side is soft and fuzzy, and the front plane is more transparent, but they are both fairly definite."

Contents. A (2) "Through the small circle, positively transparent and glass-like with the shine and the slickness of glass; through the sides, lighter and filmy."

Bi (1) "A blue black, rather glassy, cloudy, murkiness; foggy." (3) "Sometimes the figure seems set in a glassy, rigid stuff."

H (2) "I do not see anything that I could call a content. I believe that the figure is hollow."

For A the indefinite surfaces and the content are different in the front circle and in the sides. The small circle is glassy and the content through it is glassy; the sides are filmy, and the content through them is filmy. There is a very positive report of glass, A (C 2). As opposed to the hard surface of Series *a*, Bi sees a gray shell which is not a real surface. The content for this *O*, while still black, is glassy, and his report under (C 3) is new.

General Summary

The observations from experiment to experiment are very much alike. A finds indefinite surfaces and a content which is different through the sides and through the smaller circle. For H the sides are always transparent

and gray; and although he takes the figure as solid, except where flecks empty it (Series *c*), he is not sure that he sees a content. Although, in Series *a* and *b*, Bi says that the sides are harder than those of unmirrored figures, he decides in Series *c* and *d* that they are "indefinite, not really surfaces." The content is either a fluffy black or a black jelly, depending upon the position of fixation points or flecks (Series *c* and *d*). A and H both indicate surface glass. A reports a content glass, which is also present for H by implication (*i. e.*, like the sides), if there is a content.

(2) *Experiments with Irregular Ridges*

We suspended the irregular cardboard ridges of (*B*) (3) in the carrier and mirrored them as in (1) Skeleton Figures. We changed the lighting during the series, following (*B*) (3).

A (1) "There must be something between that holds these things apart, but I don't see it."

Bi (1) "The filling between them has no definite surface limits. It's a yellowish haze that's hard and glittery; transparent, but almost opaque." The only difference which *O* reports with change of light is one of tint. As the light was moved farther forward, the haze became grayer.

H (2) "The space between, if I fixate it, becomes a solid roof; light gray and soft. It means solid. It is not transparent. If I fixate the top of the figure, the space between is seen as slightly blurred, hazy, and grayish, like the stuff one gets on a hazy day."

A does not see a filling, but Bi and H do. This filling is for both *O*s a haze, which for H is gray and for Bi yellow. It is not transparent for H; for Bi, though it is transparent, it is "almost opaque." It is soft for H and hard for Bi, who adds further that it is glittery. For both *O*s tint alone is a function of illumination.

(3) *Simple Objects*

We placed 3 simple objects (a 7 × 12 × 17 cm. box, a 3 × 10 × 35 cm. and a 3 × 12 × 60 cm. smooth board) on a table before one of the mirrors used in (*C*) (1), in such a way that in the reflection a portion of the table surface appeared between the objects. This visible surface appeared first equal in extent between the objects, then greater between the first and the second objects, then greater between the second and the third objects. The *O*s (Bi, By and Bs) who sat 3 m. from the mirror, could see the objects but not themselves in the mirror. We obtained a low degree of illumination for the objects and the mirror by passing the light from a 200 watt Mazda bulb through a heavy tissue paper screen. The bulb and the screen stood to the right of the objects, and were cut off from the *O* by a black velvet curtain. The instruction read "You are to describe what you see, noting particularly the space between the objects."

Bi (1) "I'm looking for that jelly-like stuff but I don't see it. There is nothing but the table between the objects. The third dimension comes in somehow, but it doesn't seem to be anything visible. It's more like knowledge about space than anything visible."

By (3) "The only thing I see between the objects is the brown quality of the table. There is a suggestion of a white colorless filling, too, but I'm not sure that I really see it."

Bs (3) "That space is filled all right, but it's absolutely clear and transparent. It seems to be a thick air, like that you see on a heavy rainy day, just before it begins to rain. It reminds me of the dust particles in a sunbeam, though here the particles are quite close together. It looks glassy, too, like looking into a paper-weight."

Bs is the only *O* who reports a positive filling. She notes the presence of fine particles and says that the filling is glassy. Both Bi and By see only the table between the objects, although By finds a suggestion of a white colorless filling, which he is not sure that he really sees. Bi prefers to ascribe his realization of the third dimension to what he knows rather than to what he sees.

(*D*) *Miscellaneous Experiments*

(1) *Experiments with Wall-Papers*

Both Brewster[25] and Helmholtz[26] have observed that by squinting the pattern of a flowered or figured wall paper may be brought close to *O*. We performed our experiments of this type in the darkroom, where we suspended 1.5 m. before the *O*s (Bi, By and N)[27] 2-meter squares of cretonne, linoleum, and wall paper, in which a pronounced pattern (about 25 cm. square) was repeated at regular intervals. For illumination we used a 200 watt Mazda lamp, the light of which was filtered through a Gage 'daylight' glass, faced with a double screen of tissue paper. The instruction read: "By squinting, you are to lift the pattern from the background. Then you are to describe what you see, noting particularly the presence of any glass in the experience." After all the experimenting that we had done,—(*D*) followed (*A*), (*B*), and (*C*),—we felt that we should not influence the result by asking directly for glass. Since *O* found it difficult, at first, to converge in front of the pattern, we gave him a steel needle upon which he fixated, as he held it out before him, and which he discarded as soon as he could proceed without it.

Bi (1) "All the patterns are set in a glass which covers the whole field. I don't believe that I'd say that the patterns themselves are glassy." (3) "I get glassiness all right. The patterns are set in it, but it's so out of focus that I can't tell about the distance between figure and background. It's not as flat as paper normally is though." (4) "There is the impression that these things are set in glass, though it's out of focus and I see it poorly." (7) "This thing is filagreed and set in something similar to glassiness. It's bulky like a glass, but it has little particles dancing in it."

By (2) "I think I do see something there, but it's hard to say, because the background looks filmy. You seem to see something like what you see in the stereoscope, but it doesn't have quite as much body as the stereoscope stuff has. I think it's there and surrounds the whole flower in a thin film." (3) "I certainly see a lot of glassy stuff everywhere. The pattern is off the background, and the glassy stuff is between it and the background, and over the pattern and between me and the paper. It seems filled with it." (5) "The glassy stuff is there in a big cloud, only cloud isn't a good word. It is denser and has more body than a cloud has. It is very smooth, and it glitters. It looks something like jello, that is, it's not quite colorless, but has a tinge of the background color in it."

N (1) "Between me and the pattern there is a suggestion of what you see in the summer heat shimmers. Sometimes this glassiness is a thick cloud and sometimes it is a thin sheet before the paper." (4) "The glassiness is very solid and apparent, but the pattern does not stand out clearly in it. It is bleary." (5) "This stuff is in the nature of a thickening of some kind. It can be more or less glassy, and more or less thick, and it is very transparent. Glass is the best word I can think of."

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[25]D. Brewster, On the Knowledge of Distance given by Binocular Vision, *Phil. Mag.*, 3rd. Series, 30, 1847, 305f.

[26]H. v. Helmholtz, *op. cit.*, 798 f.

[27]Dr. J. P. Nafe (N) was a member of the department of psychology.

Bi and By are more successful than N in raising the pattern from the background, but all report the presence of glass which covers the whole field, and in which, for the first two at least, the pattern is set. Bi mentions dancing particles in it; By says that it glitters and has more body than a cloud, and N suggests the heat shimmer, either in a thick cloud or in a thin sheet. We may thus infer that By and N also saw something resembling a flecky medium.

(2) *Experiments with Funnels, Cylinder, and Pierced Card-board*

We next repeated some experiments described by Rubin.[28] Because our time was limited, we divided the experiments: By observed the funnels and the cylinder, and Bi and N the opening in the card-board. In order to discover whether or not there was a limit to the extent of the air-figure, we introduced a slight variation by using for By a series of 7 funnels, the openings of which ranged in diameter from 3 to 15 cm. by 2 cm. intervals. The instruction read "You are to describe what you see (*a*) while attending to and converging upon the distant object; (*b*) while attending to the irregular opening and converging upon the distant object; and (*c*) while attending to and converging upon the irregular opening. Note particularly the presence of any glass in the experience."[29] After the report on this instruction we asked By to repeat his observations with the same instruction, to which we now added "Move the funnel about over the field." We next requested *O* to fixate a point inside the funnel, and for some observations inserted a pin in the body of the funnel as a fixation point.

The instruction for Bi and N read "Fixate the edge of the hole in the card. Describe what you see *in the plane of the opening*, noting particularly the presence of any glass in the experience." We also asked the *O*s to move the card about over the field.

By. Under (*a*) of the instruction, *O* always reports: (2) "I don't think there's any glass there. It's hard to say, because it's hard to follow this instruction, but I think that I am following (*b*) when I get any glass." Under (*b*); (1) "There is a thin film of glass over the end of the tube. It is like a little window, and that film is very thin. The object at which I am looking is a thing by itself." (2) "The film moves with the funnel, as if it belonged to it." Under (*c*); (2) "The space beyond the tube is filled with a whole bulk of that glassy stuff. The object beyond it becomes indistinct and like a film." (5) "When I move it the glass bulk doesn't move." The reports were similar for all the funnels except the largest (15 cm.). Here the glass disappeared, both as film and as bulk. With the introduction of fixation inside the funnel; (1) "The hole comes up to where I fixate. The tube is foreshortened. The glass is a bright, brilliant sheet across this opening, but it doesn't come into the tube. The tube is just foreshortened." (2) "When I use the pin and fixate on it, it appears to lie in the hole. The glass is very much present. The pin lies in it. I think it is a bulk, not a sheet. If I move the pin it moves around in the glass. I don't think it comes up into the tube."

The reports for the cylinder were like those for the funnel.

Bi (1) "There is a filling that does not come this side of the hole. It's like the clouding that you get in water by stirring up a light sediment that's gone to the bottom. But this doesn't have a front surface." (4) "This is excellent glass. The rough places in the background look as if they were covered up with a great mass of glass, which fills everything solidly from the hole backwards. The glass is so real that the cardboard seems to lean

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[28]E. Rubin, *op. cit.*, 61-66.

[29]That this instruction proved too difficult for him to follow is evident in *O*'s report.

up against it. It's perfectly clear, but it has streaks and splotches in it that don't seem to belong to the background. It doesn't seem to have a front surface."

N (1) "I see a solid glass film lying just behind the opening. It's of indefinite depth, but there's more of it than a plane; like looking into a block of glass, except that there are no imperfections in it. The lines of the background seem to lie behind it and they are blurred."

Later, both *O*s said that they saw the thin film in the plane of the card.

By finds not alone the thin glass film of Rubin, but a bulky mass beyond the funnel, or no glass at all, depending on instruction. Neither the film nor the bulk appeared for him with the funnel in which the irregular opening was 15 cm. in diameter. Bi and N practically duplicated with the cardboard the results of By with the funnel. They found a bulky glass beyond the hole and a glass film across it.

(3) *Experiments of Jaensch with Colored Liquids*

We also repeated the experiments described by Jaensch with flasks and threads.[30] We used a red and a yellow liquid. *O*s were Bi, By and N.

Bi (1) "The glass is there in the figure. I get a murkiness that is yellow and that is associated with the glass like a dye or stain. I think the glass is colored as a paper weight is colored. It's a solid yellow prism all right." (3) "Now the prism is perfectly clear, and the color is back. That clearness is glassiness though, and it looks solid and hard, without quality in the ordinary sense. I mean it's neither gray nor color, film nor surface, but it has a transparent glittery character." (4) "I get planes all right, but I can't see any surfaces."

By (1) "A yellow prism. I can't tell much about it because it is set in a mass of stuff just like itself. This is glassy, thick and very dense." (3) "A glass prism. It's difficult to say what the sides are made of. It seems to be set in a block of glassy stuff which is colorless, shiny, lustrous; and because it is made of this stuff too, I can't say much about it. The yellow lies behind the figure somewhere; at least the figure itself is colorless."

N (1) "A yellowish-brown prism. It is transparent or at least translucent. I really don't see any planes, I just read them in, I think." (3) "I take it as a solid figure; transparent and made of glass. I think I see planes, but it is not so easy to be sure of them, because, since it is transparent, I don't localize anything inside." (5) "It seems to be a luminous figure, whether I see it as colored or not."

Our results agree with those of Jaensch. Whether the figure is colored or not, the *O*s describe it as glassy. While they take it as a solid figure, they cannot say much about either surfaces or content, except that what is there is glassy.

(4) *Experiments with Field Glasses*

We asked H and By to observe bits of landscape through a pair of field glasses. The instruction read "You are to describe what you see, noting particularly the space between things. Please note also the presence of any glass in the experience."

By (1) "If I don't look at any particular spot, the whole field seems set in a block of glassy stuff. If I look at some special spot, as between the twigs, then I see a thin sheet of something like clear glass between them." (5) "I am sure now. In a small space, like that between two branches or twigs, there is a thin lustrous glass sheet, but when I take the whole field, then there is a bulk of glass all over it."

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[30]E. Jaensch, *op. cit.*, 266ff.

H (1) "If I look at the space between the limbs, and if the limbs are fairly close together, there is a blurred effect that looks like a slightly lighter, grayish, very dense, highly translucent mass, that nevertheless is a little soft. If I look away at the side of the hill below the house, there is still something like a blurring effect, but it is a little lighter and more transparent. It seems to be more of a general mass without any boundaries at all. I don't see any particular space that way, but just a total space out there." (2) "Where the limbs are close together, I get a massive blurred effect. This is apparently something seen; something light gray, slightly lighter than the building which I see between the branches. It is localized in an indefinite mass all around the tree. If I focus for the tree in a place where the branches aren't so thick, the spaces seem to break up into individual areas. Then I get something like a surface-filling filling up that particular space. This surface seems to start from a slight chromatic effect, but I can't say that I really see anything actually between the things except these slight starts from each side." (3) "I get images of the blood capillaries in the eye. If I look closely, there is an effect of waving, quivering, and it gets localized at the plane between the bounding branches. It seems to generate a plane."

The *Os* agree that there is a filling which is a kind of bulk and which fills rather large areas, and a surface-filling which appears between the boundaries of small areas. H says that both types of filling are gray and blurred, while By describes them as "glassy."

(5) *Comparison of All the 'Glasses'*

At the conclusion of our experiments upon the "glassy sensation," we gave the *Os* (Bi and By) a figure from each group—(*a*) a stereogram, (*b*) a wire figure, (*c*) a colored liquid in which 3 threads hung, and (*d*) a square of wall-paper—and asked them to compare the glasses which they found in them.

Bi "They're all alike in their bulky tridimensional character. Sometimes the glass is perfectly clear and sometimes it isn't. It all glitters, though some doesn't glitter quite so much as others. It blurs too, more or less, and some is harder than others. It gets smoky or cloudy, too, but it's all the same kind of stuff."

By "The glassy stuff is the same kind of stuff for all of them. It may be more streaked in some cases, or it may be of different thicknesses, but it's the same kind of material in general. I think they're all different steps in the same series."

The *Os* agree that the glass in all the experiments is fundamentally the same kind of thing. The difference is not one of nature but one of degree.

(*E*) *Casual Instances*[31]

(M) "This morning while N and I were talking during an interval in his experiment, I suddenly noticed that the space between the two of us was filled with glass. N seemed to be set in a heavy transparent stuff that was perfectly clear, yet somehow visible. It most resembled the heavy glass of a thick paper-weight, and it seemed oppressive, as if I were trying to breathe in spite of a crushing weight. This was at 11 a.m. The sun was shining brightly, but it had not as yet come into the window of that room."

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[31]T is Professor E. B. Titchener; Hu and K were students in psychology, but not trained *Os*; Bm and L were students taking their first course in psychology; Ma was an untrained *O*, interested in psychology; C and Hw were totally untrained *Os*, who knew nothing about scientific psychology. Some casual instances are also furnished by the writer, M.

(Bi) "I was looking at that tripod downstairs. It reminded me of your wire triangles, and I looked to see if there were any glass there. Then I saw it just the way I saw those figures, with the planes filled in with glass. It was a cloudy day."

(N) "This morning, just as the sun came around to my side of the building, but before it was shining directly into my window, I saw a lot of glass. The smoke from my cigarette curled up very slowly, in fact it hardly seemed to move at all. I saw it in a block of glass that extended out all around it. It looked hard. It was something like the thing you see in a sunbeam when the dust is dancing in it."

(M) "As I was coming across the campus the other night, I glanced up at the treetops and saw the branches held rigidly apart by some solid medium, which was perfectly transparent, and yet plainly there before the gray of the sky. It seemed to hold the branches stiffly, as if they were frozen in it. The night was a clear and rather light one in early fall, before the leaves had fallen. The glass first appeared when I was about 40 ft. away and stayed as I approached the trees."

(M) "I was looking down upon Baker Tower through an open window in Morrill Hall, about 4 p. m. on a rather cloudy day. I saw the Tower and all the trees around it set in a mass of glass. The glass lay heavily, it seemed, upon the roof. It was most dense and thick and had that solid transparency about it that a thick window-pane has. I saw it both binocularly and monocularly. It was at least 200 yds. from the window to the Tower. I couldn't say where the glass began, at all, but it was just there, everywhere I looked, about the way water looks when you open your eyes in it."

(Hu) "At 10 a. m. when I was coming toward the Library, the snow was falling very slowly. I saw the library and the snow as if they were all set in a glass paper-weight. I almost expected to walk into a wall of glass, only the wall was all around me, too. I was about 100 yds. away, I think, when I first noticed it."

(M) "Some students asked me to tell them something about the glassy sensation. As I started to talk, I looked at the door and saw it filled with a shin sheet of glassy stuff. As I mentioned the bulky character which it tometimes has, I suddenly saw that the glass in the door was no longer a thin sheet, but was now merely part of a bulk that extended into the next room as far as I could see. This was about 3 p.m. on a cloudy afternoon in March."

(M) "I was observing for S in the dark-room. Everywhere between my eyes and the card which he was exposing, I saw a fine haze. It was transparent, yet filled with something like tiny bits of dust, and I want to say it was gray, though I don't think there was really any quality there. There was a rigid frozen stiffness about it. It interfered with my observation, and persisted during several exposures."

(By) "Seated in an automobile, I saw the space between the body and the top filled with glass. A cigarette-end held outside the car looked like the reflection of a cigarette-end in a mica mirror. There was an electric street-lamp directly above the car. The glass was slightly yellowish; it seemed thick and dense. It looked less like glass than like isinglass."

(M) "We were driving at night along a road bordered on one side by [illegible] the other by a rather thick pine growth. There was no wind and the heavy snow [illegible] directional trend. The whole landscape seemed unreal, as if the trees were made of stiff paper encased in glass. The snow fell in it, too, and the glass was still there, though one wouldn't have expected it to persist. The glass was there, whether one looked monocularly or binocularly, through the glass of the windshield, or out of the open side of the car."

(By) "I was sitting about 10 ft. away from the window, about 4:45 p.m. on a cloudy December afternoon. The electric light was on. The window, which is set in a kind of recess, seemed behind a box of bulky glass stuff. The box was formed by the recess. I couldn't see the glassy stuff if I looked at the edge of the box, but if I let the boundaries appear in indirect vision, the whole thing was filled."

(K) "Looking out of the window of the Laboratory in January, between the branches of a tree and the ground I saw a mass of clear, transparent, glassy substance, smooth, hard on the surface, the substance softer, more jelly-like. The mass had no definite boundaries."

(M) "I stood on the porch at night, looking over the city to South Hill. It was clear, but there was no moon. The whole valley seemed covered with glass, as some of the rock formations of Luray Caverns are immersed in water. The street lights twinkled, but I saw them through a transparent bulk. I shut one eye and looked and the impression persisted."

(C) C and the writer were canoeing on an afternoon in July. The sun was shining brightly. There was no wind. C suddenly said "Look at those trees across there (about 50 yds. away); they look as if they were painted on stiff air. Can't you see something between them that is hard and still? I suppose I am imagining it, but it looks that way."

(By) "At the end of the lecture desk in Goldwin Smith Hall there is a faucet attached to a right-angled pipe, which rises about a foot above the surface of the desk. I saw the space which was bounded on two sides by the pipe and on the third side by the desk as a square of surfacy colorless glass. It was highly reflecting and shiny. This was at 11 a.m. on a dull day in January."

(Ma) "I saw that glass this morning for the first time. I was walking up toward the Library, and I saw the building itself and the snow was falling around it as if they were in an aquarium. The stationary objects stood in something like water and the flakes fell the way food particles float to the bottom in fish-bowls. The thing that was there wasn't water, really, but that is the best way I can describe it."

(Bm) "I have been wondering what that 'glass' was that you talked about. Now I see it. It's between every snow-flake as if it fastened them together. It doesn't move, but it's always there between the flakes, and it doesn't stop them from falling."

(L) "I had heard of the glassy substance in lecture, but I doubted its existence. While sitting in a class and not even thinking of psychology, I looked toward Baker Laboratory and I saw that the atmosphere seemed to be entirely covered with this glassy substance. It was such a surprise to me that I showed it to the student at my side. It was as vivid as can be imagined and it immediately aroused my attention. I turned away, then looked again to make sure that I was not mistaken. It remained there before me. This was about 8:30 a.m. on an ordinary winter morning."

(By) "As I entered my room the other night the electric heater was on, but the light was not. Between the wires across the front of the heater I saw a surface of glass. The space behind the glass seemed filled with a red film. I could not say whether the glass itself was red or whether I was looking through a colorless glass at a red quality beyond it."

(Hw) Hw and the writer were walking along the road on a bright moonlight night in August. Hw said "I almost believe that I can see air. Doesn't it look as if there were a dancing, soft stuff everywhere? Of course I don't really see it, but I think I do. I can't quite describe it." The writer (M) noticed at the time that there was a bulky soft glass that seemed to be all around her.

(M) "While looking absently at a small table, it occurred to me to try to surround it with glass. I looked and saw that the table was set in a bulky glassiness, which had no very definite boundaries, but which was most in-

sistent between the table-legs. It was somehow more than a surface. Then I tried to destroy the glass, but the impression persisted. This was in a small room, under artificial illumination."

(T) "I was looking across the road at a slope of smooth snow running up the hill between fairly even lines of trees. The snow had become gray, and I absent-mindedly took it for an expanse of gray sky seen between the trees. Suddenly I noticed that the gray sky was glassy all through; and only then did it occur to me that the sky itself was a 'memory color' and that I was really looking at the solid slope of snow."

Both By and M find that, although they may call up the glass practically at will, once it has appeared they cannot destroy it.

Conclusions

The conditions which Schumann finds for the arousal of the 'glassy sensation' are (1) plasticity, which is aided by eye-movement; and (2) the localization of small points in the field, which may be a further aid in bringing about eye-movement with roving attention.[32] Although the presence of contours may attract attention, there are also cases in which the medium is present, but has no limiting surfaces. A secondary condition is the degree of compactness of the filling; which means merely that, where the medium is less compact, it is more difficult to observe.

These were, no doubt, the conditions to which Schumann was led by his observations. Our own far larger material, however, proves them to be inadequate. Thus (1) plasticity, in the sense of perceived tridimensionality, may occur in the absence of the 'glassy sensation'. If, however, the 'glassy sensation' appears, it is itself constitutive of plasticity. It does not seem, then, that plasticity can be set down as a condition of the 'glassy sensation'. Nor can (2) eye-movement be accepted without reservation as a condition of the phenomenon; for the elimination of eye-movement, as in certain of our experiments with the stereoscopic slides, does not sensibly change the reports upon the glass. It is, however, true that in some instances the point at which fixation occurs determines both the presence and the nature of the glass filling. We cannot grant either (3) that the presence of perceived flecks is a condition for the observation of glass, although our results indicate that the nature of the filling is influenced by their presence. As for (4) roving attention,—where, as in the Rubin experiments, we required a fixed direction of attention, we still obtained reports of glass, the nature of which again differed with different points of fixation. With regard (5) to compactness, our reports show that there are indeed different degrees of compactness of the filling, and that the more compact the filling, the more definite is the report of it. But this condition seems akin to that of plasticity, in that it presupposes the existence of the phenomenon under investigation.

[32]F. Schumann, *op. cit.*, 238ff.

Schumann makes no reference to object-consciousness. Indeed, his statement that, although contours attract attention, there are cases in which the glass has no limiting surfaces might be interpreted to mean that object-consciousness is not a necessary condition of the phenomenon. In our own experiments, however, the glass, whatever its extent over the visual field, was always localized. This rule, to which there is not a single exception, indicates that we are dealing with a phenomenon at the level of perception,—a conclusion which is borne out by the rôle which certain of our experiments ascribe to fixation. For the rest, we can ourselves give no conditions for the glassy phenomenon except the conditions for spatial perception at large. Wherever there can be spatial perception, wherever there can be visual object-consciousness by way of localization, there may also be glass.

The casual instances alone prove that it may appear outdoors or indoors, in summer or in winter, during the day or at night, on bright or on dull days, surrounding large or small objects, large or small in extent, near *O* or far away, monocularly or binocularly, to the naked eye or through lenses, under artificial or natural illumination, immediately or after an interval, with or without expectation, to trained or untrained *O*s. We have been wholly baffled in the attempt to put our finger upon some precise moment or moments that should control the appearance of the phenomenon.

We remarked at the beginning of this paper that we believe the 'glassy sensation' to be essentially imaginal in nature. During the experiments we were constantly meeting the fact that any change in conditions which brought about a change in the reports did not effect the same change for all the *O*s. This result leads us to believe that the phenomenon is largely of central, not of peripheral origin.

We are not, therefore, prepared with Schumann to admit a 'new sensation'. The very fact that the term 'sensation' is classificatory only, demands that the basis of classification be adhered to. We thus expect that the proposed 'sensation' shall arise as the result of adequate external stimulation, and that it shall have the attributes at least of quality and intensity.[33] But Schumann admits that, if such criteria are insisted upon, the 'glassy sensation' does not exist; for it lacks the attribute of intensity, and it has no adequate external stimulus. That it has the attribute of extensity (since it exists both as surface and as bulk), and that it may have different degrees of saturation are, along with quality, the grounds upon which he bases his

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[33]Cf. G. E. Müller, *Abriss der Psychologie*, 1924, 76. "This (Schumann's) view, which would have to start out from the assumption of a spatiality of the original visual sensations, would become very important if it could be shown that to the impression of greater depth there always corresponds a greater thickness of that spatial sensation." There is no hint, in our observations, of any such thickening with distance.

conclusion that he has found a new sensation.[34] It seems useless on such grounds as these to attempt controversy. To accept the glassy impression as a sensation is, at any rate, to erect a new definition of that protean term.

We conclude, accordingly, that the 'glassy impression' is not a sensation, but a perceptive content which is largely imaginal. It may be that this content is to be regarded with Jaensch as identical with or based upon G. E. Müller's "central gray." It may be that it is based upon a projection of the humor of the eye, such as may be seen in the observation of *muscae volitantes*.[35] Or both of these factors may be concerned,—or other factors, which as yet escape identification. We hope that future experiments may decide this still outstanding question.

[34]F. Schumann, *op. cit.*, 235 ff.

[35]This explanation was also suggested by Professor K. M. Dallenbach. Our observer H suggested that the projection of the retinal capillaries might afford the necessary basis for the phenomenon. Bühler's view seems to be negatived by the results of D. Katz' experiments (Neue Beiträge zu den Erscheinungsweisen der Farben; Luftlicht und Beleuchtungseindruck, *Zts. f. Psych.*, 95, 1924, 129-136). We have repeated and confirmed Katz' observations.

GENERAL ANTHROPONOMY AND ITS SYSTEMATIC PROBLEMS

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Prospect

In the present paper we shall seek to justify the use of a new name for the scientific study of human nature. The necessity for such a change arises partly from the serious inappropriateness of the term 'psychology' as a designation for the science and partly from the discredit cast upon the field by charlatans and spirit-seekers. The discussion will also lead us to attempt to dispel the notion that a non-psychic study of human nature is mere biology.

Argument

Anthroponomy is the science of the laws which govern human action—the science of human nature. It seeks by experiment and systematic observation to arrive at an understanding of the factors which determine certain of the observable phenomena of the human individual. This science of anthroponomy is a newcomer, having arisen gradually in opposition to its nearby relative, psychology. The essential difference between psychology and anthroponomy lies in the attitude towards the observations which are made. The former science is derived directly from the Greek philosophers and is concerned only with the study of mind, consciousness, or the psyche. This object of study is held to be directly accessible only to the subject who possesses it, and it is also regarded as a separate and distinct aspect of the universe. Such a point of view results, as might be expected, in the neglect of all data concerning the human individual which cannot be interpreted as evidence for the existence of some type of psychic process. In other words, scientific observations are not to be valued for their own sake, but solely because of the inferences concerning consciousness which they make possible. How different is the case of the anthroponomist! He never raises the question whether or not the observable phenomena express or embody a psychic world beyond them. He studies the large field of observable human nature in order to describe and explain the phenomena there found. When the question of the control of human nature arises, the anthroponomist proceeds in terms of the laws derived from his matter-of-fact study. Scientific psychologists are one with the anthroponomist here, inasmuch as matters of control are practical affairs and do not rest upon philosophical theory. However, a method equally valid theoretically is fol-

lowed by the host of unscientific psychologists, the method of psychic control which seeks to shape human events through spiritual influences. Although this effort and this school of psychologists are pooh-poohed by scientific men, the amount of unscientific work does not seem to abate. In attempting to avoid the problem of spirit-control and psychic causality, scientific students who still insist that they are investigating "consciousness" turn to the nervous system as an explanatory rubric. This general problem of explanation, as it arises in anthroponomy, will be discussed later (p. 300). At the present time, however, it should be pointed out that recourse to neural processes in psychological study leads the psychologist outside the psychic and involves him inevitably in philosophical theories of psychophysical relationships. Not only does this difficulty occur, but another equally serious appears in the uncertainty over the boundaries of his science. The human individual is so thoroughly an organism that it becomes a practical impossibility to admit one segment of the organism (the neural basis of consciousness) into a science and to exclude all other phases of that segment and all other segments. The neural processes accompanying the psyche are so organically related to other biological (neural and non-neural) processes that once the psychologist opens the door to the former he cannot perforce close it to the latter. The psychologist is thus led to expend much energy in attempts at the proper delimitation of his field. The psyche he cannot abandon and remain psychologist. If he includes in his science certain neural processes and stimulus-response situations upon the pretext of elucidating the psyche, he disrupts the organic character of the individual studied. If he reaches out still further and includes all of the data on behavior in which he may be interested he must defend himself from the implication that he has become a biologist.

It has been said above that the essential difference between psychology and anthroponomy lies in the attitude adopted toward the data gathered. One difference in this attitude is shown in the unwillingness of the psychologist to describe and explain observable data as such irrespective of their supposed relationships to a psychic world. The second difference in attitude arises from the first. Anthroponomy is essentially genetic and seeks to explain human nature from the simple to the more complex processes. Psychology on the other hand is essentially non-[illegible] from the vantage-point of the most complex process, the assumed psyche. Anthroponomy begins with the relatively simple stimulus-response situations in man and animals and attempts to understand the more complex forms of behavior upon this basis. It studies peoples of varying degrees of cultural attainment, not in an at-

tempt to read psychic powers into them, but in order to understand the observable phenomena of the primitive peoples themselves. Knowledge so gained, it is believed, should clarify much that is found in the more developed groups. Animals are studied in terms of their own behavior-history as subjects interesting in themselves, but with the added conviction that the understanding of human nature will be advanced when the problems concerning infra-human animals are solved. Anthroponomists do not attempt to discover a psyche or a mental life in animals, nor do they treat the animal's responses as instances of introspection. Psychology does all of the things which we have said that anthroponomy does not do. It endeavors continually to find traces of 'psychic processes' in lower animals and peoples. Its experimental data on laboratory humans are almost entirely in the form of language-responses understood as symbols of the 'psychic'. *These responses and the problems arising from the analysis of their symbolism are taken as the key to the understanding of human nature, in spite of the fact that language-responses and the psychic processes symbolized are most complex affairs which are probably late in appearance in racial history.* This anti-genetic method is well illustrated in the treatment of instinct, where psychology seeks for conscious correlates and analogies in place of devoting itself to the description and analysis of observable unlearned behavior and its language connections, even to the extent of asserting that instincts arise by a process of lapsed intelligence. The nearest that psychology has come to a scientific genetic account of phenomena is in its attempts to show the origin of adult states of consciousness through analysis by introspection. Such studies, conceived in the manner of Wundt, Stout, and Titchener, however, are more aptly termed analytic than genetic. The reason for the essentially non-genetic or anti-genetic character of psychology lies in the fact that by definition the subject-matter and goal of the discipline are kept within the confines of language-responses treated as symbolic of the mental world. The psychologist cannot by definition remain a psychologist and go outside of these responses. And yet he must go outside and relate them to other and simpler stimulus-response connections, if their explanation is to be found.

Anthroponomy is in the process of being born. Much good work has been done in it even by the psychologist, for he has taken the chief interest in a scientific approach to human nature in spite of the fact that he has usually been diverted from the main topic by the will-o'-the-wisp of consciousness. Indeed the relationship between anthroponomy and psychology is not unlike that between alchemy and chemistry and between astrology and astronomy. The alchemists were the chief group of scientists interested in the different kinds of matter and in their

interactions. They were, however, obsessed by one problem, that of the transmutation of substances. It is unwise to call this either an insoluble or an insignificant problem. It is, nevertheless, but one problem in a large field, and it was insoluble for the men who approached it in the days of alchemy. These early students lacked the objective non-magical point of view of the present chemist. They lacked the great body of chemical knowledge and the refinement of technique necessary to attack the problem with a hope of success. And yet, in the pursuit of their goal, they made many solid contributions to knowledge which were incorporated into the later science, renamed chemistry. The parallelism is not quite so close with astrology and astronomy, inasmuch as the astrologers were chiefly concerned with the horoscopic aspect of astral observations, a problem which astronomy has repudiated perhaps for all time. And yet the astrologers made many careful observations of great scientific value. The casting out of devils was the great need in alchemy and astrology and is now the great need in psychology. With the attainment of an objective point of view, the magical mind-body problem, which is characteristic of the three pseudo-sciences of alchemy, astrology and psychology disappears. This prerequisite to complete scientific adequacy comes slowly but surely. But once the point of view is secured, a proper perspective of the task in hand is possible and a common-sense programme can be formulated. Problems can then be attacked in the order of their probability of solution under the conviction that only upon a genetic basis can the most complex and baffling phenomena be analyzed.

The connotations of the terms alchemy and astrology were so embarrassing to the scientific men concerned and so inadequate to describe the nature of the problems investigated that the new terms chemistry and astronomy have come into general use. Alchemy persists only in historical and literary usage, but astrology is still practised by charlatans and self-styled psychologists of a non-scientific variety. Psychology, like alchemy and astrology, is a term which is quite inadequate to describe the investigations of the scientific men to which it is applied. Inasmuch, however, as there is now a large body of general scientific information and a close communion between scientific men, the situation might be left to correct itself through its own development. 'Psychology' would then be a term whose early social significance had lapsed and been forgotten just as has been the case with many other names. Future students would then be mildly interested in the fact that their forbears thought that a psyche could actually be observed and described, but their own scientific labors would be uninfluenced by the term. We are not permitted, however, to take so optimistic a view of the future

satisfactoriness of the term. In the first place, at least so long as the term psychological science exists, giving an apparently solid foundation of fact to the assumed existence of a psyche, philosophers will speculate upon the nature of consciousness, and by so doing they will give moral courage through the mechanism of group suggestion to those who seek to found and perpetuate such a study. In the second place, popular charlatans will always gain ready access to the popular imagination by appealing to the psychic, with the result that scientific work will continue to be embarrassed by the general misconception of the field. The situation here is of much the same order as that which confronted the early chemists and late alchemists. As the chemists could work in an atmosphere freer from scepticism and distrust when they had freed themselves from the incubus of the term alchemy, so it should be expected, even in this enlightened age, that the investigation of human nature should be subject to fewer misunderstandings when the term psychology has been dispensed with. In the third place, the non-psychic studies have become so numerous at the present time, and the label 'psychology' is contemporaneously so misrepresentative, that new names for the science are coming into use. Of these Objective Psychology and Behaviorism are the most prominent. The former, however, is defective in that it suggests a sub-division of psychology and in that it contains the obvious contradiction of a non-psychic psychology. Furthermore, it fails to obviate the popular misunderstanding of the field created through quackish and literary usages of long standing and of great vitality. Among Americans, Behaviorism has proved a wonderfully apt term. It contains no suggestion of the psychic, and therefore possesses the many consequent advantages above suggested. It does, however, refer to an "ism" and is not, therefore, well suited to designate an entire science. The term Science of Behavior is too cumbersome, and the Greek equivalent Tropology is too reminiscent of a special problem in the behavior of lower animals, to make these acceptable names for the science. Again, the term Behavior is too general for the field of human behavior. This term includes many biological processes in which the scientific students of human nature are not vitally interested. The term Anthropology, science of man, escapes these difficulties, but it is preempted by a closely related discipline. Anthroponomy, the science of the laws which govern human action, is therefore chosen to designate the subject of the science of human nature whose chief followers have been listed as psychologists. Such a change in scientific nomenclature as this is, to be sure, established neither in a day nor by arbitrary decree. It is the outcome of common usage, and only the future can tell what the final

designation of the science will be. At present only one thing is certain, and that is the inappropriateness of the term 'psychology'.

We shall be greatly aided in our understanding of the nature and scope of anthroponomy if we compare it with related disciplines and then examine briefly its subdivisions. We shall thus be led to see that anthroponomy is set off from other disciplines not by a principle (such as the limits of consciousness) but by a content historically and practically determined. First let us consider sociology and education, which are related to anthroponomy in much the same way that this science is related to biology. We must first insist that the chief differences between the fields are to be arrived at through an examination of the activities of the investigators belonging to each, rather than through an analysis of the terms employed. From the standpoint of philology, *e. g.*, biology may be made to include all of the studies of living organisms. But from the standpoint of what the biologists are actually doing, the picture is quite different and will be sketched below. Sociologists, to return to our immediate problem, are concerned with field-observations of human social behavior. The procedures employed are as scientific and as thorough as are those found in economics and history, but there is the barest minimum of experimental analysis. Rather, the empirical data are gathered by observational methods. So far as an explanation of social behavior is sought in terms of the fundamental characteristics of human nature, sociology depends upon anthroponomy. Sociologists, therefore, make much use of such factors as instinct, imitation, the self, and thought in attempting to explain social phenomena; but few if any sociologists are directly concerned with increasing the precision of these fundamental categories.

The discipline of education is a broad field of theory, history, art, science, and administration. It overlaps anthroponomy in the scientific study of human nature under school-room conditions and in the study of the rating problems which arise in training in the school-subjects. This latter field is well developed under the title of educational tests and measurements, and it is historically and actually closely related in method to the fields of individual and industrial anthroponomy. The former field is as yet less clearly defined. Having been dominated by normal adult anthroponomy, it has hardly begun to attack its problems upon an independent basis. It still depends upon borrowing data gathered under laboratory conditions in anthrop- [illegible] concerned with the impossibility of generalizing such data beyond the experimental conditions under which they were obtained. The treatment of the learning process is the most glaring [illegible]

data gathered by psychologists and anthroponomists is applicable to the school system. Methods of learning which are most efficient under controlled laboratory conditions may very well fail to show this high value under the hurly-burly of schoolroom teaching and administration. *Most of these shortcomings would be overcome by treating the science of education as a branch of personnel management.* In this case the educator could feel free to attempt the solution of his own special problems unencumbered by efforts to *apply* some other science. Finally educational theory, so far as it deals with the scientific and not with the philosophic, turns to anthroponomy and biology for its fundamental conceptions.

We are particularly interested in the relationship existing between anthroponomy and biology because many critics have said that to eliminate consciousness from psychology is to turn the study of human nature over to the biologist. Let us analyze the problem and see what conclusion is to be drawn. Biologists turn to physics and chemistry for certain of their more fundamental explanatory formulations, although most biological phenomena cannot as yet be advantageously handled in this fashion and must be referred to proximate sources. Anthroponomy draws many of its explanatory rubrics from biology and may, therefore, ultimately secure them from physics and chemistry. As yet, however, not only does it not go so far afield, but the great majority of its explanations are evolved from its own domain rather than from the processes studied by the biologist. In addition to this contact with biology, there is a great similarity in the general character of the phenomena studied in the two sciences. This similarity we shall proceed to develop.

Biology may be subdivided into a plant and an animal science. Animal biology concerns man and the animals below him. Human biology is most specifically organized in the medical sciences with their underlying pure sciences of human anatomy, embryology, histology and physiology. Outside of the medical field and the above-noted closely related human sciences, animal biology is primarily working with the animals below man where the human entanglements are genuine but secondary. The field of animal biology is concerned with structure and behavior. These two problems shade one into the other and still represent a difference in emphasis which is justified by practical interest and by experimental returns. Structure and behavior may be approached from the standpoint of the adult organism or from that of ontogenesis and phylogenesis. In the latter case (phylogenesis) the science of genetics appears, dealing unfortunately at present with the genetics of structures to the practical neglect of the genetics of behavior. Human biology takes little account of the problems of the ontogenesis of behavior except in so far as

and of physical measurement of the individual, while of immense importance in anthroponomy, are nevertheless problems to whose solution significant contributions have been made and will continue to be made from outside the field of that particular science.

If we were to seek a definition of human nature which would satisfy the needs of scientific study, we should derive it from the data of human behavior. Human nature is most characteristically shown in three classes of adjustments: those which have a well marked developmental history in the individual's life-time, those which involve or otherwise come under the control of symbolic stimulus-response relationships, and those which involve the inter-stimulation and response of human individuals. Anthroponomy, as the science of the laws of human behavior, is therefore the science specializing in such human behavior as we have listed above. And as a science of human nature it specializes in those aspects of environmental adjustments which significantly define human nature.

Certain fundamental systematic problems of anthroponomy will be outlined later in the present paper. It is necessary at this point to indicate the subdivisions of the science into its various fields, and thereby to gain an understanding of the various points of view from which human nature is envisaged.

General anthroponomy may be subdivided as follows.

(1) *Physical* anthroponomy is concerned with the measurement of various organic characters such as the cephalic index, body length, weight, etc., and with the relationship of such measures to the constitution of races on the one hand and to behavior-possibilities on the other. The established science of physical anthropology specializes in the above problem of racial differences, while the work on the behavior-significance of physical traits has been developed by psychologists. It is upon the possibility of growth in the latter problem that the chief expectation of value in the field of physical anthroponomy is based. Unless the various physical measurements are related to individual or racial differences in behavior, they have no value for anthroponomy. This relationship may be remote and may involve no direct influence upon behavior. It may serve merely as a means of classification of individuals who in their turn show on the average certain significant behavior-differences; and yet unless the differences are related to behavior in the end, they possess no appreciable value for the science.

(2) *Cultural* anthroponomy is concerned with the nature and development of cultural products in the various groups and races of men. It is interested in the origin of various types of tools, houses, arts, religious and economic customs, etc., and in the dissemination of such phenomena over the face of the earth. As a field of endeavor, cultural anthroponomy deals chiefly with inferior races and with the cultural remains of vanished races, [illegible] logical reason why its activities should not be extended to the present-day [illegible] Cultural anthroponomy not only throws a significant light upon the genetic [illegible] of present-day customs, but it enables one to construct a fairly accurate account of the modes of behavior of early man. It is, however, largely a descriptive field of endeavor, inasmuch as its genetic explanation [illegible]

nish a satisfactory account of the factors bringing about custom and change in custom. For this explanation recourse must be had to some of the following fields.

(3) *Social* anthroponomy deals with the inter-stimulation and response of human individuals. These relationships may be of varying degrees of complexity, organization, temporal and spatial extent. They may occur between two or more individuals of the same or different generations, and they may be relatively isolated instances of social behavior or cases of customary or institutional behavior. Inasmuch as it emphasizes contemporary human groups, social anthroponomy is not characteristically phylogenetic in its method. It deals in a descriptive and explanatory way with such well recognized phenomena as custom, tradition, mobs, crowds, audiences, leadership, the family, religion, fashion, and fads. It is concerned with language as a form of inter-stimulation and response and with the various uniformities in human behavior. It is closely related to the preceding field of cultural anthroponomy to the extent that materials from the lower and the vanished social groups are utilized. Social anthroponomy is as yet largely theoretical and descriptive. There is, however, an increasing tendency for it to become experimental in its method and for it to use the most recent explanatory conceptions developed in the study of human behavior considered irrespective of social significance.

(4) *Individual* anthroponomy is primarily concerned with individual differences in behavior. These problems involve differences in receptor capacities, in unlearned and learned equipment, and in general and specific capacities. The method of tests at present dominates the field, although training methods are equally applicable to its general problem. In general interest in this field is confined to differences which may be quantitatively evaluated.

(5) *Abnormal* anthroponomy is devoted to a study of all behavior which departs markedly from the norm. It thus deals with the very superior as well as the very inferior response. Its chief field lies at present in the abnormal forms of behavior which are not quantitatively measurable, *viz.*, the abnormal traits of personality and temperament, insanities, phobias, multiple personalities, dreams, etc.

(6) *Normal human adult* anthroponomy is the fundamental field of the science largely because of the fact that the normal human adult is the most accessible subject for experimental study. This field is concerned with an analysis of the normal human adult's behavior into its fundamental aspects.

The six fields of general anthroponomy which we have enumerated are among the most important of the organized subdivisions of the science at the present time. Any grouping of problems within the general field is arbitrary, however, and an even better insight into the nature and scope of the science of human behavior can be secured by considering the various aspects of the human individual which have been emphasized as points of organization for the subdivision of labor within the science. Individuals share alike an animal ancestry. They differ in age, sex, race, culture, nationality, in their physical and character traits. They may be studied with reference to a single trait analytically, genetically, or comparatively from trait to trait. Thus there may be, and is, a genetic anthroponomy, a comparative anthroponomy, as well as a racial, animal, social, and national anthroponomy, and an anthroponomy of personality, sex, infancy and childhood. Indeed circumstances may

even justify the terms laboratory, clinical, and field anthroponomy. Human nature, as we have defined it, can be adequately understood only when studied in these various significant aspects,—any one of which may be made a legitimate subdivision of the general science.

Let us now turn from a statement of the character and scope of general anthroponomy to a brief indication of its most important systematic problems. There are certain of these problems which at the present time command an attention only less absorbing than the problems of experiment and observation. A large portion of this interest is of a temporary character and grows out of the transition which is taking place from the psychological to the anthroponomical point of view. A certain portion of the interest, however, is permanent and concerns the theoretical problems which are inevitable in the systematization of any science. These permanent theoretical problems are concerned with the fundamental conceptions which constitute the skeletal frame-work upon which empirical material is placed. Conceptions of this character are abstractions made from the continuum of empirical data. They are thus artifacts, and their value depends upon their scientific usefulness. Atoms, electrons, light, heat, sound, and magnetism are illustrations of such abstractions in physics. The introspective psychologist utilizes such arbitrary divisions of the observable continuum as sensation, perception, memory, conception, volition, and thought. General anthroponomy likewise regards certain rubrics as particularly important and groups its materials under their names. We shall see later in detail what these are, but at present we must turn back to comment briefly upon that large interest in systematic anthroponomy which has been called temporary in character.

The systematic problem which is most prominent in contemporary discussion is that of the subject-matter of the science. This problem arises in psychology rather than in anthroponomy and concerns the question of consciousness. In preceding papers[1] we have discussed this topic in detail, and at that time attention was called to the three chief psychological points of view of structuralism, functionalism, and behaviorism. No psychologist has ever denied the existence of consciousness conceived as a different aspect of the universe from that constituted by matter, unless the behaviorists should be called psychologists. Philos- [illegible] of other scientific fields have done so at times since the days of the ancient Greeks, [illegible] has clung to the position that the psyche constitutes the essential

[1] W. S. Hunter, The Problem of Consciousness, *Psychol. Rev.*, 31, 1924, 1-31; The Symbolic Process, *ibid.*, [illegible].

aspect of human nature. The behavioristic psychologists have repudiated this position and thereby in fact, but not in name, have constituted themselves anthroponomists.

If consciousness and the bodily processes closely related to it constituted the subject-matter for study, various additional systematic questions would arise at once. (1) What is the relation of mind and body? (2) What methods are available for the study of consciousness? (3) What are the essential attributes of consciousness? (4) What are the constituent elements of consciousness? (5) How may the presence of consciousness be objectively determined? (6) What is the function of consciousness in the biological process? (7) How are the elements combined to form the complex conscious processes? (8) Into what combinations do the complex conscious processes enter? (9) Is the subconscious a verifiable datum? These fundamental systematic problems of the psychologist need not all be solved by the anthroponomist; but they must be absorbed and not ignored in his science, just as room has been made in chemistry for the observational data out of which the problems of the alchemist grew.

The concern over the subject-matter of the science of human nature can be permanently allayed only as a result (1) of an accumulation of experimental data which will give unambiguous evidence of what is being studied; and (2) of an understanding of the nature of consciousness derived not from philosophic or religious prejudice but from its actual empirical value in the scientific study of man. The problem of consciousness and the systematic questions growing therefrom cannot be successfully ignored. As we have just said, they must be absorbed into the science. It has been our purpose elsewhere to attempt such an absorption of the psychic problem into the field of anthroponomy. In the present paper we may therefore pass over these questions and attempt an enumeration of the systematic problems of anthroponomy. These may be listed as follows.

I. The nature of the subject matter. Three particular problems, each of which has numerous sub-heads, are to be noted here:

(*a*) Is consciousness a separate aspect of human nature?

(*b*) What are the essential characteristics of human nature?

(*c*) What is the pragmatic relationship of anthroponomy to the other sciences?

II. The general methods of gathering data in anthroponomy.

(*a*) Field observation, clinical observation, experiment.

(*b*) Introspection as a possible method.

III. Explanatory technique.

IV. The fundamental classificatory conceptions in anthroponomy.

The following list is suggestive, but it is in no sense exhaustive. Since the items are not logically determined, such a list is in constant revision.

1. Unlearned stimulus-response connections.
2. Learned S-R[2] connections.
3. S-R relationships in which two or more individuals are vital elements: custom, fashion, mob, tradition, crowd, audience.
4. Symbolic and non-symbolic S-R relationships.

[2]S-R = stimulus-response.

5. S-R relationships under (and not under) the organism's control through the self-excitation of receptors and possibly through central arousal.
6. Integration and disintegration of S-R connections.
7. Retention, re-instatement, recognition.
8. Controlled and uncontrolled, purposeful and non-purposeful, sequences of S-R relationships (free and controlled association).
9. Stimulus and response.
10. Interference and transfer.
11. Innate capacities, special abilities.
12. Irreversible sensory process-language response (SP-LR) relationships[3] (sensation, attention, images, emotions, etc.).

In connection with the fundamental classificatory categories, it is worth noting that in the transition from psychology to anthroponomy the fields of animal and individual psychology have had a leading rôle. This is chiefly because these fields have specialized in the objective (non-psychic) study of behavior. And yet, although the field of normal human adult psychology has been dominated by the shadow of the psychic, one should not ignore the objective character of much of the experimental work that has been accomplished in this field, the topics of sensory processes and learning being particularly important. It is probably because of these facts, *plus* the importance which attaches to the method of experiment, that the classificatory rubrics of general anthroponomy are chiefly drawn from these three fields of study.

Since we have already discussed the problem of consciousness in the articles already referred to, we shall confine ourselves in the remainder of the present paper to a brief examination of the second and third systematic problems above listed.

Field observation and clinical observation were given with experiment as two of the three methods of gathering data in the science of anthroponomy. Concerning experiment, nothing further need be added. The principle of experiment is the same in all sciences, and involves the artificial control and repetition of conditions for the accurate analysis and description of phenomena. Such experimental methods are employed most widely in normal human adult, individual, and comparative anthroponomy. They are being introduced on a small scale in the fields of abnormal and social anthroponomy. In contrast to the controlled and quantitative character of these methods stand the uncontrolled and qualitative procedures of field and clinical [illegible]. [illegible] observation is a term borrowed from biology to designate the method of [illegible] of subjects free in their natural habitat and undisturbed by experimental [illegible]. In biology the method is best exemplified in studies of homing,

[3] For the nature of these relationships see "The Problem of Consciousness" and "The Symbolic Process."

migration, reproduction, feeding, and other similar activities; in other words, in the great field of habits and instincts as studied by the naturalist. These studies have great value partly in their suggestiveness of specific problems and controls for experimental analysis and partly in the historical, sympathetic, and cultural view which they afford of the animals concerned. In anthroponomy this method of field observation, with the same values and defects, dominates the subdivisions of cultural and social anthroponomy and is essentially the method employed in physical anthroponomy. It is also employed in the compilation of case-histories in individual and abnormal anthroponomy. Clinical observation, although non-experimental and qualitative, differs from field observation in the following fundamental ways. (1) The subject is removed during observation from his natural habitat to special surroundings in which the observer is a dominant factor. (2) Through direct observation of the subject's behavior and through observation aided by measurement, the observer seeks a diagnosis of the subject's relative status of personality. (3) Because of the comparative point of view and the insistent need to evaluate the subject's behavior, clinical observation is less objective and is more open to the influence of prepossession than is field observation. This defect, it should be noted, extends beyond the fields of individual and abnormal anthroponomy wherever the method of personal interview, unaided by objective standards, is found. And (4) the method of clinical observation is typically an individual method of observation. In conclusion it should be pointed out that clinical observation accompanies much experimental work and furnishes a qualitative record of the subject's attitude and peculiarities of response. The typically qualitative character of the data gathered by field and clinical methods is undoubtedly largely responsible for the shortcomings of the fields concerned in the development of fundamental contributions to the classificatory concepts of anthroponomy.

In concluding this brief account of the character of general anthroponomy and its systematic problems, a few words must be added concerning the explanatory methods of the science. Explanation seeks to give the determining conditions for whatever phenomenon is under consideration. It always falls short of completeness, and it never pretends to be ultimate. The choice between the more and the less nearly ultimate is one to be made upon the basis of practicality conceived in terms of control and verification by the experimenter. There is, therefore, no one kind of explanation which can be called *the* method of explanation in anthroponomy. In general three types of explanation are available: explanation by analysis into component parts, by formulation of the genetic conditions, and by reference to receptor-conductor-effector mechanisms.

These three types are not always equally practicable. Where they are, the third method of explanation in terms of the individual's action-system is the most nearly ultimate. As the progress of science makes possible a physico-chemical explanation of receptor-conductor-effector activities, it is to be expected that this additional type of explanation will be employed, as is indeed even now the case in many physiological studies. We may illustrate the problem of explanation as it arises in anthroponomy as follows. The verbal response "box" following upon visual stimulation may be explained: (1) by an analysis of the response into its component elements through the analysis of the sound waves produced or by the registration of tongue and laryngeal movements; (2) by a statement of the number of trials, the total time, etc., of the learning process which was necessary in order that the behavior might occur; and (3) by a statement of the receptor activities which initiated the neural impulse, of the synaptic conditions present, and of the resulting effector activities. It is rarely possible at the present stage of neurological knowledge to make the third type of explanation anything other than suggestive when it is applied even to moderately complex behavior. This form of explanation, however, is approximated by stating the natures of the stimulus and the response and by filling in the neurological data largely by arm-chair methods. If we change our illustration from that of a verbal response to that of a custom, the third type of explanation may at once be ruled out as an actual method now employed. The explanations attempted of custom are in terms of analysis into component responses and in terms of genesis. This is also true of the explanations offered for many other phenomena, *e. g.*, pathological personalities. Perhaps the most prevalent form of the explanatory question in the science of anthroponomy is this: what are the historical conditions of the behavior in question? When we said earlier in the paper that anthroponomy is essentially genetic in its point of view, we referred primarily to the systematic attitude adopted toward its subject-matter. Here we wish to stress the point that anthroponomical explanations are also largely genetic, involving to a very large extent—as they do—accounts of the previous training of the subject as well as attempted descriptions of inherited probabilities of response. To this genetic explanation of a given phenomenon, neurological data are added where they are available.

It is worth noting that the psychologist has inevitably been compelled to resort to the scientific methods of explanation outlined above in order to escape from theories of the psychic causality and control of conduct. In like manner the alchemists were led to chemical explanations as they divorced themselves from the control of their magical prepossessions. When the psyche alone is considered, the psychologists have sought to explain consciousness either by a structural analysis or by a psychic causality, although some of them have taken the theoretically invalid position that the psyche can be explained in terms of neural process (see above, p. 287). The structural psychologists have thought that this latter method could be followed without abandoning a psychophysical independence; but the functionalists in their development of the field, although paying lip-worship to the [illegible] parallelism have made extensive practical use of a causal interaction of the psy[illegible] the biological process. No adequate neural or genetic explanation can be given for the problems which are conceived to arise in the realm of the psychic.

BOOK REVIEWS

Psychologie vom empirischen Standpunkt. By FRANZ BRENTANO. Mit ausführlicher Einleitung, Anmerkungen und Register herausgegeben von OSKAR KRAUS. Erster Band. Leipzig, 1924, F. Meiner. pp. xcvii., 279

In the reissue of Brentano's *Psychologie* Dr. Kraus, who is professor of philosophy in the University of Prag, has rendered a notable service to all students of psychology. For many years past Brentano's book has been—not difficult to procure, not procurable only at an exaggeratedly high price, but simply and flatly unprocurable. This fact alone might have prompted its editor to think twice before penning the second sentence of his preface: "the scientific world is only gradually becoming aware that the work was epoch-making." A whole edition does not disappear out of sight, and copies in private libraries do not resist the temptation of American gold, unless the book in question is quite especially valued. Nor would it be difficult to show that the psychologists of thirty years ago—Kuelpe in his *Grundriss*, Stout in his *Analytic Psychology*—were fully alive to the importance of Brentano's doctrine.

The new issue is to form two volumes of the *Philosophisch Bibliothek*; only the first volume (192) is now before us. There is good reason for the division, since Brentano himself published in 1911 a corrected reprint (with additions) of the last five chapters of his book (*Von der Klassifikation der psychischen Phänomene*, Leipzig, Duncker & Humblot. pp. viii., 167). The volume which I am here noticing gives us, then, the *Psychologie* down to the end of Bk. ii., Ch. 4. The second and as yet unpublished volume will give us the contents of the *Klassifikation*, the essay *Miklosich über subjektlose Sätze* (1883), five posthumous memoirs from Brentano's papers, and a much-needed index. So, for about five dollars, Brentano's work may henceforth be obtained by our graduate students. It is perhaps worth remarking, in the light of what I said above, that the new issue has not brought on the market—so far as I have been able to learn—a single copy of the original edition.

Dr. Kraus has written for the present volume, besides a general preface and a bibliographical list of Brentano's publications, an elaborate 75-page Introduction to his master's teaching. The chapters are entitled: (1) Brentano's distinction of 'descriptive' and 'genetic' psychology and his relation to phenomenology and theory of object; (2) Brentano's doctrine of the 'psychical relation' in its historical development; (3) Brentano's doctrine of the real as the exclusive object of our consciousness; (4) Brentano's attitude to psychologism; (5) the doctrine of outer perception; (6) Brentano's use of the expressions 'psychical' and 'physical' phenomenon; and (7) the method of descriptive or phenomenological psychology; inner perception and inner observation. All these discussions are of value; the second, third and fourth refer us directly to the appendix of the *Klassifikation*; but the mingling of exposition and controversy makes them difficult reading for a beginning student of Brentano. Still, nobody who is afraid of difficult reading will attempt Brentano himself.

I shall not try in this place to give any further account of the Introduction; its various topics need far more space than is proper for a review. [illegible] ginning—which seems to demand comment. The editor states positively that the *Psychologie* of 1874 contains aprioristic elements. After showing that descriptive psychology (the *Psychognosie* of Brentano's later classification) advances at a bound and without any sort of induction from general

concepts to general cognitions, Dr. Kraus writes: "These apodictic (always and wholly negative) cognitions, that are directly evident from the concepts, are however already known to the *Psychology* of 1874. 'Nothing is judged that is not ideated,' 'No loving or hating without ideation of that with which we are occupied emotionally,'—these, and many other psychological cognitions of which the *Psychologie vom empirischen Standpunkte* . . . treats, bear an aprioristic character" (xvii.f.; cf. lx., 268, 276). I do not, of course, deny Brentano's apriorism in the late eighties. I am strongly of the opinion, however, that there is no conscious trace of it in the *Psychologie*; if there were, Brentano must have been either deceitful or stupid,—and one hardly knows which adjective suits him the worse. For he says in his Preface: "My standpoint in psychology is the empirical; I recognize experience alone as my teacher"; and when he adds that as philosopher he entertains *eine gewisse ideale Anschauung*, he is at pains to declare that he finds it compatible with 'this standpoint' (this edition, 1). Moreover, the three chapters on psychological method are concerned with its experiential basis, with induction, and with deduction and verification; there is no word of the 'jump' made by descriptive psychology. Thirdly, the passages to which Dr. Kraus refers (112, 225, 227) appeal obviously either to experience or to argument; and the negative formulas are obviously rhetorical and not essential (cf. the positive formulas, 112, 120, etc.; and for argument cf. the 'demnach' of 120 and the 'dargetan' of 225). I cannot find, then, that there are aprioristic elements in the *Psychologie*. Indeed, on the basis of my own experience, I must go further; I doubt if a student of the *Psychologie* is prepared by that study for the part played by the *a priori* in Brentano's later thinking. I did not happen to read the tract *Vom Ursprung sittlicher Erkenntnis* (1889) until some time after I had become familiar with the *Psychologie*, and its *Wertaxiomatik* came to me as something altogether new.

So much, at present, for the Introduction. The text is, of course, a reprint of that of the original edition. Not much has been done in the way of 'textual' revision; *Tätigkeit* has been replaced by the correct *Fähigkeit* on p. 181; but the offending *als* has not been removed on p. 14, l. 9; on p. 65 the two sentences "Doch sind . . zuzuweisen" should have been enclosed in a parenthesis—else they are hardly intelligible; and so on. Dr. Kraus has, however, put in squared brackets those passages (cf. esp. 117, 202, 204, 219, 222, 230) which contain doctrine that Brentano himself has later rejected or corrected. The indications are useful; one wonders only why the "worunter hier nicht eine Realität zu verstehen ist" of pp. 124 f. has been allowed to stand unmarked (cf. 269 and *Klassifikation*, iv.). The running heads do not give the corresponding page-numbers of the first edition, and the running head of the recto does not give the section-number of the chapter. The footnote on p. 190 should be deleted.

The volume ends with some 25 pp. of Notes by the editor. The student will find these Notes exceedingly valuable. They clear up the meaning of doubtful passages, indicate shifts of terminology, furnish cross-references and outside references to related work, and provide in general just the sort of commentary that the beginner needs. Unfortunately, their indexing is of the one-way sort; the pages of the text refer to them, but they do not refer back to the pages,—though one may very well wish to turn from a given Note to the passage of the text which the Note illustrates.

Mechanically considered, the volume leaves a good deal to be desired, and the proof-reading of the Notes has been hasty; I have, however, not found any error that cannot at once be corrected by the reader. The book is light, and of the right size for a pocket.

E. B. T.

Helmholtz's Treatise on Physiological Optics, translated from the Third German Edition. Edited by JAMES P. C. SOUTHALL, Professor of Physics in Columbia University. Vol. I. The Optical Society of America, 1924. pp. xxi., 482.

Helmholtz was born in 1821. The Optical Society of America decided to celebrate the hundredth anniversary of his birth by publishing an English translation of the *Handbuch der physiologischen Optik*. It is curious that the work has never before been translated: possibly its length was a deterrent, perhaps there was no A. J. Ellis to undertake it, perhaps there was no guarantee of academic sales such as made the translation of the *Tonempfindungen* a promising commercial venture. We are now, at all events, to have the book in English; the first volume, Anatomical Description and Dioptrics of the Eye, is before us.

The editor has decided, and I think rightly decided, to make the English version essentially a reproduction of the third German edition. The German editors faced a very difficult task. They chose, as we all know, to ignore the second edition, and to reprint (with supplements of their own) the practically unchanged text of the original work of 1867. This was to cut the knot, rather than to disentangle it; but they adopted the more heroic measure with their eyes open, and their course has been generally approved. Psychologists, of course, must still make use of the second edition as well as of the first; and that is something of an inconvenience. It is, nevertheless, difficult to see how the second edition could have been converted, in reasonable space, into a Helmholtz memorial of the sort intended by the sponsors of the third.

The first volume of the English translation has as frontispiece the portrait of Helmholtz which appeared in the third volume of the German original. It contains also (443-482) a new supplement on ophthalmoscopy, reprinted from A. Gullstrand's *Einführung in die Methoden der Dioptrik des Auges des Menschen*, 1911. It has, further, a number of explanatory and corrective footnotes, initialled by the editor and his associates; these notes are crisp and authoritative—let us hope that the corresponding notes in the other two volumes will be of like nature! Aside from these additions, the book is a reproduction of the first volume of the German third edition.

The translation is loose, looser perhaps than it need have been, though in the dozen passages that I have compared with the German text there is rarely any serious distortion of meaning; every now and then, however, the translator slips, as in the sentence about Carion on p. 257. Technical terms —the technical terms of the psychophysicist—come off at second best. Our familiar dispersion circles appear as 'blur circles' or 'circles of diffusion' (121), although the word *Dispersion* occurs in Helmholtz' own index, and although both Baldwin's Dictionary and the new Century Dictionary give the accepted usage. The *Visirlinie*, which is the sighting line, is rendered 'line of sight' (124), and *Gesichtslinie* is translated 'visual axis' (97). *Gesichtsfeld* becomes 'field of view' (93). Fortunately, all these terms are rightly translated in Baldwin's Dictionary, and the Dictionary has had time to make its influence felt.

The *Optik* is a serious book, and the translators have taken it seriously. I do not think that the phrase "this youthful and at the same time effervescing field of knowledge" (viii.) could have been penned by anyone who was not in such deadly earnest that his sense of humor was in abeyance. "Concentric circles . . . very close together but at equal distances apart" (191) suggests the tall but respectably dressed stranger. The passage about the *Zolltel* [illegible] Helmholtz might have been allowed his little joke ('tenth' is, after all, not much less euphonious than 'twelfth') especially since the joke did not go too far.

I have touched only on minor points; the volume as a whole does not fall within the competence of the psychologist. The remaining two volumes will call for fuller treatment. E. B. T.

Intelligenz und Wille. By E. MEUMANN. 4te verbesserte Auflage, herausgegeben von G. STOERRING. Leipzig, Quelle & Meyer, 1925. pp. xii., 360.

Meumann published the first edition of this book in 1908. In the following year, Wundt reviewed it in the *Psychologische Studien.* Wundt wrote more in sorrow than in anger, but his sorrow expressed itself in some fairly severe criticism. Meumann, apparently, was not impressed; in the preface to the second edition of 1913 he says that few of the many reviews that the book received have helped him, and he makes no mention of Wundt's. After Meumann's death in 1915 the care of the book fell to his friend Professor Störring, who brought out a third edition in 1920 and is responsible for the present, fourth, edition of 1925. Professor Störring has wisely confined himself to the writing of notes—defensive, critical, illustrative—appended to the various chapters. He takes his stand, in general, as defender of Meumann's views against the attacks or counter-opinions of contemporaries.

The *Intelligenz und Wille* is, of course, a contribution to the psychology of personality, differing from most essays of its kind in that it seeks to go back to 'psychical elements.' As these elements, however, are mainly 'types' and 'capacities,' the book has a distinctly popular flavor.

The editor might, perhaps, have removed one or other of the identical phrases on pp. 222, 230. The repetition is natural enough for Meumann, who wrote under stress of emotion, but there is no reason to retain it in cold blood. The subject-index seems to have been revised, though it has some mistakes. The index of names is quite untrustworthy, both by mistake and by omission. The references given to Wundt are 120, 138, 224, 344, 345; I make them (and there may be others) 115, 132, 206, 221, 222, 224, 225, 230, 330, 344, 345. Helmholtz and Herbart, *e. g.*, do not figure in the index; nor does Wentscher—curiously, since Meumann himself mentions her in his preface of 1913.

E. B. T.

Dynamic Psychology. By THOMAS VERNER MOORE. Philadelphia, London and Chicago, J. B. Lippincott Company, 1924. pp. viii., 444.

Two tasks are attempted in this text: (1) the venturing of a synthesis of behaviorism and introspectionism, and (2) the meeting of a growing demand for a systematic presentation of the dynamic point of view in psychology.

The first task is approached with a definition of psychology as the science of human personality. More or less adopting Kant's tripartite division of the mental life into knowing, feeling and doing, the author supplements introspection with behaviorism in this wise: he presents the cognitive aspect of mind introspectively, while the conative and affective aspects are considered from both the introspective and the behavioristic standpoints.

The second task is commenced with a positing of impulses as the driving forces of human nature. Here follows a subtle differentiation of impulses from the instincts with which they may be easily confused. Impulse is defined as a tendency to action of which we are focally or marginally aware prior to the action itself. In other words, it is a conscious or subconscious urge to action of a specific sort. At once impulse assumes the rôle of a conscious element destined to initiate action of some sort. An impulse precedes in consciousness every unit action. Consequently, the number of impulses is unlimited. On the other hand, the various so-called human instincts, like the various so-called human habits, are but general names for large groups of impulses subjectively considered.

Impulses are divided into motor, sensory, affective and intellectual, according to the part of the personality which stands in need of exercise. Here impulses appear familiar to the student of introspection as simple feelings which activate us. The inhibition of a lively impulse transforms impulse into desire or craving. Impulses often conflict, and the stronger prevails; for it is assumed that these elemental impulses vary in strength.

Having thus grounded dynamic psychology, Moore proceeds to recapitulate the psychoanalytic doctrine of wish. It must be added that to the discoveries of Freud, Adler and Jung the author contributes something of his own.

The closing chapters of the book are devoted to a critical analysis of volition, from which the reader emerges somewhat confused. The author, owing to his psychodynamic premises, is compelled to refute James' ideomotor theory of volition and also Woodworth's theory of conceptual control. A voluntary act, he admits, is preceded by an idea or thought of the action to be performed, but this idea or thought is not the cause of the act. Prior even to the idea and provocative of it is the impulse, the veritable creative fiat.

To the freedom of the will Moore subscribes with certain reservations. We are free in the sense of being at liberty to choose the means that make us happy; while, on the other hand, we are compelled by nature to seek to be happy. He would emancipate us from the tyranny of external circumstance, but not from that of our internal drives.

The book has appended to it a glossary of technical terms. Its style is simple and fluent; its logic is somewhat intricate.

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Introductory Psychology for Teachers (*Revised*). By E. K. STRONG, Jr. Baltimore, Warwick and York, Inc. 1922. pp. ix, 255.

In this text, Strong, the author of *The Psychology of Selling Life Insurance*, has brought forth such a mass of material illustrative of the many problems in education that it is rather difficult to get down to the fundamentals. Perhaps, however, one should not properly speak of illustrative material, for the writer is not merely using illustrations to clarify definitions and theories, but has so planned the work upon the inductive method that the student may work ahead and develop his own concepts. "The even numbered lessons present problems to be solved and the odd numbered lessons supply in a general way answers to the problems, together with a broader interpretation than the average student will discover for himself." As the book is designed for prospective teachers who have not studied psychology and who will not pursue the subject farther, the results of using such a method will probably be satisfactory enough. The reviewer finds it very difficult, however, to start out without a few guiding concepts.

The psychological interpretation is based entirely upon the situation-bond-response theory.

Angewandte Psychologie: Methoden und Ergebnisse. By [illegible]

This little manual is a convenient summary of the methods and results of Applied Psychology. It covers the usual subjects of intelligence tests, [illegible] psychology, work and fatigue, [illegible] psychology. For one who wishes a short account of the status of Applied Psychology [illegible] will prove [illegible].

Methodensammlung zur Intelligenzprüfung von Kindern und Jugendlichen. By WILLIAM STERN and OTTO WIEGMANN. Zweite stark vermehrte Auflage. Beiheft 20, Zeitschrift für angewandte Psychologie. Verlag von J. A. Barth, Leipzig, 1922. pp. iv, 316.

This monograph upon tests and test-methods should prove valuable to those interested in tests. In the past ten years, many new tests have been developed in Germany and few of them have become known in this country. The more recent tests in Germany have this point of difference from American tests: in Germany, more attention has evidently been given to the psychological background of tests, whereas we have been more concerned with the statistical treatment—our tests are more empirical.

The authors give a classification of both single and group tests with a complete bibliography. Although the attempt has been made to include all of the work upon this subject, many of the American tests have escaped attention.

Schriften zur Psychologie der Berufseignung und des Wirtschaftslebens. Edited by OTTO LIPMANN and WILLIAM STERN. Published by J. A. Barth, Leipzig.

Heft 11 **Methoden zur Auslese hochwertiger Facharbeiter der Metallindustrie.* By OTTO LIPMANN and OTTO STOLZENBERG. 1920. pp. 79.

" 12 **Kurzer Bericht über Arbeiten zu Eignungsprüfungen für Flieger-Beobachter.* By W. BENARY. 1920. pp. 61.

" 13 *Fragen zur Erforschung des Wirtschaftslebens der Naturvölker.* By RUDOLPH STEINMETZ.

**Die Wirtschaftspsychologie und Berufseignung in Überseeländern.* By HANS HENNING. 1920. pp. 52.

" 16 **Untersuchungen über die Fehlleistungen beim Maschinenschreiben.* By WILH. HEINITZ. 1921. pp. 17.

" 17 **Die Berufseignung des Damenfriseurs.* By R. W. SCHULTE. 1921. pp. 77.

" 18 *Die Berufseignung des mittleren kaufmännischen Bureaubeamten im Buchhandel.* By JUSTUS STRELLER. 1921. pp. 61.

" 19 **Die Monotonie der Arbeit.* By HERBERT WINKLER. 1922. pp. 45.

" 20 **Bibliographie zur psychologischen Berufsberatung, Berufseignungsforschung und Berufskunde.* By OTTO LIPMANN. 1922. pp. 60.

" 21 *Die Münchner Eignungsprüfung für Buchdrucker und Schriftsetzer.* By ALBERT HUTH. 1922. pp. 28.

" 23 *Taylorsystem unter schwerer Muskelarbeit.* By HEINRICH DOEVENSPECK. 1923. pp. 38.

These monographs (those marked by an asterisk are off-prints from the *Zeitschrift für angewandte Psychologie*) are from the series edited by Lipmann and Stern. The general purpose of the series is the scientific study of vocational and economic psychology and its applications to the fields of social, economic and pedagogical organization. In general, the investigations are well carried out and interestingly presented.

The investigations dealing with specific trades, such as the metal industry, aviation, printing, etc., are of a more definite value. A careful analysis—a job-analysis—has been made, and various experimental methods adapted to the measurement of the traits necessary in the particular trades have been ingeniously devised. Vocational psychologists and employment managers will find these monographs, and also Lipmann's Bibliography (Heft 20), both useful and suggestive.

It is interesting to note that the interest in vocational psychology and the use of diagnostic tests in employment work depends to a certain extent upon the condition of the labor market. Vocational and employment psychology received a good deal of attention in this country a few years ago, when there were a good many unemployed. More recently, less interest has been manifested in this field, and much of the work undertaken by corporations, industries, etc., has either been curtailed or abandoned. With the present unemployment in Germany, a similar demand for precise methods in the selection of employees and for trade studies is evidently making itself felt.

Cornell University SETH WAKEMAN

NOTES

EXPERIMENTS ON THE COMIC

In the time that has elapsed since my paper "Laughter, a Glory in Sanity" appeared in the JOURNAL for July 1922, I have seen no comment on my theory, and the subject is doubtless of slight current interest to psychologists, who are engrossed in more urgent problems. Presumably no one has gone so far as to attempt to devise tests for the theory. I have tried to think for myself how it might be tested, and I here set down my ideas.

The theory, if true, implies the following corollaries.

(1) Everything comic states or suggests something perceived as a falsehood.

(2) Nothing conveying a falsehood is thereby comic, so long as it deceives.

(3) Nothing conveying a falsehood is thereby comic, unless it tends to deceive, or, in other words, unless an effort of judgment is needed to perceive the falsity.

(4) Anything conveying a great enough falsehood that is suddenly perceived as such by an effort of judgment is comic (for a free and susceptible mind).

(5) Of two things conveying falsehoods perceived as such by equal efforts of judgment, the one conveying the greater falsehood is the more comic.

(6) Of two things conveying falsehoods of equal degree, the one requiring the greater effort of judgment to perceive the falsity is the more comic.

The first two corollaries contain, in different terms, the substance of Schopenhauer's analysis of the comic. He said that the source of the comic is the incongruity between what we think and what we perceive. M. Eastman, in "The Sense of Humor," remarks that this is merely a philosophic way of describing an error,—I should say rather, a detected falsehood. Most or all writers on laughter recognize incongruity as the base of the comic but I have found none who has pointed out that incongruity is simply the opposition of what we consider as true and what we perceive as false, except in so far as this is implicit in Schopenhauer's way of putting it. This seems to me to [illegible] to explain why incongruity should excite a pleasant emotion.

The words "effort of judgment," in the propositions given above, contain [illegible] of whatever is new in my theory. If it could be proved that an effort of judgment always occurs when the sense of the comic [illegible], and that the sudden rejection of a sufficient falsehood by an effort of judgment

will always arouse a sense of the comic in an open mind, it would then be manifest, I take it, that the agreeable quality of the emotion is associated in some way with the power of judgment. If it could be proved further that the intensity of the emotion varies directly with the effort, and also with the degree of discrepancy overcome by the effort, the final inference that the emotion is pleasure in the power of judgment would seem to me unescapable.

Tests of the theory, therefore, might be directed toward determining whether an effort of judgment is thus related to the sense of the comic, in the rejection of falsehoods. The task may be formidable, because the "effort of judgment" thus involved in the theory is by no means always on the surface of consciousness. It does not mean obvious effort, necessarily, but a relatively strong exertion of the faculty that works abundantly and unceasingly in every conscious moment to interpret and correlate experiences. Obviously, such an exertion is not readily to be caught and measured in the act. But we can study at leisure the material on which it works, and we can select for this study matter that has been stripped of unessentials.

My idea of a working method would be to take a random series of bald statements contrary to familiar fact, to make actual tests of their relative comic value, and then to study them to see whether these relative values can be accounted for by corresponding degrees of falsity and deceptiveness. I would first ask a friend, uninformed of my purpose, to get me up a list of (say) 20 "brief, snappy lies," without other restriction. Or perhaps it would be better to get one lie each from a number of persons. I would write these false statements on numbered cards and submit the pack to different persons, preferably to bright children 12 to 15 years old, asking them to arrange the cards according to the degree in which the statements appealed to them as funny. After each such test, of course, due record would be made and the cards shuffled. If the result was a fair degree of uniformity, an attempt at analysis might prove profitable.

When a large number of falsehoods had thus been valued and analyzed, I should hope to have acquired a better understanding of the exact nature of the element of plausibility or deceptiveness, and also of just what I mean by "degree of falsity." I would then attempt a final test by trying to devise falsehoods that would conflict with the conditions posed in my six corollaries, especially the fourth.

I say I would do these things, but I do not propose to. My technique would be too crude. My hope is that some skilled experimenter may find meat enough in the suggestion to translate my rough ideas and lay language into proper psychological terms, and undertake to prove me right or wrong.

A single instance may help to show that there is profit for the student of the comic in the scrutiny of barefaced falsehoods. It will be recalled that among the 'many things' of which the Walrus proposed to talk with the Carpenter were

"Why the sea is boiling hot
"And whether pigs have wings."

These lines still hold the profoundly comic flavor that they had for me as a child. Yet it is not particularly funny to say "The sea is boiling hot," or "Pigs have wings." What is the difference? These bare extravagances have simply been wrapped in a kind of plausibility by suggesting that one is a familiar but unexplained fact and the other a doubtful subject for debate. The lines gain a heightened effect, of course, by immediately following so genuinely rich and stimulating a list of topics for conversation as the classic "shoes and ships and sealing-wax, and cabbages and kings." I do not know what primal longings may be satisfied by utterance of the lines that I have quoted, nor what elements of social correction or palliation of grief they may contain. But I am sure they give me pleasure, and I believe this pleasure, in its plain and palpable aspect at least, is nothing more than recurring delight at not being fooled by them.

W. R. Carpenter

Demonstrations of Affective Method

A recent article in the Journal mentions two demonstrations designed to accompany lectures on simple feeling, "a demonstration of the method of impression, by way of paired comparisons of colors, and a demonstration of the method of expression, by way of the pneumograph" (34, 1923, 471). The materials of these demonstrations are as follows.

Method of Expression. The kymograph has a light metal drum, 12 in. high and 12 in. in diam., which revolves (roughly) once in the 1 min. The construction is as simple as possible: the clock-house forms the base of the instrument, and the shaft of the drum stands free, without lateral support. The recording tambour is an exaggerated Marey tambour, about 5 in. across and 1 in. deep. The capsule (transmitting tambour) is of the Paul Bert type, home-made from a coffee-tin. The time-marker utilises the clock-work of a metronome. The two writing points are wound with wisps of cotton-wool, so that the curves drawn on the smoked paper are some eighth-in. wide.

The whole intent of the set-up is to demonstrate procedure; the audience watch the record of the assistant's breathing, and the writing of the time-line beneath it, and thus (one hopes) are prepared for the following discussion of method and exhibition of experimental records. If adjustments have been made beforehand, the demonstration runs off quickly and smoothly, without interruption of the lecture.

Kymograph, recording tambour and time-marker were built for me by the C. H. Stoelting Co. The tambour and time-marker can be simplified, and the demonstration in so far improved. The dimensions of the various pieces were chosen for an audience of two hundred.

Method of Impression. A sheet of neutral grey cardboard (dimensions will vary with circumstances) is fastened to a stout frame of blackened wood, which stands on a black wooden base. The cardboard has, at its center, two square windows of convenient size, set side by side. I will call this cardboard the 'screen.'

Behind the screen stands the 'first slide,' made of the same grey cardboard. The first slide come down just low enough to cover the windows in the screen. Above the windows it has an oblong cut-out, large enough to uncover the windows. Its top edge extends above the screen; the corners of the projecting edge are bevelled off.

Behind the first slide stands the 'second slide,' also of cardboard. It carries three pairs of color-squares, slightly larger than the windows of the screen, arranged pair above pair; the uppermost pair lies directly behind the windows, so that, if there were no first slide to cut them off, the colours would show through the windows.

As the apparatus stands, then, the audience see the two windows of the grey screen filled with the same grey (of the first slide). In demonstrating, the lecturer pushes down the first slide, against a spring, and so brings its cut-out behind the windows. The second slide stays in place, held by a ratchet behind; and the colors of the top pair appear in the windows. The lecturer removes his hand; the first slide is drawn up by the spring, till its grey again fills the windows; at the same time a ratchet on it lifts the second slide, and the middle pair of colors takes the place formerly occupied by the top pair.

see the middle pair of colors in the windows. He removes his hand; the first slide comes up to fill the windows, and lifts the second slide with it as before; the lower pair of colors is thus raised into the position originally occupied by the top pair.

The lecturer pushes down the first slide a third time, and the audience see the bottom pair of colors in the windows. He removes his hand, and the colors are replaced, finally, by grey.

The apparatus—which, in its latest form, is largely the work of Dr. Boring and Dr. Bishop—thus permits the serial exposure of three pairs of colors (in principle, of course, of more; but three are enough) by as many simple hand-movements. There are no loose colors to get dirty; the set-up is permanent. Also there is no loss of time for the lecturer.

E. B. T.

The Sanford Foundation

Mrs. W. F. Frear of Honolulu proposes to give one thousand dollars to Wellesley College as nucleus of the Edmund Clark Sanford Foundation for Psychological Research. Mrs. Frear, who is a graduate of Wellesley, was a student in the school to which Mr. Sanford went as instructor immediately after leaving college. She makes the gift in grateful memory of his teaching and of his friendship. It is the more appropriate in that Dr. Sanford was keenly interested in the foundation, more than thirty years ago, of the Wellesley Psychology Laboratory. Some pieces of the original apparatus were made under his personal direction; and indeed he played, in those early days, the rôle of its kindly Providence. The friendly relation was never broken and one of the last letters which he wrote was to fix the date for a lecture at Wellesley. It is hoped that not a few of Dr. Sanford's friends may care to add to the endowment of which the accumulated interest will from time to time be used to purchase apparatus or to defray expenses of publication. Gifts or pledges may be sent to President E. F. Pendleton or to Professor M. W. Calkins, Wellesley College, Wellesley, Mass.

Habitual Motivations

Stories of horses turning up a familiar lane, of the doctor's horse pulling up at the home of a former patient, of the milk-man's horse turning in at every house on the milk route, have often been told as evidence of 'memory' in animals. A different explanation of at least some of these incidents is suggested by the fact, which has recently come to my attention, that the automobile sometimes behaves in a similar way. Several physicians have told me that they find themselves, not infrequently, turning in, stopping, and even getting out with their medical cases at the house of some former patient. Since, unfortunately, none of my informants has kept a record of these occurrences, I cannot attempt an analysis. Abstraction, however, seems to be a necessary condition; for all the instances reported to me occurred when the physician, on his morning rounds, was tired or worried, and was driving automatically. It is reasonable to assume that, in some instances at least, the horse is similarly guided; and that, in both cases, abstraction plays the same rôle as it does in the common experience of catching oneself, when intending to change one's coat or tie, somewhere in the process of complete undressing.

K. M. D.

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EXPERIMENTAL PSYCHOLOGY: A RETROSPECT[1]

By E. B. TITCHENER

Experimental psychology, as we all know, took shape against a background of physics and of a physiology informed by physics. Fechner was a physicist; Helmholtz was physicist and physiologist; Weber and Hering and Wundt were physiologists. The new science, then, was to be a science of the type of experimental physiology; Wundt called his great compendium of 1874 Principles of Physiological Psychology; and in all sorts of ways—in choice of subjects for investigation, in the manner of presentation of published results, in the psychologist's general mode of approach to experimental problems—the influence of physiology was manifest. Think of the early problems! — sensory quality and sensory intensity, the reaction experiment in its various complications, the time sense, the range and fluctuations of attention, the perception of tactual and visual space; they were one and all of them problems that had a physiological aspect and that suggested an experimental control by instruments of physiological type. Wundt's Physiological Psychology is a tremendous advance upon the Lectures of eleven years before; yet a very great deal of it is anticipated in the third edition of Wundt's own Physiology.

These things, in the large, are known to all of us, and I do not wish to spend time on the familiar. We cannot, however, if we are taking an historical view, omit facts simply because they are obvious; and besides, the facts of which I am reminding you are, in my judgment, of quite first-rate importance. I believe that experimental psychology had an extraordinarily fortunate birth. No doubt, the early investigators set to work too hastily and too confidently. No doubt, we have wasted an immense

[1]This paper contains the substance of an address delivered before the twenty-second annual meeting of Experimental Psychologists, April 9, 1925, at the dedication of Eno Hall, Princeton University.

amount of energy on Weber's Law and Helmholtz' theory of visual sensations and the windinesses of nativism and empiricism. No doubt. But when all is said it remains true that the initial impulse was sound and the initial perspective correct. If physics and physiology and psychology—let us rather use the more general word, and say: if physics and biology and psychology cannot be housed under one roof, as members of a single family, then the term science can have no stable meaning. The accomplishment seems more difficult to us than it did to the mid nineteenth century; our store of facts is more bewildering, and we put less trust in our coordinating concepts. The ideal, nevertheless, of a biology modelled upon physics, the mother of the sciences, and of a psychology modelled upon that physically informed biology holds for us no less than it held for our fathers.

So experimental psychology, as it started on its course, was impelled by the inner urge to be a science in the manner of experimental physiology. What, now, were the external influences that bore upon it, and under what environmental conditions did it grow up? The environment, I need not say, was hostile; and if we are to 'know ourselves' as we are today,[2] we must take some account of the kind and degree of that hostility.

The first of the three major influences that wrought against the establishment of experimental psychology—for as I look back on our history, I distinguish three such influences—was local and temporal; it was the influence of Herbart and the Herbartian psychology. To most of us, I suppose, Herbart is little more than an historical name; to Wundt in the seventies Herbart was an all-pervading institutional opponent.[3] Leipzig, in particular, the university to which Wundt was called in 1875, had long been the head and centre of the Herbartian movement. Hartenstein, Herbart's editor, had it is true given up his chair some fifteen years earlier. But Ziller was still there,—Ziller, who was one of the original editors of the *Zeitschrift für exacte Philosophie*, the technical organ of the Herbartian school, and who in 1868 had founded the hardly less important *Verein für wissenschaftliche Pädagogik.* Drobisch and Strümpell, two of the greatest names among the Herbartians, still held their Leipzig chairs,—and were to hold them for some time to come, since Drobisch lived to 1896 and Strümpell to 1899. In 1875 Lazarus and Steinthal, the founders of the *Zeitschrift für Völkerpsychologie und Sprachwissenschaft* (Wundt had already had a tilt with them in 1863) were professors in Berlin; Volkmann and Lindner were at Prag,—the whole of Austria, indeed,

[2]The Delphic maxim γνῶθι σαυτόν is carved above the door of Eno Hall.

[3]It is worth remembering that the lives of the two men overlapped; Wundt was nine years old when Herbart died.

had been Herbartianised by Bonitz and Exner; Waitz was at Marburg; Stoy was at Jena. To us, truly, these men are just a list of names and dates; to a nascent experimental psychology they represented a highly formidable opposition.

It would, of course, be a very foolish critic who should say that Wundt became a voluntarist because Herbart had been an intellectualist, or that Wundt made psychology the basis of the *Geisteswissenschaftèn* because Herbart had severed theoretical from practical philosophy and had derived his psychology in part from metaphysics. Yes, but it would be a foolish reader, too, who should forget that all through the second part of the Physiological Psychology Wundt has Herbart steadily in mind; that the improvised doctrine of apperception is meant as a counterblast to Herbart; that the whole of Wundt's psychology beyond the chapter on perception is shaped with polemical reference to Herbart. That, you see, is the sort of trick that history plays upon us; a good many of the later Wundtians have been anti-Herbartians without realising it. So that the effect of Herbartianism upon experimental psychology was in reality twofold, internal and external. The Herbartians were in possession; they must be dislodged, superseded, discredited, if experimental psychology was to grow to power; that was the external side of things. Internally, meantime, the very fact of having to combat a well-rounded and critically tested system tinged and moulded the doctrines of experimental psychology itself. I shall not try to give you further details; let me only remind you, before we leave Herbart, that the struggle lasted over into the present century; Wundt's rejoinder to Delbrück appeared in 1901.[4]

The second unfavorable influence which I have to note is of far greater importance,—partly because of its antiquity and perennial vitality, partly because of its seeming innocence and the guise of kinship with experimental psychology which it often assumes, so that it works insidiously from within rather than openly from without. It is the influence of empirical psychology, a mode of psychology that goes back to Aristotle and Thomas Aquinas, and that forms the staple contents of most psychologies, down to and including our twentieth-century textbooks. The empirical psychologist professes to take mind as he finds it: I use the term 'mind' without prejudice, for the subject-matter of empirical psychology; in point of fact, the definition of that subject-matter has varied very greatly: always, however, the empirical psychologist professes to take [illegible] use, whether in a man's converse with himself or in his inter-

[4] *Sprachgeschichte und Sprachpsychologie, mit Rücksicht auf B. Delbrücks "Grundfragen der Sprachforschung,"* esp. I. and II.

course with his fellow-men. This subject-matter, then, mind in use, the empirical psychologist seeks to reduce to order and arrangement; and empirical psychology thus becomes a rationalisation of practice, a 'description and explanation' of mental uses. Our sister-science of biology suffers from the same sort of contamination: I have read textbooks of physiology that were in essence nothing else than rationalisations of the art of medicine, and I have read textbooks of biology that contained little more than a 'description and explanation' of the practice of adaptation. Biology, fortunately for her, stands nearer than we to physics, and has that great example more closely before her eyes; none the less, biology has suffered. Experimental psychology has suffered immensely more.

It happened that the year 1874, which saw the publication of the Physiological Psychology, was also the year of the appearance of Brentano's Psychology from the Empirical Standpoint,—a masterly and masterful work which shows the temper of the empirical psychologist in exemplary form. Brentano's book made from the first a deep impression upon psychology, and this impression has but deepened and widened as the years have passed. It did not, however, come on the scene as a third combatant, opposed equally to the Herbartians and to experimental psychology; things were not as simple as that. On the one hand, Brentano did not think it worth while to challenge Herbart on even terms. His teacher Trendelenburg had already disposed polemically of the Herbartian metaphysics; and Brentano, when he has to speak of the Herbartian psychology, dismisses it in a few polite but very decided sentences as an obvious failure. Though, then, the Empirical Psychology gives a round dozen references to Herbart and the leading Herbartians, it deals with them only in matters of detail; it looks on the dominance of the Herbartian school as an historical accident, of no intrinsic importance; it shows nothing of the obsession that we have noted in the Physiological Psychology. Wundt, in this regard, had found an ally; but the ally showed a certain aristocratic indifference toward the common enemy. On the other hand,—and this was the real danger for experimental psychology,—neither Brentano nor Wundt saw that his own conception of psychology refused and denied the conception of his rival: that an empirical psychology, a rationalisation of mental practice, was utterly at variance with an experimental psychology that should be a science of fact and law coordinate with experimental physiology and experimental physics. Wundt still had one foot, so to say, in the empirical camp; and Brentano was ready, subject to his own view of psychological method, to adapt the procedures of experiment to his empirical purposes.

You will expect me, now, to compare and contrast the two books, the Physiological and the Empirical Psychologies. You will realise, from what I have just said, that the contrast will not be clean and sharp; there is in Wundt a great deal of empirical psychology. That apart, however, there is still a preliminary point to be made, before we can draw our parallels. Empirical psychology is not to be defined by the contents of any single book. I have called Brentano's work exemplary; but that does not mean that his doctrines would find universal acceptance, even among his fellow empiricists. For it is of the very essence of empiricism, you remember, to classify and explain the uses of mind; and that is a task which calls less for facts than for ingenuity; and where ingenuities are in play, systems will differ individually. Let me take an illustration; and I choose one in which both the men of whom I am speaking are involved. We find that the Wundt of 1874 tends to delimit psychology, as the later Wundt overtly delimits it, by point of view and not by subject-matter; there are for Wundt no psychical, there are only psychological phenomena. We find that Brentano, on the contrary, recognises psychical phenomena as a special and distinct field of subject-matter. But on this point Brentano is at one with Herbart, the metaphysical psychologist, while Wundt represents a tendency that first appears among the empirical psychologists of purest blood. The definition by point of view has its source in the empirical psychology of Locke; and it has been adopted, in our own day, by Ward and (in his later years) by James,—by the two most distinguished and most representative empirical psychologists of the English-speaking world. Should we not have expected that Brentano, steeped as he was in the traditional British psychology, would work out to the position of Ward and James rather than range himself with Herbart? The question plainly asks too much; we must not forget Brentano's training; but at any rate the illustration shows that Brentano is not 'typical of his kind.' Or let me take another instance: we tend to think of the doctrine of association of ideas as 'typically' empirical. Here again, however, Brentano would have gone his own way. He would, we know, have discussed association when he came to deal with the laws and properties of ideas,—and what he would have made of it we may, perhaps, guess in part from the treatment accorded it by Stumpf; but he would have left it behind when he was dealing with judgments and with feeling and will. Wundt, too, revolts from associationism; but the revolt is not experimentally motived. On the contrary, the doctrine of apperception is no less empirical than that of association; it bases on the 'feeling of activity'; and since activity is not a psychological quality, the 'feeling of activity' already im-

plies a rationalisation of psychological experience, quite in the empirical manner. In this instance, therefore, both Brentano and Wundt diverge, within empiricism itself, from what we are tempted to call the empirical 'type'. It is clear that not everything that Brentano says is characteristically empirical, as I have also shown you that not every difference between Brentano and Wundt is a difference between empirical and experimental psychology.

When we put the two books side by side, we find, indeed, far too much agreement. Wundt, like Brentano, accepts from the older empiricists a whole array of classificatory and explanatory terms: consciousness, attention, association,—perception, emotion, memory, imagination. If only he had had the insight to throw them all away! Wundt possessed, in fact, just one clear concept, the concept of sensation as composed of intensity and quality; and that concept was neither happily chosen nor, after its adoption, adequate to support a whole experimental psychology. All the rest were foggy from much argument; and though Wundt made heroic efforts to clarify some of them—notably in the instance of consciousness, which he sought to redefine in a strictly experimental atmosphere—they were in the end too much for him; empirical they had been, and empirical they remained. From our point of later vantage we can see that a tremendous opportunity was lost; but we have no right to think either that Wundt should have seen that opportunity or, if he had, should have thought himself strong enough or privileged enough to seize it. At any rate, nobody has been strong enough since; we ourselves are getting rid of the equivocal and theory-ridden terms piecemeal, a little at a time; and it will be long before experimental psychology is finally free of them.

The acceptance of these concepts meant, naturally, the acceptance of certain positions with which they were historically connected; and so, for example, we find Wundt as ready as Brentano to make psychology the basis of the mental sciences. It is true that Brentano is looking particularly toward logic, while Wundt has rather in mind disciplines like history, ethics, jurisprudence. It is true also that Wundt puts experimental psychology at the lower end of the psychological scale, nearest to natural science and furthest from the mental sciences. All the same, he makes psychology continuous throughout, and does not see that he changes his attitude—from experimental to empirical—as he travels upward; that is one of the reasons why the later *Völkerpsychologie* is psychologically unsatisfactory. We can only regret, once more, that his vision was not clearer, and try to take care ourselves, now that our eyes are opened,

to avoid the sort of confusion of thought that this example betrays. The task for us is very far from easy.

What, then, of the differences between the books? The differences are unmistakable. Let me remind you, to begin with, of the difference in treatment of psychological method. Three of Brentano's thirteen chapters are given over to the discussion of method. Wundt does not discuss method at all; he names it in passing, and gets on at once to the account of experimental procedures, the description of available apparatus.[5] Surely, that single difference would be enough to prove that Wundt, however imperfectly he realises what he is about, has in fact attained to a new standpoint in psychology. A science is concerned to set forth its facts and uniformities; it has no call and, as science, it has no competence to treat of method. The textbooks of the pattern sciences, experimental physics and experimental physiology, do not dream of introducing chapters on method; they leave all such preliminaries to logic, which has a place reserved for methodology; and Wundt's own *Methodenlehre* appears, accordingly, not in the Psychology of 1874, but in the Logic of 1883. Brentano, on his side, was entirely consistent. Since empirical psychology is, as I have pointed out, a work of ingenuity, since logical argumentation is of its very texture and substance, it involves a special methodology as an integral part of its construction; science is transformed into an applied logic. There is the difference; and in the effecting of the difference experimental psychology had chosen the right scientific road. Would that we might have been allowed to walk in it! But controversies began; Wundt was presently beguiled into discussing method within his psychological system; and our experimental textbooks, ever since, have done homage to logic. Remember, nevertheless, that we were started aright, and that if we ever succeed in ousting empiricism and in banishing method to its proper limbo we shall only be recovering the position that Wundt once occupied for us.

This difference, the stressing of logical method and the stressing of experimental procedure, is perhaps the most salient single difference between the two books. There are, however, plenty of other differences. Brentano, for instance, gives a long programme of psychological applications, a programme that has an extraordinarily modern tone; Wundt says not a word about applications. This difference, again, is characteristic. [illegible] out from mind in use, and his general psychology

[5]Cf. the references to "innere Beobachtung," "[illegible]," [illegible] The discussion of Kent's second objection is, evidently, methodological; but even here the argument [illegible] [illegible] of recourse to an experimental procedure.

is, one might almost say, a theoretical applied psychology; it is, at any rate, a rationalising of general mental application; it was but natural, then, that he should look forward to special fields of application. Moreover, Brentano's general psychology is, for him, a completed psychology; however much remains to be done in detail, the principles are established,—established internationally, for psychology as a whole, now and for ever; and such a psychology is fit and ready for application. Wundt, however, does not share the confidence that is natural to the empirical psychologist; for him, psychology is just beginning; he foresees long years of patient experimentation to be spent on the establishment of principle; and application simply does not occur to him, does not show above his horizon. Or consider, again, the use that the two men make of physiology. Brentano appeals to physiology for purposes of explanation, under the principle of sufficient reason; Wundt is, in 1874, a thoroughgoing parallelist. And so the list might be lengthened. In sum, however, all differences may be traced back to the fundamental ground of divergence: Brentano is an empirical psychologist, Wundt is trying, however blindly, to escape from empiricism, and to inaugurate an experimental psychology. Brentano offers us a logical construction, built of argument and counter-argument; his bricks are the opinions of authorities, which he accepts, rejects, reshapes, in accordance with his design. Wundt seeks to put together a psychology of experimentally determined fact, and is ready to give as much space to the description of an experimental set-up as to the view of some influential empiricist. The resulting structure is ricketty enough, and is patched out all to freely with borrowed or improvised empirical materials; it represents, nevertheless, a new kind of psychology; the design, for all its hesitancies and concessions, is the design of experimental physics and experimental physiology.

I have spoken of this second influence upon our science, the influence of empirical psychology, as if it were unmitigatedly bad. So, abstractly regarded, I take it to have been. But that does not mean, let me add, that empirical psychology in and for itself is unmitigatedly bad. Not only has it shown itself, historically, the necessary forerunner of experimental psychology, but even in the days of experimental psychology it may do good service as an intermediary between science and special application. I am not here concerned with that question,[6] any more

[6]I do not myself believe that empirical psychology is adequate to this mediating function; I believe that we should be on firmer ground in logic and should also get better results in practice if we looked for mediation to a phenomenology of the 'psychophysical' organism, the entire man (cf. my *Thought-Processes*, 1909, 75). But the question is not in point here,—and obviously a footnote is not the place for its discussion.

than I am with the intrinsic merits of particular empiricists,—some of whom have done sheerly first-rate work. I am concerned with the effect of empiricism upon experimentalism; and in the abstract that effect has been wholly bad. Only, we live in the concrete; and in the concrete there are always mitigating circumstances. It was very much to the good, I said just now, that experimental psychology takes its descent from experimental physiology. Yes, but the 'psychology' of the physicist and the physiologist is almost universally an empirical psychology; that is why we see distinguished physicists running to spiritism; and you can hardly imagine a follower of Helmholtz, for example, approaching psychology in any other than empirical terms. It may well be, therefore, that the admixture of empirical psychology, which I have been condemning, was of practical utility—not only in holding Wundt's reports of experiments together in a quasi-systematic way, but also in securing friends for the new movement among the representatives of the other sciences.[7]

Such friendship, even if based in part upon misunderstanding, would help the new science in its struggle with the third environmental influence,—the hostility of philosophy. I have spoken of the Herbartians: but while in the early seventies the Herbartian psychology was dominant in Germany, it cannot be said that the Herbartian philosophy was similarly dominant. Think, for instance, of the more important men who derive from Hegel,—Erdmann at Halle (I am afraid that we do not nowadays read his delightful *Psychologische Briefe* as widely as we should), Kuno Fischer at Heidelberg, Michelet and Zeller at Berlin; and there were other schools and other distinguished and influential men. All these philosophers were, so to say on principle, opposed to experimental psychology, and Wundt's choice of the title *Philosophische Studien* for his laboratory studies reminds one of nothing so much as the small boy with thumb to nose. The philosophers were shocked at the mere idea of subjecting mind to the color-mixer and the chronoscope; and they were shocked the more as the sacrilege threatened one of their vested rights. So the controversies began,—and, really, there is a good deal in them of childish wrangling. Wundt in 1905 thought that the main battle was won, and changed to the title *Psychologische Studien.* Perhaps he was right; though the field of battle has not by any means been cleared up, and local conflicts are still going merrily on. Good time and valuable [illegible] been wasted upon an intrinsically

[7] I need hardly remark that if I had been tracing the history of [illegible] mental psychology [illegible] other than in its formal aspects I should have had a great deal to say of the positive contributions [illegible] [illegible].

foolish quarrel; and there is no countervailing gain save a certain solidarity of front, a certain esprit de corps, that we should have attained in any case, under the other two hostile influences, without the added spur of philosophical antagonism. The struggle with Herbart was incidental; Herbartianism would have died a natural death in due course, and the coming of experimental psychology merely hastened the end. The struggle with empirical psychology was inevitable and has inevitably been long drawn out; for the experimental movement was, in fact, a revolutionary movement,—and, like other revolutionary movements, could grow to power and to clear vision only by way of actual conflict. The struggle with philosophy was on a lower plane than either of these others; it reflects on the one side the over-assurance of youth, and on the other the unwillingness of high priori prejudice to face the trend of events.

I have now set forth, in the oversimplified outline that is all that an address of this sort permits, what I take to be the most important facts regarding the nature and nurture of our young science. Experimental psychology came of excellent stock: a stock that had not only attained prestige in matters of the intellect, but that had also learned to frame ideals and to bear responsibilities; the birth of the science was auspicious. Its bringing up was variously chequered. The hostile influences of which I have spoken have, obviously, been unable to throttle it; otherwise we should not be here today. But indeed, its growth has been, on the whole, steady and assured. There were a few years, in the nineties of the last century, when it seemed to be outgrowing its strength; but that danger passed; and no one can now doubt that the infant of the sixties and seventies has proved itself viable. The question that remains is—What, in the year of grace 1925, is its mental age?

The question is one, I take it, that has attracted us all; and we have answered it differently, according as our mood happened to be depressed or cheerful. It is, of course, a question that we cannot properly answer at all, because—if I may use the figure—we stand somewhere within our own perspective, and therefore cannot judge either how far we have come or how far, to attain the status of the adult, we have yet to go. On the whole, I daresay that we tend to overrate our progress, perhaps on the principle that a space of time variously filled looks long in retrospect. Still, there are signs and symptoms; and we may as well pluck up our courage and try to read them. We have, I think without question, passed from infancy into childhood. Our independence of physiology is a guarantee of that: we no longer feel any necessity of consulting physiology when we lay out our investigations; we do not necessarily borrow physiological apparatus and procedures; it does not occur to us to imitate

physiology in the presentation of results; in a word, we are out of our physiological leading-strings. It is pleasant and reassuring, certainly, if while an enquiry is in course or after it has been brought to completion we can make a cross-correlation with physiology; but we feel ourselves, none the less, to be independent; we do not lean upon physiology. That sign, then, seems unequivocal; and there is another, which a bold spirit might interpret to the effect that we are approaching adolescence —I mean the radical change that has been wrought over the whole field of the science since it turned to phenomenology. We can trace the impulse to this change directly to Hering; and if Hering has had to wait a long time before coming to his own, that is partly because he was overshadowed, for almost the whole of his working life, by the counter-influence of Helmholtz, and partly because he was himself otherwise entangled in a rather crude form of empirical psychology. Phenomenology is not yet, is not of itself, experimental psychology; but it provides today a safe and sure mode of approach to the analysis of our psychological subject-matter; and our recourse to it, our realisation of its promise, may perhaps be taken as a sign of adolescence. If, then, Godfrey Thomson is right, and the intell .ct is most alert and most capable at the age of sixteen, we may congratulate ourselves that experimental psychology is nearing that critical point, and may expect far better things from it in the near future than have been accomplished in the past.[8]

[8]Several friendly critics among my hearers have told me that this concluding paragraph is too optimistic. I am afraid they are right; I can only plead in excuse that the occasion of the address—the dedication of the first independent building in the western hemisphere planned and erected exclusively for psychological teaching and psychological research—naturally disposed one toward optimism.

AN EXAMINATION OF KÜLPE'S EXPERIMENTS ON ABSTRACTION

By R. W. Wilcocks, University of Stellenbosch

In 1904 Külpe published the results of a series of experiments on Abstraction in which the *O*s were given certain observational tasks beforehand with regard to characteristics of an object which was to be shown tachistoscopically, and on which they had to report after the exposition, mostly in answer to questions put them.

Groups of four nonsense syllables were projected on a screen for 1/8 sec. The syllables were in different colours and in different positions in different cases, but at a fixed distance from the fixation-point in the centre. Before each exposition the *O* was given the task of observing the object from a certain point of view, namely, either (1) that of deciding the total number of visible letters, or (2) that of determining the colours with their approximate position in the visual field, or (3) that of determining the figure which the syllables formed with each other, or (4) that of determining as many as possible of the single letters with their approximate position. In some cases, however, (5) "wurde. . .auch noch jedes Objekt 'ohne' Aufgabe einer rein passiven Erwartung dargeboten." Immediately after the exposition the *O* had "*zunächst die Bestimmung im Sinne der Aufgabe zu geben und dann auf meine Fragen nach den anderen Teilinhalten zu antworten.*[1] War keine Aufgabe gestellt, so musste mir die Versuchsperson zuerst über das Auskunft geben, was ihr zunächst besonders aufgefallen war, sie in erster Linie interessiert oder beschäftigt hatte, und dann gleichfalls zu meinen Fragen nach dem übrigen Stellung nehmen."[2]

As a general result from these experiments we have, according to Külpe, "dass die *meisten, richtigsten und bestimmtesten Aussagen da stattfinden, wo die Aussagen mit den Aufgaben zusammenfallen.* Die Abstraction im Sinne des Hervorhebens gewisser Teilinhalte, die *positive* Abstraction, gelingt also am besten, wo vorher eine Präoccupation des Bewusstseins, eine Prädisposition für diese Teilinhalte gegeben oder gesetzt war."[3]

Against Külpe's procedure with regard to the order in which the reports about the different characteristics of the objects were taken, Rubin advances the following. "Wenn Külpe herauszustellen wünscht wie es sich mit Antworten im Sinne der Aufgabe und den anderen Antworten verhält, um von dem, was im Gedächtnis behalten ist, auf das, was tachistoskopisch aufgefasst worden war, zu schliessen, dann muss man sein Versuchsverfahren als so unzweckmässig bezeichnen, dass es ohne

[1]The italics are ours.

[2]O. Külpe, Versuche über Abstraction, *Bericht über den I. Kongress für experimentelle Psychologie*, 1904, 57-58.

[3]*Op. cit.*, 61. The italics are Külpe's.

Beweiskraft ist. Es ist ja bekannt, dass das tachistoskopisch Aufgefasste sehr schnell aus dem Bewusstsein verschwindet. Und dadurch, dass die Bestimmungen, die nicht im Sinne der Aufgabe waren, später zur Protokollierung gelangen, sind sie in unberechenbarer Weise benachteiligt."[4]

Külpe himself was not, it would seem, unaware of this as a possible objection, since he himself states that in many cases "ein rasches Vergessen" may have affected "die Aussagen über die der Aufgabe nicht entsprechenden Teilinhalte" disadvantageously.[5] For the rest, however, Külpe does not take this into account in arriving at the general result already mentioned, except to state on the basis of his protocols "dass in den Abstraktionstatsachen unmittelbare Bewusstseinsphänomene vorliegen" and to suggest "dass gelegentlich Mängel in der sprachlichen Bezeichnung eine Rolle gespielt haben. Aber sie sind durchaus nicht schlechthin für unsere Ergebnisse in Anspruch zu nehmen, so wenig wie man Gedächtnisfehler dafür allein heranziehen darf. Die Versuchspersonen glaubten tatsächlich die Eindrücke in der angegebenen Unbestimmtheit zu *sehen*, bezw. tatsächlich keine Farbe, kein Objekt u.s.w. wahrgenommen zu haben."[6] Against these arguments from introspection which Külpe adduces in favour of his interpretation of his quantitative results, Rubin adds: "solange diese naheliegenden Annahmen nicht explicite widerlegt sind, genügen sie als Erklärung für alles was in diesen Selbstbeobachtungen steht."[7] Ach had, however, before this accepted Külpe's introspective results, reporting that he had obtained similar ones.[8] Mittenzwey had also accepted Külpe's view as to the meaning of his quantitative results;[9] and the same thing holds true of Grünbaum, it seems, for he states that "in der. . . Untersuchung von Külpe ist gezeigt, welche Bedeutung für die Abstraktion die Setzung bestimmter *Aufgaben* hat," although he himself states that Külpe's method was "eine unmittelbare Reproduktionsmethode," and "auf Grund der Reproduktion wird also über die positive Abstraktion (Hervorhebung des Teilinhaltes) geurteilt."[10] Titchener states shortly in a note that "the influence of the *Aufgabe* is. . . plainly apparent in O. Külpe's Versuche über Abstraktion."[11] Koch, in discussing Rubin's criticism of Külpe's work, remarks as against Rubin that "K.s Arbeit hat u.a. gezeigt, welche Bedeutung der Aufgabestellung zukommt, wie sie den Ablauf des Abstraktionsprozesses wesentlich bestimmt; sie diente K. durchaus nicht dazu, 'um von dem, was im Gedächtnis behalten ist, auf das, was tachistoskopisch aufgefasst worden war, zu schliessen,' denn auch das, was im Sinne der Aufgabe lag, wurde ja tachistoskopisch dargeboten. . . Die Frage, inwieweit ein rasches Vergessen bei den Aussagen der Vp. in Betracht kommt, ist von K. nur aufgeworfen, aber nicht untersucht worden."[12] It is clear that this answer does not invalidate Rubin's criticism; rather, that it unwittingly supports it; since, as has already been indicated, Külpe's

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[4]E. Rubin, Bericht über experimentelle Untersuchungen der Abstraktion, *Z. f. Psych.*, 63, 1912, 386.

[5]*Op. cit.*, 66.

[6]*Op. cit.*, 67.

[7]*Op. cit.*, 387.

[8]N. Ach, *Über die Willenstätigkeit und das Denken*, 1905, 239-240.

[9]K. Mittenzwey, Über abstrahierende Apperzeption, *Psychol. Studien*, 2, 1907, 480.

[10]A. A. Grünbaum, Über die Abstraktion der Gleichheit, *Arch. f. d. ges. Psychol.*, 12, 1908, 346, 356.

[11]E. B. Titchener, *Lectures on the Experimental Psychology of the Thought-Processes*, 1909, 249.

[12]A. Koch, *Z. f. angew. Psychol.*, 7, 1913, 337.

tables of results, on which his conclusions as to the quantitative influence of the *Aufgabe* are based, were drawn up without taking the influence of memory, as affected by the order of the questions and reports, into account. It is also clear, as against Koch, that Külpe did base his view, that "eine Präoccupation im Sinne der Aufgabe" was favourable for the process of the abstraction of the characteristics of the object concerned, on the reports the *O*s were able to give after the moment of exposition, *i. e.*, on the basis of more or less immediate memory. Notwithstanding Rubin's criticism and Koch's obviously inadequate reply we find Fröbes adducing Külpe's general result given above without criticism;[13] whereas Lindworksy, in giving an account of Külpe's method, criticises it in other respects but does not mention this point.[14] Pauli gives the experiment in a somewhat modified form, in which, however, as with Külpe, "die besondere Aufgabe . . . dabei stets zuerst beantwortet [wird]."[15]

It is indeed clear that, where an *O* strictly carries out the instructions given him in the case where the observational task refers to a particular characteristic or particular characteristics of the object, a first process of abstraction, corresponding to the given observational task, takes place in direct connection with the tachistoscopic exposition and before the first question is put. In order, then, to answer the questions set him in succession after the exposition, no matter what the observational task, the *O* will have to carry out further acts of abstraction, corresponding to the different questions set, on the basis of the memory-effects of the impression created by the exposition and any abstractional processes which have already taken place. This means that, in answering the questions, he sets to work with particular schematic anticipations for the different characteristics to which the questions refer, with the result that the characteristic or characteristics concerned are brought to consciousness in the first place.[16] It is clear that we may then expect the memory-effects, on the basis of which these later processes of abstraction have to be carried out, to be fresher and so more favourable, other things being equal, for the characteristics corresponding to the observational task, than for those not corresponding to the observational task; since a shorter time, after the exposition, would have elapsed in the case of the former and less intervening disturbance would have taken place. Külpe, incorrectly, did not allow for these advantages. It is clear, then, that a re-examination of the influence of the observational task is neces-

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[13] J. Fröbes, *Lehrbuch der experimentellen Psychologie*, II, 1920, 190-191.

[14] J. Lindworksy, Abderhalden's *Handbuch der biologischen Arbeitsmethoden*, Abt. VI, *Methoden der experimentellen Psychologie*, Teil B, Heft 2, 1922, 168-169.

[15] R. Pauli, *Psychologisches Praktikum*, 3te Auflage, 1923, 180.

[16] Cp. O. Selz, *Zur Psychologie des produktiven Denkens und des Irrtums*, 1922, 109: "Der determinierte Abstraktionsprozess spricht nur diejenigen Abstraktionsgrundlagen an, und er führt nur die abstraktive Hervorhebung solcher Bestandstücke (selbständiger Teile oder unselbständiger Momente) der Abstraktionsgrundlage herbei, die der schematischen Antizipation des Abstraktionszieles entsprechen."

sary. The present paper gives an account of two series of experiments carried out with this aim. They were directed more especially towards the *quantitative* side with regard to the correctness of the reports of the *Os*. The experiments were carried out in the Psychological Laboratory, University of Stellenbosch, South Africa.

Series I.

In the main the method followed agreed closely with that of Külpe with regard to the nature of the objects and the time of exposition.

By means of a projection-apparatus and a rotary tachistoscope the time of exposition was made 1/8 sec., though of course it does not follow from this agreement with Külpe's exposition-time that the duration of the image was also the same.[17] The syllables fell on the radius of a circle of approximately 43 cm. and the *O* was seated at a distance of 4 m. from the screen. The 80 syllables of the 20 objects were all different, and the letters were all different in one and the same object. Five colours (white, red, yellow, green and blue)[18] were used in the present case, whereas Külpe used only four colours. The colours were all different in the case of one and the same object. The syllables were so arranged as to form a number of different figures. In the case of a number of blank experiments objects were used which differed with regard to number of letters and syllables from those already mentioned. Each diapositive was used four times in all with each *O*, the next succeeding exposition of the same diapositive being separated, for the same *O*, by at least 24 hours and 25 observations from the previous occasion on which it was used. In the case of each *O* the expositions were divided into 4 part-series, in each of which each of the objects was shown. In each of these part-series the 5 observational instructions of Külpe were given in an irregular but previously determined order. In the case of the fifth, however, the instruction was given in the following form: "Do not decide before the exposition what particular characteristic of the object to be exposed you will attend to, and attempt not to think beforehand of any particular characteristic." Experimental periods lasted for 3/4 hour or less. The observations were made in a well-darkened room. 320 observations, *i. e.*, 80 with each of the 4 *Os* were made, apart from a number of preliminary trials and 20 blank experiments with each *O* in the series itself.

The *Os* were Mr. H. F. Verwoerd, M. A. (V), a member of the staff of the Department of Psychology of the University of Stellenbosch; Mr. J. C. Conradie, M. A. (C), Mr. J. V. E. Potgieter, B. A. (P), and Mr. A. H. Murray (M), students of the same university. V and M had had previous experience as *Os* with tachistoscopic experiments.

Since, with a view to the aim of the experiments, the following is the essential point in which the procedure followed differed from that of Külpe, it is desirable to state it more fully. Immediately after each exposition the questions corresponding to Külpe's first four observational tasks were put in succession by the *E*, the next succeeding question being put as soon as

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[17] Cp. W. Wundt, Zur Kritik tachistoskopischer Versuche, *Philos. Stud.*, 15, 1899, 295.

[18] The diapositives in our case were made on coloured celluloid sheets, as supplied by E. Zimmermann, Leipzig, by carefully covering the spaces round that intended for the letters with opaque white paint. The strips on which the letters were thus made were pasted over openings in squares of cardboard, the openings being at equal distances from the centre.

possible after the previous one had been answered. The order in which the questions were put after each exposition was determined by the following principles. (1) The order had to make the impression on the O of being a "chance" one. (2) After each of the 20 observations in each part-series, of which the results were to be used, the question corresponding to the particular one set of the first four observational tasks was put first, second, third and fourth an equal number of times. This also held for the case of "no special observational task," thus making our procedure here different from Külpe's in a further, special point. This was done with a view to equalising the conditions of answering in this respect. (3) In so far as the questions put did not correspond to the particular observational task for the exposition concerned, each type of question was also put first, second, third and fourth an equal number of times.[19]

In adducing the results bearing on the points at issue, the first question that may be put is whether there is evidence of a decreasing ability to report correctly with regard to a particular characteristic of the object concerned, within the time needed for answering all the questions, according to the increasing lapse of time and the number of other questions that had first to be answered after the exposition. In other words, does the memory-factor play a negligible rôle?

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[19]In the case of the first part-series the order followed thus became:

No. Obs.	Task	Ques. 1	Ques. 2	Ques. 3	Ques. 4
1	NL	CP	F	NL	LP
2	LP	F	CP	LP	NL
3	CP	CP	F	NL	LP
4	F	CP	F	NL	LP
5	NST	F	CP	LP	NL
6	CP	NL	LP	F	CP
7	F	F	CP	LP	NL
8	NL	NL	LP	F	CP
9	CP	LP	NL	CP	F
10	F	LP	NL	CP	F
11	NST	LP	NL	CP	F
12	CP	F	CP	LP	NL
13	LP	LP	NL	CP	F
14	F	NL	LP	F	CP
15	NL	LP	NL	CP	F
16	NST	NL	LP	F	CP
17	NL	F	CP	LP	NL
18	LP	NL	LP	F	CP
19	NST	CP	F	NL	LP
20	LP	CP	F	NL	LP

NL = number of letters, LP = letters with their positions, CP = colours with their positions, F = figure, and NST = no special (observational) task.

The order of questions for the second part-series was obtained by putting the questions of part-series I in the order 2-1-4-3, for part-series III in the order 3-4-1-2, and for part-series IV in the order 4-3-2-1. The order of the observational tasks and the diapositives remained the same as for part-series I.

TABLE I

Observ. Task	Non-C	CP	Non-C	LP	Non-C	NL	Non-C	F
Question	CP 1	CP 2-4	LP 1	LP 2-4	NL 1	NL 2-4	F 1	F 2-4
Os	%	%	%	%	%	%	%	%
P	43.6	39.6	17.2	22.2	56.2	58.3	50	75
V	68.7	51	36.5	24.3	75	86.8	75	91.7
M	54.7	53.1	26.9	24.6	93.7	50	93.7	83.3
C	59.4	46.9	18.7	11.9	93.7	58.3	86.9	83.3

In Table I the percentages of correct answers are given in the columns. The percentages in the columns marked non-C are those obtained by the various *Os* where the observational tasks were the different non-corresponding ones (including NST). Here the percentages are those for the questions put first. In the other columns observational tasks and questions corresponded, and the percentages are the average for the cases where the questions were put second, third and fourth. In marking the reports one point was given for each colour correctly indicated and one further point where its position was also correctly indicated. The same thing was done in the case of letters and their positions.[20] One mark was also given if the figure was correctly drawn, and one if the number of letters was correctly indicated.

Table I shows that the influence of the memory-factor is of such importance that, if the question corresponding to the observational task is not put first, but put second, third and fourth, then the average percentage for these three places is actually lower, on the whole, than where the same question is put first but with a non-corresponding observational task. In other words, a result the opposite to that of Külpe is obtained as soon as the question corresponding to the observational task is given a similar disadvantageous position to that which he gave to the questions not corresponding to the observational task. It is clear, then, that the objection raised by Rubin against Külpe's procedure is shown by the experimental results to be fully justified as far as the order of the questions is concerned.

On the other hand, a further examination of the results obtained shows that it would be incorrect to conclude that the correspondence of observational task and report does not at all have the favourable type of influence on the ensuing process of abstraction that Külpe claimed for it. This is shown already by Table II.

TABLE II

Observ. task	CP	non-C	LP	non-C	NL	non-C	F	non-C
Question	CP 1-4	CP 1-4	LP 1-4	LP 1-4	NL 1-4	NL 1-4	F 1-4	F 1-4
P	40.6	32.6	24.2	10	44	47	81.2	61.2
V	53.1	52.3	26.4	17.7	87.5	78.1	68.7	81.2
M	53.9	41.4	26.6	18.9	63	77	75	72
C	52.3	51.5	14.3	8.6	63	73.5	81.2	73.5

In the columns [illegible] correct answers are given. The numbers after the designation of the questions, [illegible] that these percentages are the averages for the answers where the questions were put [illegible] third and fourth.

[20] [illegible] p. 331.

It is clear from the Table that in the cases of CP and LP the percentages are without exception higher where the observational tasks corresponded with the questions than where there was non-correspondence, although it is true that the differences are in some cases small. In the case of F we have such a higher score in three out of the four cases, but in the case of NL in no less than three out of the four cases a higher score was obtained where the observational task and question did not correspond. On especially this last, at first sight somewhat surprising result, a remark that Grünbaum makes in discussing Külpe's work has a bearing.

"Nun ist es nicht unwahrscheinlich, dass durch mehrfache, wenn auch zyklisch verteilte Ausfragen über die 'aufgabelosen' Inhalte eine 'Präoccupation des Bewusstseins' auch für sie geschaffen wurde, wenn auch in geringerem Masse, als für die durch die direkte Aufgabe bestimmten Inhalte. Nur bei einem einzelnen Versuch, dem keine vorausgehen und keine folgen, kann die spezielle Aufgabestellung und allgemeines Ausfragen die Prädisposition *ausschliesslich* für einen Teilinhalt bedeuten. Dieser Fall ist aber schwerlich bei einer experimentellen Häufung der Resultate durchzuführen. Die partielle Disposition für die 'aufgabelosen' Inhalte muss von Fall zu Fall sich ändern—dadurch wird ein variabler nicht berechenbarer Faktor eingeführt. . . ."[21] There are, however, other disturbing factors militating against the correspondence between observational task and question showing its influence clearly in the case of NL (and, to a lesser degree, in the case of F), and we shall have to return to this question.

In so far we have at least in part a corroboration of Külpe's main result. There is, however, a further point of difference to which we wish to draw attention immediately. Külpe states that "im allgemeinen kann man überhaupt finden, dass es für die Grösse und Güte der Leistungen nicht vorteilhaft war, ohne Aufgabe dem Objekt gegenüber zu stehen," although he points out that in some cases his *O*s got very good scores in this last case.[22] Since it appears, however, that the reports, for which

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[21] *Op. cit.*, 363-364.

[22] *Op. cit.*, 62. "Die eine von meinen Versuchspersonen hatte ein ausgesprochenes Interesse an der *Figur* und bestimmte diese bei fehlender Aufgabe fast ebensogut, wie bei entsprechender Aufgabe. Eine zweite wurde besonders durch die *Farben* gefesselt und hatte daneben eine starke Tendenz, die *Elemente* zu bestimmen. Bei fehlender Aufgabe leistete sie in diesen Richtungen fast ebensoviel wie bei entsprechender Aufgabe. Die dritte Versuchsperson wurde namentlich von der *Figur* und den *Farben* in Anspruch genommen, ohne jedoch bei fehlender Aufgabe in diesen Richtungen grössere Leistungen aufzuweisen." In view of the already quoted fact, however, that "war keine Aufgabe gestellt, so musste mir die Versuchsperson zuerst über das Auskunft geben, was ihr zunächst besonders aufgefallen war, sie in erster Linie interessiert oder beschäftigt hatte, um dann gleichfalls zu meinen Fragen nach dem übrigen Stellung zu nehmen," it may be accepted that Külpe's *O*s reported on these points for which they obtained high scores "bei fehlender Aufgabe" first. Apart from the fact that there was in their cases obviously a "Prädisposition" in the sense of an unconscious mental set, although perhaps not, literally, a "Präoccupation des

relatively good scores were obtained by Külpe's *O*s, notwithstanding the fact that the observation took place "ohne Aufgabe," were made first in time-order after the exposition, it is clear that they cannot be used—and, we take it, were not intended—as *real* exceptions to the general statement of Külpe's just quoted. Apart from this point, however, Külpe's own tables of results,[23] which we can only refer the reader to here,

TABLE III

Observ. task		NST	CP	LP	NL	F
Question 1-4	*O*s					
CP	P	34.4(3)	40.6(1)	32 (4)	36.7(2)	27.3(5)
	V	57.8(1)	53.1(3)	55.5(2)	49.2(4)	46.9(5)
	M	37.5(5)	53.9(1)	38.3(4)	40.6(3)	49.2(2)
	C	42.2(5)	52.3(2)	68 (1)	49.1(3)	46.9(4)
LP	P	14.2(2)	7.5(4)	24.2(1)	5 (5)	13.3(3)
	V	19 (2½)	17.2(4)	26.4(1)	16.3(5)	19 (2½)
	M	17 (5)	21.6(2)	26.6(1)	18.5(3½)	18.5(3½)
	C	7.2(4½)	7.2(4½)	14.3(1)	8.3(3)	11.6(2)
NL	P	44 (3½)	50 (2)	38 (4)	44 (3½)	56 (1)
	V	81.2(2½)	75 (4½)	81.2(2½)	87.8(1)	75 (4½)
	M	69 (3½)	69 (3½)	88 (1)	63 (5)	75 (2)
	C	75 (2½)	56.2(5)	88 (1)	63 (4)	75 (2½)
F	P	75 (2)	69 (3)	63 (4)	38 (5)	81.2(1)
	V	81.2(2½)	81.2(2½)	81.2(2½)	81.2(2½)	68.7(5)
	M	81.2(1)	69 (4)	63 (5)	75 (2½)	75 (2½)
	C	100 (1)	63 (4)	56.2(5)	75 (3)	81.2(2)

In this Table there are given first the percentages of correct answers obtained by the different *O*s, the percentages being the averages where the questions are set first, second, third and fourth for each particular observational task. Next to the percentages the figures in brackets indicate the order of goodness of the percentages concerned for the particular observational instructions and questions.

do not seem to us to offer clear evidence, as far as the figures actually adduced are concerned, for his statement. At the same time, however, conclusions to be drawn from these figures become uncertain for the further reason that we do not know at what point of time after the exposition Külpe's *O*s gave their reports on characteristics of the object, other than in the case of those reports the scores of which Külpe explicitly singles out as being relatively good notwithstanding absence of the "Auf-

Bewusstseins" for the characteristics favoured by them, it is clear that their answers to these points were favoured by the time at which they were [illegible] which prevents us from using these results of Külpe to come to a comparison as to the relative influence of a special observational task and no special observational task.

[23]*Op. cit.*, 59, 63.

gabe." In addition to this, we are in similar ignorance with regard to the time-order of the reports where there was a special observational task, whether corresponding or non-corresponding.

We have data, however, from our own experiments bearing on the question of the influence of NST on ensuing processes of abstraction. For the first series these are given in Table III.

In order to obtain a comprehensive view of the somewhat irregular distribution of the orders of goodness these orders were added together for NST as observational task, for all cases where observational task and question corresponded, and for CP, LP, NL and F as observational tasks, but with the questions not-corresponding. Each of these six totals was then divided by the number of orders of goodness used in getting the particular total. Since the result of this division will be smaller, the higher the order of goodness is on the whole for the particular case, it is possible in this way to obtain a rough indication of how well the questions were answered in the different cases. The result is given in Table IV.

TABLE IV

Observ. task Question	CP LP NL F correspond.	NST CP LP NL F	CP LP NL F	LP CP NL F	NL CP LP F	F CP LP NL
Average order of goodness	2.19	2.9	3.58	3	3.46	3.08
Total order of goodness	1	2	6	3	5	4

According to the last line of Table IV, then, the observational instruction NST stands second only to the case where observational task and question correspond with regard to favourableness of its influence on the correctness of the answers given. It is clear that this result stands in contradiction with Külpe's statement as to NST being relatively unfavourable.

The irregularity of the distribution of the orders of goodness, as shown in Table IV, we hold to be due to the presence of a number of disturbing factors, which have been indicated already in part in a remark of Grünbaum, quoted previously, with regard to a "preoccupation of consciousness" for the characteristics of the object not specified in the observational task of a particular observation. It should be added, however, that the *Os* were asked to pay particular attention in their introspective reports, made after each single experiment, as to whether they had carried out the instruction of the observational task successfully or not. Only in quite a minority of cases was non-success reported. The disturbing factor would rather have taken the form then, we hold, of a predisposition, an unconscious mental set. In part these disturbing factors have still to be discussed. Series II was indeed largely arranged so as to avoid them. The main result as to the influence of the observational task NST, obtained as above in Series I, is however in full agreement with the result obtained in Series II, and its adduction may be justified because of its confirmatory value.

As shown by Table II, there were certain exceptions to Külpe's main contention with regard to the favourable influence of the positive abstraction of a characteristic where observational task and question corresponded, namely, in the cases of NL and F. That these exceptions should be more than apparent seems very unlikely in view of the law that "the stimulus for which we are predisposed requires less time than a like stimulus, for which we are unprepared, to produce its full conscious effect. Or, in popular terms, the object of attention comes to consciousness more quickly than the objects that we are not attending to,"[24]—at least if we can apply this law, or at any rate extend it, to the dependent as well as the independent parts of a complex object such as was used in our experiments. Such parts would then, especially in view of the comparatively short time of exposition, be able to attain to fuller consciousness than others. It is true, of course, as Grünbaum's already quoted remark shows, that a mental set for other characteristics or parts of the object than the one to which the observational task in a particular case refers is bound to develop in time; but still one would expect the higher degree of preoccupation with the characteristic corresponding to the observational task concerned to show its influence, unless disturbing factors prevent it from doing so.

As a matter of fact indeed the different observational tasks and questions, which we, following Külpe, gave our *O*s, are not equally suitable for the aim of the experiment; and it appears to us that this unsuitability becomes a serious cause of error in the case of NL.

Külpe himself remarks: "Um zu verhüten, dass sich eine bestimmte Vorstellung über den gleichartigen Aufbau der Objekte ausbildete, wurden von Zeit zu Zeit Vexierversuche eingeschoben, bei denen mehr oder

[24]E. B. Titchener, *The Psychology of Feeling and Attention*, 1908, 251. With regard to the application of this law, which is given as one of attention, to the case of abstraction, we have to refer first to Külpe's statement: "Man versteht im allgemeinen unter der Abstraktion den Prozess durch den es gelingt, einzelne Teilinhalte des Bewusstseins hervorzuheben und andere zurücktreten zu lassen. Von diesen letzteren, sagt man, wird abstrahiert, abgesehen, sie werden in Abzug gebracht, sie gelangen nicht zur Geltung im Bewusstsein. Die erfassten Teilinhalte dagegen *werden* abgelöst, abstrahiert, aus ihrer Verbindung, ihrem Zusammengegebensein mit anderen herausgehoben" (*op. cit.*, 56). We have to refer, secondly, to Rubin's remark: "Sein Schüler Grünbaum fügt hinzu (und was er sagt dürfte wohl mit den Anschauungen Külpe's übereinstimmen): 'Solche Absonderung kann . . . an unselbständigen Teilinhalten *eines* Objektes, an *gleichen* Merkmalen mehrerer Objekte, an selbständigen Teilinhalten eines Gesamtinhaltes, [illegible] sind, vorgenommen werden' (Grünbaum, *op. cit.*, 545). Wenn Külpe statt Abstraktion Aufmerksamkeit and statt abstrahiert bemerkt geschrieben hätte, würde er in den obigen Zitaten eine Definition der Aufmerksamkeit gegeben haben, der mancher Psychologe beitreten würde" (*op. cit.*, 305).

weniger Silben bezw. mehr oder weniger Buchstaben in einer Silbe zur Verwendung kamen. Immerhin ist die Aufgabe *Zahl* durch das regelmässige Auftreten von 4 Silben à 3 Buchstaben und durch die über die Möglichkeit einer unmittelbaren Zahlbestimmung hinausgehende Zahl von 12 Elementen etwas beeinträchtigt worden."[25]

From the introspective reports of our *O*s it is clear that, notwithstanding similar blank-experiments to those Külpe introduced, they came to learn that in most cases there were four groups of three letters in each object, and that this knowledge was of assistance in answering the question as to NL. A factor such as this would tend, in an undesirable way, to equalize the scores with regard to corresponding and non-corresponding observational tasks and questions. Chance factors, such as unsuccessful carrying out of the observational task, fluctuations of attention in answering, or differences in the difficulty of the objects, may then, in a limited number of experiments, even bring about a somewhat higher percentage of correct answers in the case of a non-correspondence of observational task and question than in the case of their correspondence. In the second series we therefore omitted NL as observational task and question.

It is furthermore clear that, in the case of two of the observational tasks and questions, the *O*s had to carry out a double process of abstraction, either at the exposition or afterwards, in answering the questions, as far as the *content* is concerned, namely, in the case of *colours with positions* and *letters with positions*. Külpe does not state how he arrived at his points for correct answers with a view to the heterogeneous constituents of these answers. Our procedure, given above, of course means that points for heterogeneous contents were simply added together in arriving at the totals. It seemed desirable to avoid this in carrying out the second series of experiments, and Series II was accordingly so arranged that this could be done.

Finally, the answers given to the preceding questions, after an exposition, certainly influenced the answering of the succeeding ones, and this in a way that one is not able to control. Thus CP and LP take relatively long to answer usually, so that the time between exposition and answering further questions is increased where LP or CP preceded. In the same way, the intervening time is shortened when NL or F precedes. Again, CP having been answered, F can then be answered on the basis of the question already answered, whereas the opposite is not the case, at any rate to the same degree. The further change was therefore made in our second series that only one question was answered after each exposition.

[25] *Op. cit.*, 58.

Series II

The second series was carried out with 12 objects, similar to, but other than those used in Series I. Time and means of exposition were the same as in Series I. The second series was divided into 4 part-series in each of which each of the 12 objects was exposed in succession to each of the 10 *Os*, thus making 480 observations in all.

The *Os* were the 4 who had taken part in the first series and Messrs. P. F. Greyling (Gr), J. G. Schoeman (Sc), T. F. Cronje (Cr), postgraduate students, and J. A. Oberholster (O), J. Mommsen (Mo) and P. Stander (St), graduate students of the University of Stellenbosch.

Preliminary practice was given the new *Os* with objects other than those used for the actual experiments. The observational instructions, one of which was, as in the first series, given before each exposition, were: (1) note the figure, (2) note the colours, (3) note the letters, and (4) the fifth observational task of Series I, namely NST. The questions referred to figure, colours and letters. The order of the observational tasks and the questions was so arranged as to make the impression on the *Os* of being a "chance" one. In each part-series observational task and question corresponded in 6 out of the 12 cases, each type being equally often represented in each part-series. The same observational task was never given twice with the same object, and two expositions of the same object were separated from each other by 12 expositions of other objects. Since there were 4 expositions and 4 reports on each object, the possibility arose, notwithstanding the separation of expositions just mentioned, of previous reports and observations, as carried out under the influence of the particular observational task set, affecting the percentage of correct answers in later cases in an undesirably favourable way, in so far as there was correspondence between the characteristics specially to be noted, according to observational task and question, in the case of the previous exposition(s) and the later expositions of the same object. Thus if, in the first series, the observational task was C (colours) with regard to a particular object and the question also referred to C, a later question C, with the same object but with a non-corresponding observational task, might show a result which benefited from the previous exposure and answer. To meet this, another pair of observations were then made with another object with C as question and a non-corresponding observational task coming in a similarly preceding part-series, and C as observational task and question with the same object coming in the succeeding part-series, and so on. In no other case was the same question repeated with the same object. Table V gives the quantitative results in percentages of correct answers for Series II.

TABLE V

Observ. task	C	NST	F	L	L	NST	F	C	F	NST	C	L
Question	C	C	C	C	L	L	L	L	F	F	F	F
Os												
M	68.7 (3)	81.3 (1)	62.5 (4)	75 (2)	48 (1½)	48 (1½)	39.6 (3)	—	88 (2)	100 (1)	—	74 (3)
Mo	93.7 (1)	68.7 (2)	62.5 (3½)	62.5 (3½)	37.5 (1)	35.4 (2)	31.2 (3)	—	74 (3)	100 (1½)	—	100 (1½)
O	59.4 (3)	68.7 (1)	62.5 (2)	50 (4)	19.7 (1½)	19.7 (1½)	16.7 (3)	—	88 (3)	100 (1½)	—	100 (1½)
St	87.5 (1)	81.3 (2)	62.5 (4)	75 (3)	36.5 (2)	35.4 (3)	37.5 (1)	—	100 (1½)	100 (1½)	—	74 (3)
V	84.4 (3)	75 (4)	100 (1)	87.7 (2)	42.7 (1)	40.7 (2½)	40.7 (2½)	—	100 (2)	100 (2)	—	100 (2)
P	68.7 (2½)	68.7 (2½)	50 (4)	75 (1)	29.2 (1)	20.3 (2)	16.7 (3)	—	100 (1)	74 (1½)	—	74 (1½)
Sc	68.7 (1)	43.8 (3½)	43.8 (3½)	62.5 (2)	14.6 (3)	19.7 (1)	16.7 (2)	—	88 (2)	74 (3)	—	100 (1)
Cr	62.5 (1)	56.3 (2)	50 (3)	25 (4)	19.7 (1)	14.6 (2½)	14.6 (2½)	—	74 (2½)	74 (2½)	—	100 (1)
C	81.3 (2)	93.7 (1)	62.5 (3½)	62.5 (3½)	37.5 (3)	39.6 (2)	40.7 (1)	—	100 (2)	100 (2)	—	100 (2)
Gr	59.4 (1)	56.3 (2)	50 (3½)	50 (3½)	16.7 (1½)	16.7 (1½)	14.6 (3)	—	100 (2)	100 (2)	—	100 (2)

The data given in this Table may be assembled further in different ways so as to bring out their bearing clearly on the points at issue; namely, either by working out the average percentage for the *O*s for each separate question for the two cases when (*a*) question and observational task correspond and (*b*) when they do not correspond; or by making use of the method of orders of goodness already previously applied. We content ourselves with giving the result if the second method be used, though it may be added that the results of using the first mathematical treatment are in principle in agreement with those adduced. No observations corresponding to the blank columns were made.

It is clear from the Tables that there is a decided tendency for the answers to be correct to a higher degree where the observational tasks and questions correspond than where they do

TABLE VI

Observ. task	C	NST	F	L	L	NST	F	F	NST	L
Question	C	C	C	C	L	L	L	F	F	F
Average order of goodness	1.85	2.1	3.2	2.85	1.65	1.95	2.4	2.1	1.95	1.95
Total order of goodness	1	2	4	3	1	2	3	3	1½	1½

not,—if we omit from consideration for the time being the case of F, which gives a somewhat similar result as in the case of Series I. It may be mentioned, however, that the distribution of the orders of goodness shows a certain amount of irregularity here too. This we hold to be largely due to the fact that the factor of a preoccupation or predisposition for the characteristics of the object not indicated in the observational task, or present in spite of it, had not been eliminated in the second series. In the long run these disturbances evidently cancel each other; so that, at any rate for C and L, the favourable influence of the observational task corresponding to the question comes out sufficiently clearly in Table VI.

Coming to the question as to the results reached in the case of F as observational task, namely that in the case of the first series only an uncertainly favourable, and in the case of the second series a definitely unfavourable result is obtained, as far as the figures are concerned, where observational task and question correspond, compared with the results where they do not correspond, we hold that they are to be explained on the supposition, generally stated, that the advantage accruing to answers relating to F, where F is also observational task, becomes relatively small as compared with the influence of other factors that also come into play.

It is clear that the mere fact of a mental set (or of a preoccupation of consciousness) for the characteristics of the object other than those indicated by the observational task actually set will not explain this difference between the cases of F on the one hand and those of C and L on the other, if it is held, as we hold, that this factor is present in all three cases; unless there is reason to accept the view that it plays a larger rôle in the case of F than in the others. It is possible that there was present with most, if not all the *O*s a relatively strong mental set for forms.

Seifert, working on the problem of abstraction by means of tachistoscopic experiments with objects consisting of points and small figures arranged so as to form a "Gestalt," adduces introspective evidence from his *O*s for the view that the "Gestalteinheit" especially draws attention. "In allen diesen Aussagen kommt eine wichtige Tatsache zum Ausdruck: Es scheint, als ob die die Empfindungsgrundlage überbauenden Gestalteinheiten als *Faktoren für die unwillkürliche Aufmerksamkeit* wesentlich in Betracht kommen . . . Es bleibt noch übrig, zu begründen, *warum* von der Gestalt jener Zwang zur Beachtung ausgeht, warum die 'Auffälligkeit' der *Gestalt* grösser ist als die der Fundamente. Das Erklärungsprinzip bildet das . . . Moment des 'Gestaltungsdrangs'. In dem Sinn dieses Begriffs liegt ja das Moment der konstanten *Bereitschaft zur Gestaltbildung*. Man könnte hier mit vollem Recht von einer unbewussten Einstellung reden."[25]

[25]F. Seifert, Zur Psychologie der Abstraktion und Gestaltauffassung, *Z. f. Psych.*, 78, 1917, 107-108.

Clearly, then, a factor such as this could possibly have had a greater influence in the case of F than in the case of C and L (and CL and LP) in equalizing the scores for the cases of correspondence and non-correspondence. There is, however, another factor present which would act systematically in the same direction, and thus make it possible for chance factors such as fluctuations of attention in holding the observational instructions in consciousness, in observing the object and in answering the questions, as well as differences in the difficulty of the figures, to give a result, in a limited number of experiments, which does not agree with the influence the correspondence of observational task and question may be expected to exert.

In coming to the discussion of this further factor it is worth while taking into account the fact that the scores in the case of F as question are relatively high. This is seen at once on examination of Tables I, II, III and V, the results of the first and second series agreeing also on this point. As Külpe's Tables show, the same thing holds true for his results. Merely to give as an explanation for this that the answer to F is easier is of course unsatisfactory, as long as that is only another way of stating that the scores are higher.

The figures occurring in our experiments were relatively simple unitary forms, being dependent abstract characteristics of the complex concrete object. When the *O*s set themselves to answer the questions set after the exposition they have to carry out the second act of abstraction, chronologically considered, that was mentioned above. This act of abstraction is carried out on the basis of the dispositional after-effects of the observation of the object at exposition and possibly of a process of abstraction which has taken place directly in connection with it as result of the observational task. The second act of abstraction is thus a recollective one. Now Selz has found with subject-matter of a totally different kind from that used here, but where the abstraction had also to be carried out on the basis of memory after-effects, that there is an autogenous abstractive preparedness for the more general and especially for the most general characteristics of the subject-matter.[27]

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[27] O. Selz, *Psychologie des produktiven Denkens und des Irrtums*, 1922, 116. "Wir müssen. . .annehmen, dass der *logischen Struktur* der Begriffe aus allgemeineren (abstrakteren) und spezielleren (konkreteren) Merkmalen Eigentümlichkeiten in der *psychologischen* Struktur der Bedeutungskomplexe und der entsprechenden Komplexdispositionen entsprechen, welche die selbständige Reproduzierbarkeit der allgemeinsten Merkmale möglich machen. Die beobachteten Tatsachen führen ferner zu der Annahme, dass in den Bedeutungskomplexen den allgemeineren und spezielleren Charakteristiken *Schichten von verschieden hohem Bereitschaftsgrad* entsprechen, und dass hierbei die allgemeinsten Charakteristika der gemeinten Gegenstände eine Schicht von höchstem Bereitschaftsgrad

Certain errors and omissions made by Grünbaum's *Os* in drawing the figures, after exposition, which had been shown them, offer further evidence in support of the view just stated. So, *e. g.*, details are forgotten, but the figure is correctly drawn schematically; or the *O* can give only a part, which is however often characteristic of the figure; or the position of parts is transposed (Spiegelzeichnung). *Op. cit.*, 391. In all these cases abstract characteristics are more easily abstractively recollected. According to Rubin[28] Moore had observed similar phenomena in his experiments; and, as I understand Rubin, had found that the assimilation of the figure under some more general mental category took place so that this more abstract characteristic was often remembered afterwards, whereas more concrete details could no longer be remembered. The process of recollective abstraction would in such cases benefit from a preceding process of abstraction.

In part this autogenous abstractive preparedness of the more abstract characteristics of an object will be the result, in some cases at least, of previous processes of abstraction which were carried out with regard to the same characteristics to which the second act of recollective abstraction is directed. In part, however, it will be due to the fact that these abstract characteristics are, as abstract, relatively simple, the simplicity increasing with the degree of abstractness; so that, other things being equal, they could more easily be brought to consciousness on the basis of the dispositional after-effects than the fuller and more concrete contents. The figure is such a more simple content in the case of Külpe's and our objects, forming, logically considered, one of the abstract characteristics of the object. For this reason a process of recollective abstraction can be carried out more easily with regard to it, even when no corresponding abstractive process preceded. We have, then, here a further factor tending to equalize the scores where observational task and question correspond and where they do not.

Finally, we return to a point raised in connection with the first series, namely the influence of NST on the answering of the questions. Table VI, as derived from Table V, shows that also in the second series the questions were answered relatively well in this case, a result which, as has been pointed out before, is not in agreement with Külpe's statement.

It is possible that the results which Külpe has tabulated, in so far as some do actually show a poor score with the instruction "ohne Aufgabe," are due in part to a difference from our NST instructions in that somewhat more emphasis is laid on the passivity of the *Os* in the case of Külpe's instructions. Partly, too, in his case some of the reports were given only

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bilden." He points out that this agrees with results of Messer and Bühler and adds (501): "Infolge der hohen autogenen Abstraktionsbereitschaft der allgemeinsten Charakteristika werden diese auch hervortreten, wenn die Determination nicht speziell auf das Hervortreten irgendwelcher Charakteristika gerichtet ist," etc.

[28] *Op. cit.*, 395. T. V. Moore's own article (The Process of Abstraction, *University of California Public. in Psychology*, I, 1910) was unfortunately not at my disposal.

after the reports on points which had specially interested the *Os* had already been made. Speculation with regard to the influence of the order in which the questions were set becomes somewhat uncertain, however, in view of the paucity of information Külpe gives on this point.

On general principles one would expect the *Os* to get better results with the observational instructions NST where they *really attend* to the figure as a whole when exposed and where disturbing factors do not play too great a rôle, than where they attend especially to particular characteristics or constituents and have then to answer questions as to others. In the first case there would be a relatively more even distribution of attention over the different constituents and characteristics, so that the dispositional after-effects, on the basis of which the later process of recollective abstraction would have to take place, would tend to be more favourable for this process than where weaker dispositional after-effects for some had been left, as the result of focussing attention on others.[29]

So Moore[30] found that with the perception of an abstracted element the remembrance and perception of other elements were strongly disadvantageously affected. Seifert found that "zwischen einer abstrahierenden und einer (gleichzeitig aktualisierten) gestaltbildenden Tendenz besteht ein eindeutig bestimmtes Korrelationsverhältnis; es scheint, als ob die beiden Momente sich einander kompensierten. Gelingt die Gestaltauffassung gut, dann wird die Abstraktionsaufgabe beeinträchtigt; wird dagegen die Abstraktionsaufgabe vollkommen gelöst, so bleibt die Bildung der Gestalt unvollkommen."[32] The "Gestaltbildung" is, of course, in our terminology, the abstraction of a particular characteristic of the object Seifert showed his *Os*. Grünbaum, while getting similar results on the whole with regard to the effect of fulfilling the "Hauptaufgabe" on the fulfilling of the "Nebenaufgabe," found in certain cases that "die maximale Anspannung auf die Lösing der Hauptaufgabe . . . auch der Nebenaufgabe zugute kommt," where the chief task was the finding of identical figures in two groups of figures and the subsidiary task that of drawing or recognising the figures which were not identical. These were cases in which there was a large number of figures in the groups, thus rendering the carrying out of the chief task more difficult. He finds thus that "die Anspannung auf die Hauptleistung, die Konzentration des Bewusstseins auf sie, der Nebenleistung zugute kommt" and points out that distractions of attention sometimes lead to an improvement of the process actually to be carried out as task, although it is still a problem how this transfer is really fully to be explained,[32]—a statement which

[29]We have to leave it an open question in how far, in the case of our *Os*, an actual process of determined negative abstraction took place with regard to that which was not given in a special observational task, and in how far it simply attained to a lower degree of consciousness or did not become conscious at all. With regard to this difference between negative abstraction and non-observance see Grünbaum, Negative Abstraktion und Nebenaufgabe, *Arch. f. d. ges. Psychol.*, 38, 1919, 173-174.

[30]According to R. Pintner, *Arch. f. d. ges. Psychol.*, 26, 1913, 55 (Literaturbericht).

[31]*Op. cit.*, 109. Cp also E. Achenbach, Experimentalstudie über Abstraktion und Begriffsbildung, *Arch. f. d. ges. Psychol.*, 35, 1916, 535-6.

[32]*Op. cit.*, 411-413.

still holds true after Westphal's work, in which he sets himself the problem of examining the relations between the carrying out of the chief and subsidiary tasks. Westphal finds, however, that "je höher. . .die Stufe ist, auf der das Resultat der einen Aufgabe gegenwärtig ist, um so weniger ist über die andere zu sagen."[33]

Accepting that, under the influence of the consciousness of a task as being more difficult than ordinarily is the case, a spurt of attention may take place, and that, whatever the further explanation may be, a "transfer," affecting a subsidiary task favourably, such as Grünbaum found, may take place, we should not be in contradiction with our result with regard to the relatively favourable influence of the observational task NST, as long as we hold, as we do, that, on the whole, our *O*s were as attentive, at least approximately, in the case of this task as in the case of other more particularised ones.

Summary

(*a*) As the result of an error in procedure Külpe's tachistoscopic experiments on abstraction are shown not to have proved that the percentage of correct reports is higher in the case where observational task and report correspond than in the case where they do not correspond.

(*b*) If the experiments are carried out with regard to a subject-matter which does not introduce disturbing factors to a sufficiently high degree and in such a way as to eliminate Külpe's error, it is found that Külpe's result, as just stated, is still true in principle.

(*c*) Over against Külpe's statement that observations "ohne Aufgabe" are on the whole disadvantageous for abstraction, it is found that, when the observation is carried out attentively but with "no special observational task set," the results with regard to the correctness of the reports are more favourable, with suitable subject-matter, than where the observational task and question are directed to different constituents or characteristics of the object exposed tachistosocopically; but that the results are then less favourable than where they do correspond.

[33]E. Westphal, Ueber Haupt- und Nebenaufgaben bei Reaktionsversuchen, *Arch. f. d. ges. Psych.* 21, 1911, 405.

THE PSYCHOLOGY OF *GESTALT*[1]

By Harry Helson, Cornell University

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(1) *Introductory*

Among the various schools of psychology there has arisen rather suddenly in Germany a new school with a definite set of assumptions and theories of its own which has come to be known as *Gestalttheorie* or configurationism.[2] The new school may be said to date from the publication in 1912 of Wertheimer's article on seen movement, where the main theses of configurationism first find clear expression.[3] Since then the fundamental concepts of configurationism have been adopted by others, notably by Koffka and Köhler, and a large literature bears witness both to the work which these men and their followers are doing and to the discussion, *pro* and *con*, which the new theories have aroused. It is not surprising that the theories of the configurationists have attracted widespread attention both in Germany and abroad, for the leaders of the new school have applied their concepts to practically every field of interest in psychology: beginning with perception, their investigations have extended into the fields of learning, memory, thought and action in human psychology, in animal psychology, and in the

[1]The material contained in this and the following articles was first presented, in modified form, as a thesis at Harvard University, in partial fulfillment of the requirement for the degree of Ph. D., April, 1924. I am indebted to Professor Titchener for suggestions and criticisms.

[2]As a translation of the word *Gestalt* I have used the word configuration, proposed by Professor E. B. Titchener. Various grammatical forms of the word are often useful, and will be understood from the context.

[3]M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Z. f. Psych.*, 61, 1912, 161-278.

psychology of the abnormal. In fact, the proponents of the configurational theories assert that their concepts apply to physical and biological problems as well as to purely psychological questions, with the result that they have advanced new physical and physiological hypotheses to support their psychological views.

In its beginnings the new psychology has inevitably been destructive; for it has arisen as a protest against the assumptions underlying the methods and hypotheses of older schools. It has brought together from different quarters the criticisms levelled against the traditional psychologies, and it utilizes them in a frontal attack on association, attention, sensation, conditioned reflex, trial and error, and a whole host of concepts which inform current psychological systems. And it has also resurrected many historical concepts which have either fallen into the discard or play but a minor rôle in current psychological theory: the concept of the *Gestaltqualität* finds a new interpretation and becomes the basis of a new theory; act-psychology contributes its share, in the demand that mental life shall be conceived as a dynamic functional process for which functional concepts must be employed in description and explanation; phenomenology prompts an adherence to observable processes and entities and the rejection of results of artificial laboratory methods and products of subsequent reflection, be they unconscious acts, acts of attention, hypothetical sensory elements, or unobservable associative mechanisms.[4] After attacking certain older psychological concepts, the configurationists thus proceed to the erection of the structure which they wish themselves to substitute in place of the old.

In this article I seek to portray the main points of departure in configurational theory and the direction in which they seem to lead. In two following articles I shall present the experimental work upon which the new theories are based, along with the work of members of other schools in similar fields. And in a fourth article I shall attempt a critical estimate of what the school has accomplished and what it seems to offer to the psychology of the future.

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[4]The connection between configurationism and the school of the form-quality is close, as will be seen from this article. Many of the problems concerning the form-quality reappear in the configurational theories. For an excellent summary and discussion of the whole movement, see this JOURNAL, Psychology of Mental Arrangement by M. Bentley, 13, 1902, 268-293. For act-psychology, see A. Messer, Ueber den Begriff des Aktes, *Arch. f. d. ges. Psych.*, [illegible], and E. B. Titchener, Functional Psychology and Psychology of Act, this JOURNAL, 33, 1922, 43-83. For the influence of phenomenology on psychology *cf.* A. Messer, Husserl's Phänomenologie in ihrem Verhältnis zur Psychologie, *Arch. f. d. ges. Psych.*, 22, 1911, 117-129, and *ibid.*, 32, 1914, 52-67.

The configurational revolt is not an isolated, unique phenomenon in the history of modern psychology. The school of the form-quality endeavored to account for perceptual complexes and "objects of higher order" by introducing the notion of a new element added to the usual list; but in the hands of Meinong the theory became increasingly logical, and finally ended in epistemology. The new elements of the Würzburg school were just as much a protest against the Wundtian psychology of sensation, feeling and apperception as they were an attempt to deal with the thought processes; yet nothing has come of this movement. Act-psychology and its first cousins, functional and behavioristic psychology, seek to explain the activities of the organism in long-section, decrying the cross-sectional analyses of the existential and process psychologies. There have, moreover, been other influences at work to foment a new movement in Germany. The most important of these is phenomenology, which has given rise to a new "descriptive" psychology, closely related to the *Geisteswissenschaften,* whose object it is to approximate the facts of everyday life more closely than experimental psychology has done. Here the influence of Dilthey and Husserl has been felt, especially on the side of method. Husserl[5] in 1910 insisted that psychology should not try to ape the physical sciences by copying their methods, since their analyses do not lend themselves to the subject-matter of psychology; and Dilthey[6] as far back as 1894 had stated that psychology should employ concepts peculiar to itself and adequate to its own problems. At the same time there were those in experimental psychology itself who attempted to deal with complexes and relations from new points of view, though so far with little success in achieving a new system. In this group the followers of Meinong stand out as the natural inheritors of the tradition of the form-quality, a fact which makes them close kin of the configurationists. In fact, Benussi, Witasek and Höfler dropped the word "quality" and spoke of "form" (*Gestalt*) before Wertheimer's paper appeared in 1912.[7] Dissatisfaction with traditional physiological theories, built upon atomistic-associa-

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[5]E. Husserl, Philosophie als strenge Wissenschaft, *Logos,* 1, 1910, 309.

[6]W. Dilthey, Ideen über eine beschreibende und zergliedernde Psychologie, *Sitzungsberichte der königl. preuss. Akad. der Wiss. zu Berlin,* 1894, 11.

[7]Meinong had referred to complexes as *Gestalten* as early as his 1894 paper, Beiträge zur Theorie der psychischen Analyse, *Z. f. Psych,* 6, 1893-4, 340ff. *Cf.* also *Untersuchungen zur Gegenstandstheorie und Psychologie* 1904, edited by Meinong; S. Witasek, *Grundlinien der Psychologie,* 1908, and *Psychologie der Raumwahrnehmung des Auges,* 1910; V. Benussi's list of papers is large, but for a summary of his views see Gesetze der inadäquaten Gestaltauffassung, *Arch. f. d. ges. Psych.,* 32, 1914, 396-419; A. Höfler, Gestalt und Beziehung—Gestalt und Anschauung, *Z. f. Psych.,* 60, 1911-12, 161-228.

tionistic postulates, had already been expressed as early as 1898 by von Kries and in 1911 by Becher, who asserted that current theories could not explain the perception of form, relations, magnitude, similarity, general ideas and the thought processes.[8]

The stage was therefore set for the new movement in 1912. Instead of reconstructing or adding to the older theories, as most of their predecessors had done in their criticisms of traditional concepts, the configurationists insist upon starting out afresh, demolishing the old systems, and building anew from the foundation up. It will therefore be necessary to set forth the configurationists' objections to certain traditional assumptions and concepts before I attempt to state their positive programme of reform.

(2) *The Rejection of Analytic Methods*

The method of analysis, which is the chief instrument of scientific psychology, has been the main object of the configurationists' criticisms.[9] Wundt, Watson and leaders of other movements assume, the configurationists assert, that a direct analysis of mental life or behavior is possible. Direct analysis appears to them to be comparable to the actual dissections of the histologist, the partitions of the physicist or chemist. But the configurationists are convinced that the ultimate sensational and behavior units are a product of ratiocination and reflection or of an artificial 'laboratory attitude' toward the facts. Objects, meanings, values and dynamic processes are thus analyzed into meaningless units which have nothing to do with the patterns in which they originally appeared. Very often configurational structures have been excluded from the scientific field of psychological investigation. The configurationists deny that Müller's reproductive tendencies, the acts of the act-school, the chain-reflexes of the behaviorists, or the laws of association can explain how the postulated units form complex structures, how they come to mean. And a further objection against analysis touches the fact that it is impossible to synthetize what has been taken apart, unless additional assumptions are introduced *ad hoc* to explain the original totality: they cite as such assumptions Helmholtz' theory of unconscious judgments to explain contrast in vision, Wundt's principle of creative resultants to account for new emergents in perception, and J. S. Mill's doctrine of mental chemistry to supplement association.

[8] J. von [illegible] erscheinungen, Program der Universität Freiburg i. B., 1898; and E. Becher, *Gehirn und Seele*, 1911.

[9] Cf. M. [illegible] der Gestalt, *Psych. Forsch.*, 1, 1922, 47-58; 4, 1923, 301-350.

The assumption that parts constitute wholes must, they maintain, be rejected, if we are to remain true to the facts of observation. For wholes are the first data given in perception, both phylogenetically and ontogenetically. The ape, Köhler found,[10] reacts more quickly to a situation which he can survey as a whole (structure-function reaction) than to absolute units; Koffka believes that the child learns to recognize and react to the "friendly" face before it has any perception of its colors, etc., as such.[11] The evidence from the field of the psychology of the abnormal has been differently interpreted by Fuchs, Gelb and Goldstein on configurational principles and by Poppelreuter on others.[12] I may quote Fuchs at this point in illustration of the configurational position: if part of a circle is so presented tachistoscopically to a patient suffering from hemianopsia that the incomplete part of the figure falls within the blind portion of the retina, the patient will see a complete circle. The completion is due to the configuration. The elements of analysis are therefore not the real data of experience: elements, sensations, parts are the result of an artificial laboratory attitude which causes the downfall of the originally given structure. When the observer assumes an analytic attitude, the object of observation changes accordingly from a "natural" datum to another which is less natural, more poorly structured. On this point Koffka has taken issue with those psychologists who believe that change in attention or attitude leaves the object unchanged except for differences of clearness. Thus the perception of a field now as figure and again as ground means that a new object is present with new properties, not that the field has merely changed in clearness. So, too, with the staircase illusion: the inverted figure possesses new phenomenal properties, to which a mere change in clearness does not do justice.[13]

The problem of attitude still remains a difficult one for the configurationists, in view of the fact that their own results go to show that "analytic" attitudes may be as natural to some observers as the "synthetic" attitude is to others. Benussi, Seifert, Linke, Kenkel, Gneisse and Wulf have noted

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[10] W. Köhler, Nachweis einfacher Strukturfunktionen beim Schimpansen und beim Haushuhn, Nr. 2, *Abh. d. konigl. preuss. Akad. d. Wiss.*, 1918, Phys.-Math. Klasse.

[11] K. Koffka, *Die Grundlagen der psychischen Entwicklung*, 1921. Trans. by R. M. Ogden, *The Growth of the Mind*, 1924.

[12] W. Fuchs, Untersuchungen über das Sehen der Hemianopiker und Hemiamblyopiker, 1, *Z. f. Psych.*, 84, 1920, 67-169; and 86, 1921, 1-144; A. Gelb and K. Goldstein, Zur Psychologie des optischen Wahrnehmungs- und Erkennungsvorganges, *Z. f. d. ges. Neurol. u. Psychiat.*, 41, 142; W. Poppelreuter, *ibid.*, 83, 1923, 26-152.

[13] K. Koffka, Perception: An Introduction to *Gestalttheorie, Psych. Bull.*, 19, 1922, 531-584.

this fact.[14] To these findings Koffka retorts that the analytic attitude results in the formation of a new configuration, different from the configuration found in the natural state.

The other objections against analysis concern the unity and specificity of structures. If configurational structures possess specific properties which are supra-summative and belong to them as wholes, then these properties are lost in analysis. In other words, supra-summative properties "are not compoundable from similar properties and activities of so-called parts". And, following Ehrenfels,[15] Köhler says: "one finds space-forms which possess more properties than their elementary visual sensations; the same is true of melodies with respect to the tones which compose them, and of intellectual processes with respect to the data in which they originate. For it cannot be assumed that sensations of color and tone, and meanings of single words, are to be considered as 'parts' of space-forms, melodies and higher thought processes; since the exact impression of a visual figure or of the specific character of a musical motif, and the meaning of an intelligible proposition, contain more than a sum of patches of color, tone sensations, and individual word-meanings."[16] Configurations, Wertheimer insists, must be conceived of as governed by internal laws, as opposed to the summative, contingent, spatio-temporal contiguities of meaningless elements. Only in this way can the whole be explained as a rational, meaningful structure.[17]

The arguments so far advanced against analysis are based upon its effect upon the internal structure of the datum analyzed. From another point of view the configurationists find analysis of the direct kind unsatisfactory,—in that it does not satisfy certain demands which we make upon any scientific method: we must be able to control and predict what will happen to our data. It is impossible to describe or derive real wholes from a knowledge of parts. Since the whole possesses its own specific properties, we can never tell in advance from a knowledge of the parts what the whole will be or how it will behave. In this connection Fuchs has some evidence: patients suffering from hemianopsia tend to displace objects as wholes which may develop new centers of clearness varying with the size of the

[14]Benussi, *op. cit.*; F. Seifert, Zur Psychologie der Abstraktion und Gestaltauffassung, *Z. f. Psych.*, 78, 1917, 54-144; Kenkel and F. Wulf in Koffka's *Beiträge*; P. Linke, *Grundfragen der Wahrnehmungslehre*, 1913; K. Gneisse, [illegible] *etc.*, *Arch. f. d. ges. Psych.*, 42,

[15]C. von Ehrenfels, [illegible] 1890, 249ff.; 15, 285ff.

[16][illegible] *[illegible] Gestalten in Ruhe und im stationären Zustand*, 1920, ix. This is a free and adapted translation.

[17][illegible] *ibid.*

object (pseudofovea).[18] An investigation of the retina point by point would never have enabled us to predict these new centers of clearness. In place of direct analysis the configurationists substitute what Koffka has called functional analyses: the method of functional analysis consists in limiting the stimulus instead of changing the phenomenal datum. Variations in the stimulus-situation can thus be controlled and measured. Functional and configurational situations may then be regarded as identical; for whenever one perceives or reacts to a whole differently from what one would to the parts in isolation, there we may say are observable differences which must be accounted for both descriptively and functionally. At the same time the descriptive and functional aspects of experience are subsumed to the same concepts and fit into a single conceptual system.[19]

It is noteworthy that the configurationists discourage analysis even of spatio-temporal wholes. It has long been recognized that the analysis of qualities offers difficulties; but spatio-temporal contiguity has been regarded as a connection which does not change the parts entering into the union. The configurationists believe that space-time may make a difference to units which enter into spatio-temporal wholes. For example, the form of a square is a function of the spatio-temporal relations, since changing these changes the configuration; yet there is something over and above the parts in relation once they have entered into relation. Spatio-temporal configurations are found in the domain of physics, according to Köhler, where two electric fields undergo a change in potential when brought near each other. Here a new configuration seems to appear as a result of the new spatio-temporal arrangement of the single fields.[20]

The decisive factors within any given configuration which are responsible for the new properties of the whole and for the modifications of the parts within the structure are varied and often elusive. Sometimes it seems to be the form of the group which is of especial importance in determining the character of the configurational changes; at other times it may be some single factor within the group which exerts its influence over the rest; or again it may be the internal relations of the parts which determine what the whole shall be. And often the configurationists confess that the determinants of configurational events are due to a variety of factors some of which are unknown. The Graz configurationists have here raised certain fundamental questions regarding the origin and nature of configurations. In the perception of square, for example, the configurationists deny that the relations of the lines to each other are responsible for the "squareness." Are then the new properties of the square as such a result of squareness, or does squareness set into operation some factor which is responsible for the emergents? The

[18] W. Fuchs, *opp. citt.*, but more especially, Eine Pseudofovea bei Hemianopikern, *Psych. Forsch.*, 1, 1921, 157-186.

[19] K. Koffka, Zur Theorie der Erlebniswahrnehmung, *Ann. d. Philos.*, 3, 1922, 375-399.

[20] Köhler, *ibid.*

Graz configurationists have answered such questions by postulating a special higher psychical activity (*Produktionsvorgang*) which stands over and above the sensory contents and orders them. Wertheimer, Koffka and Köhler, on the other hand, while they agree that there is something over and above the parts in relation, deny that special processes or acts are necessary to explain this fact: the new properties of structures, they say, belong to them as totalities, and are functions of the configuration itself.[21] But the Graz configurationists maintain that the configuration is by no means univocally determined, whether as to form or as to structure. Thus four dots arranged in the form of a square may be seen as two horizontal lines, two vertical lines, or two oblique lines at right angles to each other. Benussi therefore holds that the determinant in equivocal cases must be some higher psychical factor which unites sensory contents into patterns.[22]

The configurationists of the stricter school have replied to these and similar arguments. Bringing in new elements or special acts will not, they declare, explain the configurational phenomena. The parts of the configuration must be evolved from changes within the whole. Imageless thoughts, special acts, unconscious or unnoticed acts and sensations are all cast into the limbo of unobservables, all carry with them the assumption that mental life or behavior is composed of elements or units enjoying independent existence and united by summative bonds.[23]

The configurationists also attack the behavioristic and chance theories of learning formulated by Watson and Thorndike and their followers.[24] The main weakness of these theories, according to the configurationists, is that they are based on the assumption that intelligent behavior consists of part-activities which in themselves are meaningless. How these units come to be linked together in meaningful, purposeful behavior is left unexplained. Chance connections of reflexes can never account for correct solutions of problems presented for the first time. Chance theories also leave out of account the importance of the end or goal which gives significance to each act leading to its attainment. Still another difficulty of these theories is that behind them lurks an atomistic physiology: physiological units

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[21]For a full discussion of this point, see Koffka, Zur Grundlegung der Wahrnehmungspsychologie, eine Auseinandersetzung mit V. Benussi, *Z. f. Psych.*, 75, 1915, 11-90; and W. Köhler, *Physische Gestalten*. C. Spearman calls the Graz configurationists the "binarians" as opposed to the [illegible]" B. J. Psych., 15, 1925, 211ff.

[22]V. Benussi, *op. cit.*

[23]M. Wertheimer, *op. cit.*

[24]K. Koffka, *The Growth of the Mind*; W. Köhler, *Intelligenzprüfungen an Menschenaffen*, 1921. [illegible] *The Mentality of Apes*, 1925.

function in isolation from each other and independently of the whole organism; organic activities become summations of part-activities. The configurationists assert that, physiologically, the typical neural, muscular and glandular processes are not composed of single units corresponding to discrete aspects of the stimulus-situation, but rather must be regarded as total processes in which the correlations and correspondences are between total structures. These total structures determine, descriptively and functionally, the specific character of their parts.

The chance-reflex theories substitute muscular and glandular responses knit together by chance, or the pleasure principle, for the real behavior of the organism. Observation of behavior has given place to observation of units which have nothing to do with the original situation calling forth the conduct to be explained. The configurationists urge a return to *Gebaren*-observation instead of description in terms of analytic units.

Nor does the general programme of the behaviorists (if there are any thoroughgoing behaviorists left) meet with the approval of the configurationists, who insist that phenomenal patterns may be of the greatest influence in determining behavior patterns. To understand behavior we must understand the action of the environment upon the organism, and often only an introspective report can give us the information we need concerning the phenomenal side of experience.

(3) *Sensation and the 'Constancy' Hypothesis*

The sensation is closely allied with the current concepts of analysis, functioning in some systems as the last unit of analysis. Many of the facts which the configurationists have explained without resorting to sensation have received attention from members of other schools. But the explanations of the so-called atomistic psychologists do not satisfy the configurationists. Thus G. E. Müller has sought to account for the perception of groups or structures by means of traditional concepts. The perception of complexes, according to this writer, takes place by means of rapid shifts of attention from one member of the group to another. Müller postulates simultaneous and successive acts in which groups are perceived. As a result of his theory Müller comes to the conclusion that it is more difficult to grasp wholes than their parts, especially when one is fatigued, because configurations require collective perception and collective perception requires effort.[25] Koffka, taking issue with this position, asserts that it is more difficult and less "natural" to attend to the parts than to the whole: it is more difficult to perceive an overtone than the total clang, and a note requires more effort of

[25] G. E. Müller, *Komplextheorie und Gestalttheorie*, 1923.

perception than a melody. The more fatigued the observer, the more difficult it is for him to isolate natural parts from their context,[26] though not the artificial parts on which Müller's argument rests.

The sensation as the ultimate unit in psychophysics, perception and thought has given rise to what Köhler calls the 'constancy' hypothesis.[27] This hypothesis in turn is responsible for the use of unobserved processes and entities in order to explain why the constants in our systems deviate from what we have been led to expect of them. The constancy hypothesis implies that we can correlate simple aspects of the stimulus with simple ultimates in experience. When it is impossible to obtain a direct one-to-one correlation between sensation and stimulus, the nervous system furnishes the required units in the form of single excitations aroused by the stimulus and united by associative paths. Atomistic psychology thus goes hand in hand with an atomistic physiology. The elements, both physiological and psychological, count for one in a democracy of individuals, regardless of the context in which they appear. Perception, thought, memory, imagination, and the whole furniture of mind are regarded as summations of these simple units. The whole contains no more than a sum of its parts, the parts remain unaltered in the whole.

The associationists, when a given totality does not behave as they were led to expect from their knowledge of the parts, introduce such concepts as attention, apperception, tied images, and illusory or completion mechanisms to fill the gaps in our knowledge. These processes and mechanisms harnessed into explanation may or may not be observable. When they are not observable, it is plainly impossible either to test or to refute them. The doctrines of unconscious or false judgments, unnoticed sensations, special acts, and imaginal completions all spring from the same source, a desire to preserve the constant value which a constant stimulus is supposed to have for the organism under all conditions. But phenomenal fields may vary while the physical stimuli remain constant. To maintain that unobservable processes are responsible for the variations means, says Köhler, that the whole problem is withdrawn from observation; and then one *ad hoc* explanation is as good as another. The lack of one-to-one correspondence between stimulus and perception or response is due to the fact that other factors may be of equal or greater importance in the total situation than the stimuli; central processes may change the whole situation,

[26] K. Koffka, *Psychologie* in [illegible] Köhler has answered Müller at length in *Psych. Forsch.*, 6, 1925, [illegible].

[27] W. Köhler, Über unbemerkte Empfindungen und Urteilstäuschungen, *Z. f. Psych.*, 66, 1913, 51-66.

subjectively and objectively; an indeterminate *Aufgabe* may lead to almost any kind of report; and "attention" may change not a single sensation alone, but the whole state of the nervous system, with a concomitant change in the phenomenal configuration.[28]

There are no isolated, elementary experiences except under that most artificial analytic *Einstellung* which favors the disintegration of the structured perception. Configurations consist of at least two "parts" (figure-and-ground) entering into reciprocal relationship with each other. Any change occurring in one part of an experience affects the rest. On the behavioristic side, responses are made to total situations; they are "structure-function," "step-wise" reactions. What the configurationists are fighting becomes clear if we compare Köhler's explanation of the learning of apes with that of Jaensch, Schumann, and Lindworsky. Köhler trained a number of these animals to pick their food from the darker of two gray containers. When a third container, darker than that to which the animals had been reacting, was introduced, the animals picked the darker of the last two present, and not the one to which they had been reacting the most frequently, recently and vividly. The perception must, therefore, in order for transposition to take place, have included the two shades of gray within a unitary configuration. The reaction was to the situation "darker than," not to the parts as such. The animals can be taught to make absolute choices, Köhler tells us, but such learning takes longer, and lasts for a much shorter period, than the responses to total situations.

Jaensch,[29] following Schumann, explains the structure-function reactions by means of "transitional sensations". The theory of transitional sensations is based upon the hypothesis that over and above the primary sensory processes are other experiences which accrue to the primary. By means of these transitional states the animal compares the stimuli and can discriminate between them. Jaensch would explain Wertheimer's and Linke's phenomenal movement on similar grounds. Koffka, defending Köhler's theory, denies that transitional sensations can explain the transfer from one situation to another, since no addition of elements can account for sameness of structure. Once the configurational similarity is admitted, elements become superfluous. Furthermore, the transitional sensations are not observed; only the primary "sensations" are in consciousness at the moment the judgment is made. "Wir sehen im Farben-Paar ein Zueinander, eine Struktur, dazu brauchen wir kein Uebergangs-Erlebnis, wohl aber setzt das Uebergangs-Erlebnis die Existenz einer Struktur voraus."[30]

In so far as atomistic theories have tried to account for objects, meanings and values, the configurationists feel that their

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[28]K. Koffka, *Theorie der Erlebniswahrnehmung.*

[29]E. R. Jaensch, *Einige allgemeine Fragen der Psychologie und Biologie des Denkens, erläutert an der Lehre von Vergleich.* Similar views are held by J. Lindworsky, Umrisskizze zu einer theoretischen Psychologie, *Z. f. Psych.*, 89, 1922, 313-357.

[30]K. Koffka, *The Growth of the Mind.*

explanations are inadequate. But they complain that ordinarily these terms find no place in scientific systems. The result has been that special disciplines, phenomenology and *Gegenstandstheorie*, have been founded to include them. With regard to the first point, Köhler complains that neither single elements nor any number of them can account for such meanings as symmetrical, triangular or object forms, whether we postulate simultaneous or successive acts of perception to unite the elements. To bring in tactual and other associative elements does not help, because the problem is only pushed back to other sense modalities. If predicates like outside, inside, round and the like will not apply to elements, they cannot apply to combinations of elements, unless we introduce further assumptions to supplement the older theories. "Aber ganz und gar nicht kann man aus ihr [der Assoziationslehre] ableiten weshalb das Tintenfass als *ein* optisches Gebilde, der Bleistift wieder als eines, das Buch wieder als eines, ein zufällig daliegender und irgendwie geformter schwarzer Stein auch als Einheit gesehen wird, weshalb überhaupt im Gesichtsfeld 'Sehdinge' von bestimmter Grösse, Begrenzung, Ausdehnung vorhanden sind."[31] The problem of perception is for the configurationists a problem of unity and structure, and not a problem of analysis. They wish to account for all the facts of perception by means of the simplest set of assumptions without bringing in supplementary aids for this or that particular anomaly of meaning or process. It is fallacious, Koffka argues, to infer that all experience is composed throughout of elementary processes. The sensation as a hypothetical unit had outlived its usefulness when psychology entered upon the investigation of dynamic phenomena, living processes and objective meanings.[32]

(4) *Associational and Relational Theories Examined*

It has not been a matter of chance in the history of psychological theory that the concepts of analysis, sensation and association have required and supplemented each other in their systematic setting. Once spatial perceptions have been analyzed into sensations, the course of ideas and thinking analyzed into a number of elementary processes, and recognition analyzed into a reappearance of contents conditioned by reproductive tendencies, some principle or mechanism must be postulated to explain how it came about that the original structure was a unity. The configurationists point out that association has been pressed into explanation in all manner of ways: it has been used to explain the facts of perception, recall and recognition; the course of ideas; the products of the imagination, and the actual con-

[31] W. Köhler, Gestaltprobleme und Anfänge einer Gestalttheorie, *Jahresber. ü. d. ges. Physiol.*, 1922, 516.

[32] Koffka, *ibid.*

stitution of mental processes. Meaning in perception is explained by means of associative images. When it is a matter of explaining the actual constitution of mental processes, the doctrine of association assumes the form of J. S. Mill's doctrine of mental chemistry, according to which association is not only a cohesive principle but possesses the power to transform or to fuse mental elements. Physiologically, the associational theories have been used to explain psychological facts: for example, in Exner's facilitation theory, residual traces of former excitations in the brain prepare the way for future excitations,[33] thus accounting for memory and perceptual complexes. Von Kries and Becher[34] criticize theories of this type as follows: the theories cannot explain the first association between two or more elements; simple summation does not explain complexity; it is inconceivable on the basis of such theories that a number of excitations could influence one another in assimilative and contrast effects in vision. The configurationists make the most of these arguments and add others: granted, says Koffka, that there are millions of nerve cells and hundreds of millions of connections between them, it is difficult to believe that every perception is conditioned by one set of elements, or that there is time enough for such connections to form in every required situation, *e.g.*, the first time that a baby fixates an object. Köhler points out that there are too many irregularities in the optic sector to result in the perception of homogeneous space if a point-to-point correlation exists between phenomenal and physical spatial structures.[35] And a final objection is that the associational theories suffer from over-simplification: complex processes, meaningful connections, and causally related events do not arise from chance connections in time and space.

If the laws of association reduce to mere existential connection between two or more elements, there is no real reason why these elements should go together or condition one another. In order to account for meaningful connections among the parts of a structure, Koffka has restated the laws of association in the following form: "If A, B, C. . . .once, or several times, have been present in experience as members of a configuration, and if one of them appears bearing its membership character, then the tendency is present for the whole structure to be completed, more or less fully and vividly." The law of similarity, which many associationists reject, would then read: "An idea A can call forth another idea A′ without being bound to it by associa-

[33]Cf W. Wundt, *Physiologische Psychologie*, 3, 1911, 538ff; and S. Exner, Zur Kenntnis von der Wechselwirkung der Erregungen im Zentralnervensystem, *Pflüger's Arch.*, 28, 1882, 487.

[34]*Opp. citt.*

[35]Köhler, *ibid.*

tion if A is sufficiently like A′." Similarity does not fit into associational theories because it represents a real, inner connection between elements which does not reduce to mere contiguity. Configurational theories encounter no difficulties with similarity, even on the side of brain process, because configurational principles apply to structures wherever found. The most general law of the reappearance of structures would then be: "A structure once present in experience creates favorable conditions for its own reappearance or that of a similar structure." In opposition to the associationists, who ask "where" an event occurred, since they are concerned with the elements which are at the bottom of the event, the configurationists ask "what" happens, since it suffices for them to know the specific form of an event in order to predict its behavior.[36]

Theories based upon elements are often relational theories as well. Sometimes it is said that complexes are composed of parts in relation, or that the course of ideas is determined by the "relational consciousness." It has been asserted that the apes react to the relations between the elements, and that these relations remain constant even though the relata change. The configurationists reply that the relational theories do not fit the facts for several reasons. (1) The relations between the containers do not appear to the animal who sees and reacts only to the particular shades or colors involved.[37] (2) The relation-seeing attitude cannot be equated to the "structure-seeing" attitude, because these are two entirely different perceptions and involve wholly different phenomenal patterns; the perception of a structure is prior to and independent of relational judgments. Thus to say that one perceives a square and to say that one perceives that "side *a* equals *b*" are two entirely different judgments involving different objects of perception. (3) As a matter of fact, Köhler denies that, in the example quoted above, the relations are the same; for the gray which stood in the relation of "darker than" in the first situation stands in the relation of "lighter than" its companion in the second situation. But even if it is granted that the relations in transposable configurations are constant, Köhler remarks that this is a rule of configurations but not an explanation. And against Müller, who would explain configurational phenomena by means of relations, collective perceptions and association, Köhler[38] argues that the problem begins where the relational theories stop. Finally, (4) a general criticism of the relational theories is that

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[37] K. Koffka, *Die Grundlagen der psychischen Entwicklung*, 1921, 176.

[38] Komplextheorie und Gestalttheorie, *Psych. Forsch.* 6, 1925, 382; and *The Problem of Form in Perception*, 7th Intern. Congress at Oxford, 1923.

they still operate with elements which are somehow external to the relations and given independently of them.[39] The configurationists stress the fact that significant relations are internal, in the philosophical sense, and make a difference to the terms related. A description of parts in relation does not reproduce the configuration with its specific properties as a whole.[40]

(5) *Attention Does Not Explain Configurational Changes*

The concept of attention plays a dominant rôle in many systems of psychology. The configurationists single out those cases in which attention has been used to bolster the 'constancy' hypothesis and association. To get some notion of the many uses to which the word 'attention' has been put, one need only turn the pages of the psychological journals, where it will be found in every imaginable sense: as a function, an act, a condition, a state, an elementary constituent of perception or a separate act in addition to the others, as clearness, vividness and so on *ad libitum*. Koffka links it up with the constancy hypothesis to illustrate one of the worst uses to which it has been put from the configurational point of view. When an object, sensation or association surprises us by its appearance, or by some change which we have not expected, attention serves as an explanation. Attention may change the intensity of the experience,

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[39]The distinction between the form of combination and the formula of combination was clearly drawn by Stout, who points out that every perceived whole involves (1) component parts, and (2) the form of combination of these parts. The problem of the form (-quality) does not concern the relation of combination to the elements combined in the actual constitution of an objective whole, but rather the relation of the apprehension of form to that of matter. "Even though a whole is nothing more than its parts taken collectively, it does not follow that our cognizance of this whole is to be identified with our cognizance of all its parts. It is not a question of mental combination (or chemistry). If apprehension of form is distinct from apprehension of matter, it is not a form of mental combination, but a material constituent of consciousness, comparable to the perception of red or blue." Apprehending the form and formula of combination are two entirely distinct operations. *Cf. Analytic Psychology*, 2, 1913, ch. 3. Husserl as far back as 1891, in *Philosophie der Arithmetik*, pointed out that the perception of the "figural factor" is not to be identified with the perception of the parts in relation.

[40]Becher, in his review of Köhler's *Physische Gestalten*, points out that we must distinguish between systems which are universally causally coherent, in which changing one part influences the rest, and systems which are not. *Cf.* W. Köhler's physikalische Theorie der physiologischen Vorgänge, die der Gestaltwahrnehmung zugrunde liegen, *Z. f. Psych.*, 87, 1921, 1-44. In other words, not all complexes are governed by internal relations. This fact was noted by Lipps, who distinguishes between relations which change the relata, *Verhältnisse*, and those which do not, *Beziehungen*. The former include relations of identity, difference, equality, inequality, similarity and dissimilarity (*cf.* Meinong's ideal relations). Mere spatial juxtaposition is all that results from the external or summative relations. See T. Lipps, *Leitfaden der Psychologie*, 1906, 133.

it may determine the train of associations, or it may leave certain processes wholly out of the field of consciousness. The thought processes and attention may influence each other through association, since attention and association determine the contents of perception and the course of ideas. This factor becomes so variable and capricious that Koffka finds it is impossible to predict what attention will do in a single case; yet it has served as a prop to the atomistic-associationistic theories when others have failed.[41] Let us examine the objections of the configurationists to the use of the word attention as act, determinant and clearness.

One of the typical act-theories which the configurationists reject may be found in Müller's latest exposition of his principles in a volume entitled *Komplextheorie und Gestalttheorie.* Since Köhler has answered Müller at some length we can get a fair notion of the configurationists' position with respect to this very important point by considering briefly the theories of these two psychologists.

Müller begins with elements as his fundamental data, and shows how they are united by collective simultaneous or successive acts of attention. Various factors aid or hinder the perception of wholes. The facts which the configurationists attribute to the properties of configurations as wholes are dealt with by Müller as cases of collective simultaneous attention. Köhler declares that Müller's use of the concept is ambiguous,[42] to say nothing of the contradictions it involves. Again we find a difference of theory leading to different statements of fact: the best configurations do not receive the maximal amount of attention, as Müller's theory would lead us to expect, while the converse is equally true: configurations may receive a maximum of attention and yet remain amorphous, ill-structured aggregates. The acts of attention by which unit elements are combined into wholes are not observable, Köhler points out, nor do they serve any theoretical purpose. In fact the assumption of acts needlessly complicates the explanation of complex perceptions, since, according to Müller's theory, it would be necessary in order to perceive the word "bat" to see the "b" first, then the "a" and finally the "t," after which an act of second order must supervene upon the first three to combine them into a single whole. With words of twelve or more letters we should expect as many more acts. The theory, Köhler concludes, is not justified by observation and is, indeed, quite fantastic.

Another use of the word attention which finds disfavor with Koffka is that of a determinant or conditioning factor, *e.g.*, a determinant of limens. This use implies, first, that there are sensations or other processes which may go unnoticed by attention. From the constancy hypothesis it follows that certain sensations must be present with a given stimulus; and if the observer does not report them, then his attention has been on [illegible]. These unnoticed sensations may, however, play some part in determining what is present in [illegible]

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[41] K. Koffka, [illegible]

[42] W. Köhler, *Psych. Forsch.*, 6, 1925, 358 ff.

Thus an observer may report that two grays, *a* and *b*, are equal in brightness, and that *b* and *c* are equal, but that *a* and *c* are not equal. It is assumed that the differences between *a* and *b* and between *b* and *c* must have been present throughout, but that they were too weak to be noticed or attended to. The configurational explanation would dispense with attention and unnoticed sensations and would proceed in this manner: phenomenally, the first and second pair are equal because there is not enough difference to form a "rise" or "fall" from the level on which they appear. But the third pair, consisting of *a* and *c*, forms a new configuration, in which a "step" up or down in brightness can be perceived. The configuration into which the two elements enter determines what is seen. The configurational theories thus need not employ judgments which have been falsely directed, unconscious acts or unnoticed sensations, or elements which have been changed by the dubious activity of attention.[43]

Nor does attention as a purely descriptive term satisfy the configurationists; for this use too supposes that objects may remain constant while their clearness value changes. The changes in objects often ascribed to changes in attributive clearness involve the whole configuration to such an extent that we must regard it as a new object with new properties. Rubin, reviving a distinction made by Helmholtz, Hering, Mach, Wirth and others, shows that when we look at a painting, at objects in a room, or at a scene, certain parts stand out in clear, well-defined pattern while the rest is more or less hazy and undifferentiated and serves as a background for the pattern; one field is figure and the other is ground. The usual explanation of the change from figure to ground (for a field which has been figure may be made into ground) as due to a change in clearness does not do justice to the change; for when figure changes to ground it is transformed into a new phenomenal object possessing new properties.[44] Somewhat analogous results are reported by Fuchs: if a circle is exposed tachistoscopically with a small black dot near its periphery, only a circle on a homogeneous ground will be seen; but if the dot is moved toward the center, where it forms an integral part of the circle structure (Wertheimer's *Schwerpunkt*), it too will be seen. Here the new configuration embodies the dot as an essential part of its structure, and the dot must appear if the configuration is to be seen at all. It is not a matter of clearness; for clearness takes it for granted that the black dot was present in the structure before it was seen, whereas the

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[43]K. Koffka, Psychologie der Wahrnehmung, *Die Geisteswissenschaften*, Part 29, 712-716 and 796ff.

[44]E. Rubin, *Visuell wahrgenommene Figuren*, Part 1, 1921. A more exact description will be given in our second article.

structure was not at all present until the dot was seen: in other words, circle and circle-with-black-dot-in-center are two entirely different configurations and must not be taken for the same object of perception. Configurational changes will therefore determine what is to predominate in perception.[45]

Clearness loses its place as a primary concept with the configurationists. It is no longer responsible for *Eindringlichkeit, Aufdringlichkeit* and vividness. What will be clear or attended to depends upon configuration or structure, and is explicable from such factors as position within a structure, whether a field is figure or ground, and how the configuration conforms to laws of simplicity, pregnancy or precision (*Prägnanz*), symmetry and the like. Descriptions formulated in these terms show how structures behave, whereas such terms as attention, association, sensation and judgment divorce the content from its form and context and imply that only external modifications have taken place in perception. What is to be clear will therefore follow configurational laws, not the capricious activity of some inner variable.

The question now arises concerning the structure which will be preferred in perception when a number of configurations are possible. We are told that a configuration may be more impressive or may be preferred in perception, imagination or memory for any one of a number of reasons: either it is a "better" configuration, "more natural," or, having appeared a number of times before, it has left a "configurational disposition" making its reappearance on subsequent occasions easier. Attitude, set, habit (*gesamter psychischer Habitus*) all tend to facilitate or inhibit certain configurations.[46]

(6) *Relation of Configurationism to Other Schools*

Questions and problems which have been the bugbear of other schools of psychology receive short shrift from the configurationists, who act upon the maxim given by Goethe to a friend: "The greatest art in theoretical and practical life consists in changing the *problem* into a *postulate*." The problem of complexes and relations is turned by the configurationists into a postulate, and the riddle is solved. Asserting that the whole determines the nature of its parts, the configurationists are certain that if we describe and explain the total configuration its parts will take care of themselves. The fundamental data for them are therefore objects, patterns, forms, as these are given in immediate experience. In place of the sensation or the mental element as the primary unit behind which [illegible]

[45] W. Fuchs, *op. cit.*

[46] Wertheimer [illegible] the peculiarities of configurations. *Cf.* his articles in *Psych. Forsch.*, 1922 and 1923.

the configurationists substitute the total structure or meaning of any given mental process or response. If any analysis is to be made, it must be in terms of conditions which affect the total situation without destroying it. All this means that we cannot separate stimulus from perception, quality from form, or a part-response from the total situation in which it is found.

The fundamental configurational concepts can be traced to several different sources. Ehrenfels laid down the two fundamental criteria of configurational structures in a paper published in 1890 on the form-quality (*Gestaltqualität*): a configuration possesses properties over and above its parts and not derivable from them; the configuration is transposable since it does not depend upon any given set of elements. Any change occurring in a definite direction, according to Ehrenfels, is a form-quality, *e.g.*, reddening, blushing, cooling, growing blue, or a rise in scale. Relations, too, are form-qualities, but in a slightly different sense, for they require the activity of a subject. But Ehrenfels' actual definition of the form-quality does not find favor with Wertheimer, because it savors too much of the atomistic presuppositions of the associationistic psychology: the form-quality is a definite ideational content appearing in consciousness and composed of separable elements. Various difficulties with the concept arose, especially with regard to the relations of the contents, and Meinong elaborated the theory in an endeavor to bring relations and contents together. To explain the emergence of the form-quality within a given complex, Meinong introduced a number of terms, chief of which was that of a special activity acting upon sensory elements to produce the unitary, structured whole.[47] Meinong's theories have figured prominently in the writings of Witasek, Höfler, Benussi and other members of the Graz school, who may perhaps be designated as the "left wing" of the configurational movement.

The disagreements between the older and newer theories have been amply set forth by Koffka, who assails Benussi for introducing the production-process (*Produktionsvorgang*) to explain how sensory elements are united into complex wholes. The followers of Meinong retain the sensation as the constant unit in perception, in immediate one-to-one correlation with the stimuli, while the form, pattern and structure arise from a psychical activity central in origin. Koffka and Köhler reject

[47]The controversy over the status of the form-quality and of complexes and relations in general involved Meinong, Witasek, Höfler, Benussi, Cornelius, Schumann, Gelb and a number of others. The problems reappear in the present discussions of the configuration, but in the modified form that we should expect after the twenty years of psychological research which have intervened since the controversy was at its height. For a review of the discussion and bibliography, see Bentley, *op. cit.*, Note 4.

this division of conscious contents into two classes, and insist that configurations have objective validity, inherent in physical systems. The new configurationists, far from making the configuration subjective, have constructed an elaborate physical and physiological theory (which has undergone several modifications) to prove that phenomenal configurations may copy, or may spring directly from, physical configurations, without the intervention of any concepts peculiar to psychology or vitalistic in character.[48]

Another point of difference between the new configurationists and the left wing concerns the origin of configurations. The configurationists are by no means agreed among themselves on the finer points of their theory here, but there is enough agreement to allow us to speak in general terms for all. Meinong, Witasek and Benussi imply that the elements are prior to the form or pattern in which they appear. The opposite view is held by the new configurationists. Koffka believes that the configuration precedes the parts, in the history of both the individual and the race. Wholes are no less real than their parts. Nor can parts be isolated from wholes at random, says Köhler, and still remain true parts. "Von diesen [Gestalten] aber können nicht beliebig herauszuschneidende geometrische Stücke als Gestaltteile gelten."[49] The natural parts of a configuration seem to be those portions of the configuration which can be recognized as belonging to a given structure even though they are isolated from the whole: thus the number 8 has only two natural parts, the upper and lower half, because these come the nearest to suggesting the original.

So far the discussion has hinged mainly upon perception; and this restriction is not out of keeping with the stress laid upon perception by the configurationists. Their theories originate in perception, and find most fruitful application in that field. But the configurationists feel confident that their concepts are applicable to every field of psychological investigation; and this belief brings them into connection with several other important schools of modern psychology. Although the configurationists regard the fundamental assumptions of the imageless-thought movement as atomistic, they agree with Watt, Bühler and Messer that meanings, goals, and any factors which give direction to mental life or behavior must become part of experimental psychology. The determinants of mental life are not, however, new elements (Bühler's *Bestandstücke*), impalpable contents or unobservable tendencies; rather, to quote Wertheimer, "we conceive of an inner necessity, of mean-

[48]The publications in which Koffka, Benussi and Köhler express their views on this important topic have been cited in Notes 7, 16, and 21.
[49]W. Köhler, *op. cit.*

ingful predictions and completions wherein events do not occur through blind, outer factors, but through inner demands. Thus (*contra* Hume) we can predict the locus of a curve through inner necessity, an actual inner motive which we do not regard as a purely psychologically fortuitous set of events."[50]

Another aspect of configurational theory borders closely upon act-psychology. We cannot separate perception from memory, thought and action.[51] Perception leads over into action and is a necessary condition of action. Phenomenal patterns determine reaction patterns. Afferent, central and efferent portions of the nervous system must be regarded as interdependent parts of a single, causally-coherent system, in which every part influences and is influenced by every other. That the phenomenal pattern may determine reaction patterns was shown by Köhler in this way: a rope wound around a beam presented insuperable difficulties to the apes; what appears to us as a clear, articulate structure evidently seems to these animals like a tangled, intricate structure. The apes react to the rope around the beam as we should react to a knotted skein of thread,—tugging, pulling and scratching, but never unwinding. The animals' reactions are accordingly as meaningless and poor as ours are in the face of a situation which is beyond our powers of discrimination and comprehension.

The configurationists do not regard the formal properties of experience as the correlates of special acts. Contents are in constant flow and point beyond themselves to other contents and to functional situations. The organism is not a passive receptacle for adventitious stimuli; rather, it selects, reacts to, and molds the situations which confront it. The changes in phenomenal and behavior patterns represent dynamic processes in time, which cannot be resolved into static units of an elementary character.

The configurational method and descriptions are largely phenomenological. Some of the objections to the traditional concepts raised by the configurationists come directly from Husserl, or can be traced to a thoroughgoing application of his principles to psychological problems. Their emphasis upon descriptions in terms of observable data, their inclusion of meanings in psychology, and their insistence upon preserving the essence of any given experience, all alike owe their origin to phenomenology.

Phenomenologically, experience presents us with self-contained wholes whose meanings are resident within the immediate data themselves and cannot be found in accruing or correlated

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[50] M. Wertheimer, *Psych. Forsch.*, 1, 1921, 57.

[51] *Cf.* F. Wulf, Über die Veränderung von Vorstellungen (Gedächtnis und Gestalt), *Psych. Forsch.*, 1, 1922, 333-373.

processes. The phenomenological method is positivistic and empirical. Deductive theorizing, logical constructs, abstraction and inference must be brought to the bar of immediate experience; thus association, the concept of the mental element, the mechanisms and processes used as explanations of the facts of perception, memory or thought are submitted to factual tests. We can therefore understand the configurationists when they assert that form, pattern, figure-and-ground, good, bad, hard, soft, precise, and articulate describe the facts of configurational structures and must be included in psychology. The perception of objects and complexes yields very readily to this kind of description. Configurational descriptions usually assume the form of statements of differences in *kind*, rather than in *degree*, since quantitative units imply that meanings and wholes can be built out of fundamentally similar units by purely additive operations. The configurationists hope to unify the facts of psychology by reducing diverse configurational phenomena to similar sets of conditions to which they owe their origin and development.

Emphasis upon total situations implies relativism. The configurationists are the *echte* psychological relativists.[52] Their relativism harmonizes with their rejection of constant elements and of the importation of subsidiary factors to help explain what is actually observed. It means that we cannot tell from the parts alone what the whole will be; every situation is a function of a total set of conditions in which it is enmeshed. Once these have been discovered, the whole appears to be internally governed or autonomous. Indeed, within a given whole many opportunities may be present for modifying the structure: position, quality, and a number of other variables may change a configuration or its parts.[53]

At this point the question arises: "What innovations do the configurationists actually propose, and what are the systematic implications of the new psychology?" Perhaps the most radical proposal of the configurationists is that objects, meanings, affective, evaluative and aesthetic categories must be included within experimental psychology. If we begin with the assumption that objects and forms are immediately given in experience, it follows that affective and evaluative experiences preceded analytical and discriminatory responses and hence must be re-

[52]G. Humphrey, The Theory of Einstein and the Gestaltpsychologie; A. Parallel, this JOURNAL, 1924, 353-359. I do not subscribe to all that is contained in this paper. In fact, according to the exposition, it will be [illegible] concerned only with the psychological aspects of configurational relativity.

[53]W. Fuchs, Experimentelle Untersuchungen über die Änderung von Farben unter dem Einfluss von Gestalten (Angleichungserscheinungen), *Z. f. Psych.*, 92, 1923, 249-326.

garded as no less fundamental and primary than red, sour and extensity. Certain objects excite fear in and for themselves. To analyze a fear-response into anything else would be to destroy what alone has meaning in the fear-situation, both perceptively and behavioristically. So also the meanings sharp, round, square cannot be analyzed into elements which are none of these. The psychology of perception and behavior must be re-written on the new basis.

But the phenomenological point of view does not drive the configurationists into a purely descriptive psychology; the concept of configuration is too all-inclusive to allow of that. Perceptual and thought patterns are involved in a net-work of physical and physiological conditions which must always be taken into account. The configurationists hope to bridge the gap between psychology, biology, and physics. This extension of the concept of configuration results in a variety of meanings of the word. Hence we must try to discover a little more exactly just what the *Gestalt* stands for, and how we are to identify configurational phenomena in experience. This must be done before we can answer the question raised above.

(7) *Various Applications of the Concept of Gestalt*

The word *Gestalt* has been used in a variety of ways by members of the new movement. Usually there is agreement in intension but the concept is variously applied in extension. On the phenomenological side we find several different meanings along the following order. The configuration may refer to any given totality described in terms of its meaning. Or the configuration may mean the form of a group, *e.g.*, square, triangle or circle. The essential designation may be the manner of combination of a group of units into a whole: four dots may be perceived as a square, as lines in various positions, or as a tilted cross (Benussi's *Gestaltmehrdeutigkeit*). Often the configuration may include factors at present unknown. Physiologically a configuration consists of the afferent, central and efferent portions of the nervous system functioning as a unit: there are no isolated paths, no single point-excitations connected by strands; the form of the excitation and its spread are here of importance. From the point of view of behavior a configurational response may be defined as a reaction pattern in which each act attains meaning and is what it is by virtue of its part in the total situation confronting the organism. The actual definition given by Köhler states the configurational point of view very clearly in a few words: "An objective consideration of the behavior of the apes must show more and more clearly that if the single acts are considered in isolation they appear to possess no meaning so far as the problem to be solved is concerned; but if they are taken in connection with the total process they are related parts of a (specific) whole and are therefore intelligent."[64]

The general definition of configurations given above is sufficiently wide to embrace several universes of discourse. There are physical, physiological, biological and psychological structures possessing properties not compoundable from their parts and hence to be regarded as configurations (Köhler). The configurational principles are not peculiar to psychology; Köhler feels that he asserts nothing in principle which would

[64] W. Köhler, *Intelligenzprüfungen*, 72.

not receive the assent of physicists and biologists.[55] It should be possible, and indeed it is necessary, he thinks, that psychology employ concepts which shall embrace physical, biological and psychological facts within a single system. With concepts common to all, the chasm between psychology and the other sciences can be bridged. First of all psychology and biology find common ground in the fact that "the striking harmony of the structure, function and reactions of the living organism illustrates the unity and completeness which differentiate organic from inorganic life." This granted, another step may be taken which will carry us over to physics. If there are physical configurations possessing properties not compoundable from their parts, it may be possible to regard central physiological processes in the nervous system as special cases of physico-chemical configurations. Physics and psychology will then employ common assumptions; and the transition can be easily made, *mutatis mutandis,* from body to mind, from stimulus to perception, and from environmental situations to phenomenal processes and reaction patterns.[56]

This programme is indeed far-reaching. The concept of the configuration becomes almost universal in application. It is to be expected that what it gains in universality it loses in specificity and uniqueness; hence the variety of meanings of the term *Gestalt.* We cannot be sure in any given case whether the facts referred to are psychological, biological, physical or logical. There are therefore at least four different domains in which configurational phenomena may be found, each one with its own specific characteristics. Whether the properties of a physical or logical structure can be carried over bodily to psychological configurations cannot be determined in advance; and unless every case is examined on its own merits, it is impossible to predict where the different disciplines will or will not meet on common ground even with reference to configurational structures. Let us examine the concept of the configuration in physics, physiology, biology and logic a little more closely.

The physicist, thinks Köhler, does not investigate summative groupings of matter or electrical charges; they offer him no problem. He is interested rather in studying the properties of material systems in which each part is influenced by every other. Although summative groupings of matter may be found in nature, a true physical system is more than a sum of parts. The classical example of a non-summative grouping is found in a static charge distribution. If an electric current is conducted to an isolated wire, its total charges quickly come to a state of equilibrium and the whole surface of the conductor has the same potential. This static grouping remains the same so long as no other disturbing factors are present. Such a state of equilibrium Köhler calls the charge distribution. This distribution is not a mere sum of part charges, since electricity cannot be taken away from any part of the system without influencing every other; when this is done, a new state of equilibrium is effected, which represents the new charge distribution. The first criterion of configurations, enunciated first by Ehrenfels, therefore applies to physical systems in that they too possess properties which are characteristic of the whole structure: the physical system possesses a certain potential, a specific density at every point, and a pattern representing the distribution of energy. The second criterion, that of the transposability of structures, applies also to these systems; for if one doubles the potential, the charge density doubles at every point. Stated generally, proportional changes in the size of the conductor and of all other factors in the situation do not change the distribution of charge. The distribution depends only on the form of the conductor. And if we substitute for the observer a single conductor, then,

[55]W. Köhler, *Physische Gestalten.*

[56]W. Köhler, ibid., Introduction.

just as the notes of a melody must be perceived by some one person in order to be heard as a melody, so must the charge be carried by a single conductor.[57]

On the basis of his physical principles Köhler erects a physiological theory according to which psychophysical configurations represent special cases of more general physico-chemical processes.[58] The events in the nervous system appear in many respects like reactions in diluted solutions. Processes in the nervous system are largely loosened matter, electrolytically dissociated. Ionic reactions play an essential part in conduction. If the nervous excitations are constant, the chemical processes in the somatic fields are stationary, and the ionic concentrations have a constant value. Excitations in the somatic field with constant outer conditions are quasi-stationary chemical reactions, in dilute solution, in which ions participate. Therefore at every moment the excitation states are determined by the concentration of the reacting ions. If two different parts of the periphery are stimulated in a constant manner, these will represent two part-domains differently stimulated but which bound each other in some kind of a continuous curve. In the somatic field there will be two corresponding part-domains bounding each other, but with different ionic concentrations. Owing to diffusion and to the different velocities of the positive and negative ions there will be a drop in potential or electromotive force in each field. This drop is a function of the kind of ions concerned in the process and of the amount of their respective concentrations. Important consequences for the various fields of perception, only one or two of which can be mentioned here, follow from these facts.

According to this theory, the nervous system is highly permeable and contains no isolated segments functioning independently of the rest. In fact, Köhler maintains that we must regard the retina as a part of the central nervous system functionally as well as anatomically, so that the whole afferent and efferent optic sector forms a single system. Contours in visual perception would then represent bounding portions of two differently colored fields in which there is a difference of potential. The amount and direction of the change in potential are determined by the color excitations: as the colors approach each other in quality the potential difference disappears and at the same time the contour vanishes. The contour is (as Rubin pointed out) not a mere geometrical boundary, but is determined by both the fields out of which it arises.

The way in which Köhler applies physical configurational principles to psychological configurations may be shown by reference to his explanation of figure-and-ground. If we perceive a small white circle on a gray background, we may assume that two quasi-stationary reactions are set up in the retina, the one corresponding to the figure, the other to the ground. There will be different ionic concentrations in each area. Between the white and the gray there will arise a difference in potential which is responsible for an electromotive force between them. Furthermore, there will be differences in potential between the stimulated portions of the retina and the optic nerves, since it requires time for the chemical reactions to occur in long-section as well as in cross-section. Because of the differences in potential electric currents will arise, travelling from the retina through the optic nerves and from one portion of the retina to the other. The current will be stronger and more dense in the smaller, circular area than in the broad gray background, for the simple reason that the energy of the system will be more strongly concentrated in the smaller area than in the larger. Since the energy per unit area is greater in the figure than in the

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[57]*Ibid.*

[58]The exposition of this and the following three paragraphs follows the argument of Köhler's *Physische Gestalten.*

ground, the figure will possess greater impressiveness, and will therefore predominate in perception. This reasoning applies just as well when the figure is gray and the background is white, so that the vividness of the figure is not a matter of color or brightness effect so long as the colors are different. Association, attention, and imaginal additions are unnecessary to explain the predominance of figure over ground, since the laws of configurational structures explain it on the basis of what goes on within the structure itself.

Köhler further believes that the new point of view can be applied to the explanation of the thought processes. The appearance of complex unities possessing specific structures in the physical world which are copied in perception leads us to think, he says, that even the higher thought processes are accompanied by configurational processes within the nervous system. It had always been an objection against the assumption of a physical correlate of the higher thought processes that unities possessing specific structures could not be correlated with any corresponding brain processes. Configurational principles enable us to achieve this correlation.

Up to this point the road from physics to psychology has seemed fairly clear. But it is not always possible to translate directly from physics into psychology. The configurationists often find themselves compelled to resort to biological and logical principles in order to supplement their descriptions and explanations of psychological configurations. Biological conditions may determine phenomenal and behavior patterns. Thus we are told that the animal and the child do not react to an environment composed of elements nor by means of unit reflexes. An emergence from a general level greets the newly-born infant, rather than patches of colors with a certain hue, saturation and tint. Primitive reactions are to functional situations by means of total responses. Chain-reflexes knit together by trial and error, stamped in no one knows how, cannot explain the activities of the child or of the hungry ape. Discrimination is a product of later development, and consists in learning to react to finer and finer structures. Biologically such terms as "good," "bad," "dynamic," and "natural" represent configurational properties of the environment and the kind of response which the organism makes to it.[59]

Still another field furnishes the configurationists with descriptive categories and criteria of configurational processes,—that of logic. Wertheimer draws very heavily upon this source. This author stresses the fact that configurations are governed by "inner" as opposed to "outer" necessity, the inner and outer referring now to internal *versus* external relations, and now again to the forces at work within a configuration as opposed to the outer conditions. Logical meanings and implications may determine what is seen in perception or what the train of associations will be in thinking. Whereas Köhler means by "simplicity," "economy," and "precision" physical or biological facts, Wertheimer usually employs the terms as logical concepts applied to psychological facts. When Wertheimer writes that the "whole determines the nature of its parts," we cannot be sure whether the whole referred to is the total psychological structure, the whole logical meaning, or some other product of reflection. A typical sentence from Wertheimer shows his logical emphasis very clearly: "dabei muss klar sein dass z. B. Symmetrie durchaus nicht einfach eine 'Gleichheit' von Teilen ist, sondern logisch richtig nur vom Ganzen her, als Ganzeigenschaft gefasst werden kann."[60]

The question arises how far logical, phenomenal and physical configurations obey the same laws and harmonize with each other. We find that in some cases phenomenal fields may copy physical fields, while in others they do not. Physical and psychological simplicity may or may not coin-

[59]K. Koffka, *The Growth of the Mind.*

[60]M. Wertheimer, *Psych. Forsch.*, 4, 1923, 325.

cide; *e.g.*, a straight line is geometrically simple and perhaps, in some sense, physically simple also; yet it cannot be considered as a configuration for a number of reasons, and mainly because it lacks the property of completeness (*Geschlossenheit*). So too "good," "bad," "precise" and "natural" mean differently according as they bear a physical, biological, logical or psychological stamp. The lines are therefore not sharply drawn, and so it is impossible to fix precise meanings for the concept of *Gestalt* when we examine it closely in its various uses in the configurational literature.

(8) *Some Systematic Implications of Configurationism*

What are the general implications of configurationism viewed in their systematic setting? I can point these out very briefly in recapitulating the sum and substance of this paper.

The most striking development in the configurational psychology seems to consist in extending the boundaries of the psychological to include the facts of biology, physics, and logic within a single system deducible from a common set of assumptions. I have already pointed out how configurational concepts are used in this task, and I leave it for later discussion to decide on the merits of the question. The emphasis put upon physical and physiological hypotheses indicates a tendency among members of the new school to seek objective explanations of phenomenal facts, or at least to employ principles common to what we may call, for convenience, the "objective" sciences. Mind thus becomes a phenomenon within a larger system of phenomena and amenable to precisely the same laws, with no peculiar status or properties. Strangely enough, this point of view does not lead to behaviorism but rather toward greater emphasis upon central neural processes as opposed to peripheral correlates of phenomenal patterns. The configurationists hope to make the transition from brain process to conscious process without any break in theory.

In order to approximate the facts of everyday life more closely and to avoid the artifacts of laboratory procedure, the configurationists substitute phenomenal patterns in place of elements as the typical data of observation. These patterns, they assert, are no less immediate than the sensory components which Benussi and others find to be given first in perception. The aim of experimentation, according to this point of view, should be to describe and explain the origin, growth, and decay of configurational processes both in the nervous system and on the "psychophysical level" where phenomenal patterns appear. No assumptions or theories regarding the ultimate constituents of consciousness find a place in configurational psychology. While it is admitted that the only basis for a choice between a description in terms of elements and one in terms of configurations is pragmatic, it is the belief of Koffka, Köhler and Wertheimer and their followers that the "best" scientific descriptions will be formulated in terms of the configurational properties of the observed facts.

The new point of view implies new functional descriptions as well as a new treatment of perception. The typical form of psychophysical connection, stimulus-experience, is no longer the sensation but rather the configuration. The configuration must be regarded as the correlate of the stimulus just as was formerly the sensation. While configurations requiring time for their completion offer the configurationists many difficulties, they feel that the concept of the *Gestalt* is designed primarily to embrace dynamic processes extended in time as well as in space. Motor reaction-patterns are determined in their temporal order by the end or goal which may be an integral part of a configuration even though it is not present in time and space. Part-activities can thus fit into meaningful ordered wholes for which the ground of connection is the concept of the configuration.

In place of the older descriptions in terms of elementary processes and their attributes, the configurationists speak in terms of configurational changes somewhat in the following fashion: configurations become more articulate or more finely structured, simpler in form, more precise or less definite, poorer in form or chaotic. What is to be singled out in perception or memory will depend upon the position of the part within the whole, whether or not it is a natural part and will lend itself to membership in a new configuration, and upon a number of other factors like coherence, persistence and impressiveness. In general, the progress of configurational change is from chaotic groups of elements, at the one extreme (really a minimum of "structuration"), to more and more complex structures. The development of perception, memory, imagination, thought and action consists in the development of configurational patterns in ever-increasing complexity and variety within a given form.[61]

Which comes first, the sensation or element without form or order within a group or the configuration? This question is answered in many ways, according to the point of view of those who have tried to answer it. We should expect to find differences between the configurationists on the one hand and members of other schools on the other. But there are "half configurationists," as well as the left wing represented by the Graz school; and the configurationists themselves have formulated various theories of the origin of configurations. We survey the various possibilities, in order to resume *in nuce* what the leading writers have said on this point.

It will be remembered that the followers of Meinong, particularly Witasek and Benussi, maintain that stimuli give rise to sensations which are united into patterns and configurations by special productive acts. Several configurations may arise from a given set of elements, with no possibility of predicting which will be favored. Koffka objects to this position on the ground that a strict division of conscious contents (sensation

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[61]K. Koffka, Introspection and the Method of Psychology, *Brit. J. Psych.*, 1924, 15, pt. 2, General Section.

and production-process) is impossible; furthermore, sensations are no more stable than configurations, and to give the former a real status as against the ideal status of the configuration implies the 'constancy' hypothesis in its worst form. Koffka drops the sensation as the immediate correlate of the stimulus and puts in its place the configuration, which thus appears first in the history of the race and of the individual.[62] Köhler derives the properties of phenomenal configurations immediately from physical structures, and endeavors to account for discrepancies between the two by reference to irregularities in the sense organs or nervous system.[63] Wertheimer's paper of 1912 seems to imply that single excitations may give rise to total physiological processes, which in turn are accompanied by phenomenal configurations; the change from singleness to group-unity seems to take place in the nervous system. Bühler[64] assumes that simple configurations cannot be derived from sensational processes, although complex configurations may be built out of simpler and resolved into simpler again. Linke[65] extends the notion of configuration to include all the "formal" aspects of experience; the "material" of experience is sensational, but always comes to us in some form or pattern. Still another view has been stressed by Poppelreuter, who worked with pathological observers.[66] His investigations demonstrate, he believes, that structures, forms, and patterns do not appear in perception until very late. Sensory contents may be present in an amorphous state with no hint of structure or form. The perception of configurations requires higher physiological mechanisms which may be destroyed while the sensational mechanisms remain intact. On the extreme sensational side stands Müller[67] who asserts that elements must be united into groups by acts of attention.

We have thus run the entire gamut of possibilities regarding the origin and nature of configurations and their parts.

(9) *Conclusion*

From this general statement of principles it is clear that configurationism draws its inspiration from many different sources. It reaffirms, in modified form, the facts enunciated by Ehrenfels, demanding that the morphological aspects of experience receive consideration at the hands of experimental theory. Configurational structures are resident within perception, thought and action and give them meaning, order and value. With the members of the imageless-thought school, the configurationists assert that the thought processes must be regarded in their temporal, meaningful connections if their significance is to be explained. The active dynamic aspects of mind and body find an explanation not in the unobservable acts of the followers of Brentano, but rather in a union of act and content, and of mind and body, as they function in a material, biological and social environment. The fundamental explanatory and descriptive terms of our science must be observable and capable of factual verification. Experimental psychology must turn from the artifacts of laboratory procedure and systematic bias to the everyday experiences of actual life.

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[62] K. Koffka, *op. cit.*, *Z. f. Psych.*, 1915.
[63] W. Köhler, *Physische Gestalten.*
[64] K. Bühler, *Die Gestaltwahrnehmungen*, 1913.
[65] P. Linke, *Grundfragen der Wahrnehmungslehre*, 1919.
[66] W. Poppelreuter, *ibid.*
[67] G. E. Müller, *op. cit.*

THE PHYSICAL BASIS OF THE CONDITIONED RESPONSE

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I. *Introduction*

The prominence which the conditioned-reaction principle has attained in modern psychology seems to justify a closer study of the physical factors involved, and the present paper attempts to supply this deficiency. We do not assume that the conditioned response is the *only* form of association, but we attempt to show that it receives considerable support from nerve-physiology, physics and chemistry. The distinctions ordinarily made between physics, biology and psychology are in the last analysis artificial and principally for academic convenience,—and a certain amount of harmony on the physical side will clarify several of the problems involved in the psychology of learning.

The particular experiment which will be used for illustrative purposes in this paper is one which was carried out by the writer on the pupillary reflex.[1] In this experiment, as in another study, on the conditioned eyelid reaction,[2] the "voluntary factor" criticism of the conditioned reaction was eliminated;[3] in the former case because the pupillary reflex could not be controlled "voluntarily," and in the latter case because the time of the conditioned eyelid reaction was made faster than the so-called voluntary factors can make it. In the pupillary experiment, the *S*s were not aware of the act which was learned; and in the eyelid experiment they were not conscious of winking until after they had winked.

In the pupillary experiment just referred to, an increase in the intensity of light falling on the *S*'s eye caused a contraction of the *sphincter iridis* muscle. In this way contractions of the pupil were evoked 400 times at intervals of about 15 sec.; and while the *S*'s pupil was contracting in this training series a bell-stimulus was also present. After this rather long training

[1]H. Cason, The Conditioned Pupillary Reaction, *J. Exper. Psychol.*, 5, 1922, 108-146.

[2]H. Cason, The Conditioned Eyelid Reaction, *ibid.*, 5, 1922, 153-196; A Note on the Conditioned Eyelid Reaction, *ibid.*, 6, 1923, 82-83.

[3]According to this criticism, the *S* is always aware of the learned act and has a "desire" or "wish" or "impulse" to acquire the new reaction-tendency.

period, the bell-stimulus alone would contract the pupil. A connection was obviously formed between the end-brushes of axones coming from the cochlear division of the eighth nerve and the dendrites of cell-bodies whose processes innervate the *sphincter iridis* muscle directly or indirectly. This association may have been formed in the oculomotor nuclei, or more probably in the temporal lobe of the cerebrum. Before the training period for each *S* was begun the bell caused a slight dilatation of the pupil in the case of every *S* tested. The nervous connection between the bell and a contraction of the pupil was therefore not in functional activity at the beginning of the training period, and the experiment illustrates clearly the way in which an acquired reaction can be actively built up on the nervous structure already present. It is very arbitrary to suppose that

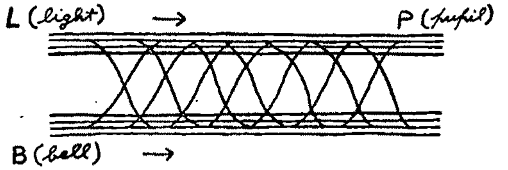

FIG. 1. Showing the nervous path from the retina to the *sphincter iridis* muscle (L to P), together with certain nerve fibres which were innervated by the bell-stimulus (B). The fibres passing from the retina to the *sphincter iridis* muscle and those innervated by the bell intermingle at several points.

a conditioned reaction can be established only when there is a "tendency" already present in the nervous system. The nerve fibres are there, to be sure, but they are not in functional activity. We could with just as good reason suppose that there is a "tendency" in the nervous system for any stimulus to evoke any response.

The path between the retina and the *sphincter iridis* muscle was innervated every time the light-intensity was increased, and this path is represented diagrammatically in Fig. 1 (L to P). The bell-stimulus (B) was also present every time this path was functioning. While we have little detailed knowledge of the reactions ordinarily called out in the human organism by the sound of an electric bell, the bell-stimulus certainly innervated several neurones whose end-brushes are in close proximity to the path from L to P. Since the bell-stimulus alone would later cause a pupillary contraction, some such intermingling as is represented diagrammatically in the figure must be accepted.

Fig. 2 represents a unit of Fig. 1. The nervous path between the light and the pupil was in very good working order during the experiment, and one of the synapses in this path is represented in the figure. An end-brush of one of the neurones

innervated by the bell, which at the beginning of the experiment was not functionally connected with P, is also represented diagrammatically in the Fig. by B. At the beginning of the experiment, the impulses coming along B may or may not have passed over to another neurone S. As stated above, the bell-stimulus caused a dilatation of the pupil at the beginning of the experiment in the case of every *S* tested. Some of this effect may have persisted throughout the experiment, but the external results showed that the tendency toward contraction was relatively more prominent than the tendency toward dilatation at the end of the experiment. Some impulses may have continued to pass from B (in Fig. 2) to cell-bodies whose processes caused

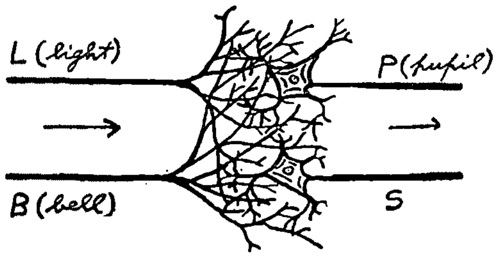

FIG. 2. Showing a unit of the situation represented in Fig. 1. L is an end-brush of one of the neurones innervated by the light-stimulus, and B an end-brush of one of the neurones innervated by the bell-stimulus. P represents a cell-body in the path from an increased light-intensity to a contraction of the pupil. The synapse between L and P remained in fairly good working order throughout the conditioned reaction experiment described in the text. Neurone S indicates the course which an impulse coming along B may or may not have taken at the beginning of the experiment. The two end-brushes, L and B, end in close proximity to nerve-cell P.

various reactions in the body, such as vasomotor changes, alteration in heart-beat, breathing, etc., the exact nature of which is not known. S in Fig. 2 arbitrarily represents all of these changes in the body ("secondary reactions"), in addition to a dilatation of the pupil caused originally by the bell-stimulus. The two end-brushes in the figure (L and B) are shown ending in close proximity to nerve cell P. In our conditioned-pupillary-reaction experiment, L and P were *active* simultaneously 400 times. Every time they were active, however, end-brush B was *also active*. At the *end* of the experiment, but not at the beginning, the stimulus from L could be omitted, and an impulse would pass from B to P. Our task in the present paper is to give an explanation of the formation of this association between B and P. Before proceeding with this explanation, however, we shall have to consider a modern and also a very

plausible theory of conduction in nerves, that is, the theory which has been developed by Nernst, Lillie, Lucas, and Troland.[4]

II. The Nature of Nervous Conduction

The following account of the theory of nervous conduction is largely based on the account given by Troland,[5] to which the reader should turn if a statement of the experimentally demonstrated facts which support the theory is desired. The main features of this theory which concern us particularly are as follows.

A neurone is made up of an aqueous solution of various substances ordinarily held within the cell by a more solid differentiation in structure,—the cell membrane. Certain of these substances become ionized in solution and an equal number of positive and negative ions are formed. The ions of carbonic acid are formed by the respiratory function of the cell.[6] Since the cell has a temperature considerably above absolute zero, all of the molecules and ions of the cell are in rapid motion. The motion of the dissolved particles in the solvent, water, is similar to that of the molecules of a gas in free space; and these particles exert a pressure on the cell membrane. The positive hydrogen ion is the smallest known particle, and it will get through this semi-permeable membrane. The carbonate ions, which are much larger than the hydrogen ions, are held back by the membrane. In time a state of equilibrium is reached between the positive hydrogen ions on the outside of the membrane and the negative carbonate ions on the inside. The mutual attraction between these two layers of ions causes the hydrogen ions on the outside to remain in the immediate vicinity and limits the number of hydrogen ions which can pass through the membrane. The magnitude of this "electrical double layer," or polarization, depends upon the permeability of the membrane and the concentration of the ionized substance.

In order to stimulate the nerve cell, the electrical current must *depolarize* the membrane to a certain critical degree. This assumption seems to harmonize with various facts concerning nerve-functions, including Pflüger's law of electrical stimulation.[7] The cathode sprays the nerve with electrons, and these negative charges neutralize the positive hydrogen ions on the outside of the membrane and depolarize it. The anode takes electrons away from the outside of the cell, thereby increasing the layer of hydrogen ions at this point. The membrane compensates for this increased polarization.

To quote from Troland: "It is evident that the Nernst theory of stimulation in the modified form outlined by Lillie demands a 'negative variation' of the 'current of rest,' as empirically found, since depolarization would necessarily manifest itself in this way. It does not follow, however, from any statements made heretofore that this negative variation will be

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[4]W. Nernst, Zur Theorie des elektrischen Reizes, *Arch. f. d. ges. Physiol.*, 122, 1908, 275-315; R. S. Lillie, The Relation of Stimulation and Conduction in Irritable Tissues to Changes in the Permeability of the Limiting Membranes, *Amer. J. of Physiol.*, 28, 1911, 197-222; K. Lucas, *The Conduction of the Nervous Impulse*, 1917; L. T. Troland, The Physical Basis of Nerve Functions, *Psychol. Rev.*, 27, 1920, 323-350; and other papers.

[5]*Op. cit.*

[6]A solution of carbon dioxide in water (H_2CO_3) has the properties of a weak acid. Carbonic acid conducts electricity, but not well. The bicarbonate ion (HCO_3^-) itself ionizes to form a hydrogen ion (H^+) and a carbonate ion CO_3^{--}). The ion, HCO_3^-, is an extremely weak acid.

[7]For a brief account, see W. H. Howell, *A Text-Book of Physiology*, 1918, 88-90.

propagated along the cell or fibre from the point of stimulation. Lillie has pointed out that to explain propagation of the variation we must suppose not only that the polarization of the membrane depends upon its differential permeability but that its relative impermeability depends upon its degree of polarization. Polarization and permeability, in other words, are the two essential factors in the nerve process, and they are related to one another in the propagation of the nerve impulse in a manner analogous to the relation of pressure and displacement in the propagation of sound or the relation of the electric and magnetic vectors in the propagation of light. These relations are all such that a change in one of the quantities always involves a change in the other, the locus of the secondary change not coinciding with or falling wholly inside of that of the primary change. This relation necessitates propagation."[8] The permeability is altered when an electric current depolarizes the membrane, because of the removal of the electrical lines of force which extended through the polarized membrane. These electrical lines of force which connect positive charges on the outside of the membrane and negative charges on the inside necessarily affect the structure of the membrane, and this distortion renders the membrane less permeable. Stimulation of a cell therefore increases the permeability of its membrane. "Tashiro finds that a small amount of carbon dioxide is given off by the nerve fibre even in the resting state, but that this amount is markedly increased during stimulation. This indicates that the normal permeability of the membrane is such as to permit a slight diffusion of carbonate ions and that the depolarization accompanying excitation is the result of an increased permeability to these ions rather than a decreased permeability to the positive hydrogen ions."[9]

The system, however, has a point of unstable equilibrium, as the following statements by Troland indicate: "An increase in permeability always produces a decrease in polarization. An initial decrease of polarization, however, first results in a decrease in permeability which tends to compensate for the initial change. But if the depolarization reaches a critical or threshold amount the 'sign' of the change is reversed, so that an increase in permeability results. As soon as this increase begins, the initial depolarization is further augmented by the law which makes polarization depend reciprocally upon permeability, so that the equilibrium of the nerve-membrane system is completely upset, the polarization now decreasing to a minimum while the permeability increases to a maximum."[10]

If an external depolarizing agent is applied very slowly at the cathode, the permeability of the membrane decreases below normal, the polarization increases above normal or remains at normal, and the depolarizing agency fails to stimulate. But "if the external depolarizing agency is applied very rapidly the membrane does not have time to develop its compensating reaction, so that the threshold depolarization is reached and excitation results."[11]

We shall have to let this matter rest with one further quotation from Troland's paper. "There are undoubtedly certain physical *constants* which determine the processes of the nerve membrane. Among these are the threshold depolarization required to stimulate, the magnitude of the excitation or maximal depolarization, the rate of recovery of the membrane, etc. At the synapse these constants appear to suffer a radical change in magnitude, of such a character that all of the processes are retarded; the latent period is much longer, as is also the refractory phase, and if we suppose the synapse to have a phase of hyperexcitability this

[8]*Op. cit.*, 329.
[9]Troland, *op. cit.*, 331.
[10]*Op. cit.*, 333.
[11]*Op. cit.*, 334.

also is probably much prolonged. By a proper choice of the values of the various membrane constants the majority of the characteristic features of synaptic functions can be explained."[12]

The reader should refer to the original papers for evidence in support of these views. We have given no evidence justifying the theory, but have attempted merely to state it; and even this has been done in a very summary fashion.[13] However, we are now in a better position to make several concrete suggestions in regard to the learning process, the first being in terms of an alteration in the nerve-cell membrane.

III. Learning and the Nerve-Cell Membrane

We may now take up the learning process where we left it in *I.*, and attempt to explain how the association between B

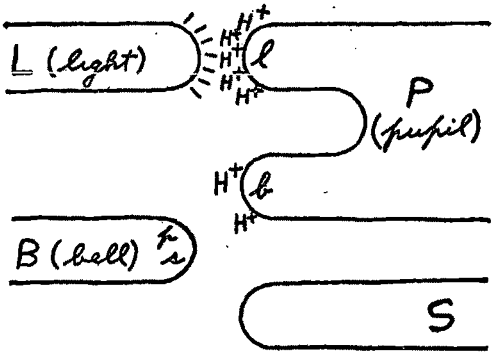

FIG. 3. A unit of the situation as represented in Fig. 2. L represents a portion of an end-brush in the path from an increased light-intensity to a contraction of the pupil. P represents part of a dendrite or cell-body in this same path. B represents a portion of an end-brush innervated by the bell-stimulus. The portion of P at *l* is close to L, and the part of P at *b* is close to B. The path from L to P continued in fairly good working order throughout the conditioned-reaction experiment described in the text. B would innervate P at the end but not at the beginning of the experiment. An impulse from B may or may not have passed over to a dendrite or cell-body S, calling out various reactions in the organism.

and P in Fig. 2 was formed. In Fig. 3 we have represented a still smaller unit of the situation as shown in Fig. 2. We shall continue to use the experiment on the conditioned pupillary reaction for illustrative purposes. While we make use of several principles included in the Nernst-Lillie theory, our suggestions [illegible]ng the physical basis of the conditioned response do not [illegible] solely upon this theory. The Nernst-Lillie theory relates directly to only one of the four general groups of factors which we shall consider.

In Fig. 3, L represents a portion of an end-brush in the path from an increased light-intensity to a contraction of the pupil.

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[12] *Op. cit.*, 344.

[13] For a general and historical discussion of nerves as conductors, cf. G. T. Ladd and R. S. Woodworth, *Elements of Physiological Psychology*, 1911, 127-144.

P stands for part of a dendrite or cell-body in the same path. B represents a portion of an end-brush innervated by the bell-stimulus. The portion of P at *l* is close to L, and the part of P at *b* is close to B. The path from L to P continued in fairly good working order throughout the experiment. An impulse from B may have passed over to a dendrite or cell-body S, thereby causing some reactions in the organism. The general significance of S has been discussed in *II*., in connection with Fig. 2, and these considerations apply to Fig. 3 as well.

As explained above, B and P are close together at several points. In our illustrative experiment, P was innervated and made active by L 400 times. B was also active every time L and P were active. At the beginning of the experiment, an impulse would not pass from B to P; but after 400 repetitions of the training stimuli (L, P, and B active simultaneously) L could be omitted and B alone would innervate P. The compound stimulus, L with B, followed of course by activity in P, continued for about 1 sec. at each repetition. Now every time P was made active by L, the ionization process in P was increased. This caused more positive hydrogen ions to appear on the outside of P than would otherwise have been the case. While the processes described in *II*. were taking place in the synapse between L and P, the positive hydrogen ions appearing on the outside of membrane *b* were also being neutralized by a "spray of electrons" from B.[14] Now if B had not been active at all, *some* hydrogen ions would have been present on the outside of P, even on portions of P situated at a distance from *l*. The *simultaneous* activity of B and P, however, causes a considerable *increase* in the activity at *b*. Hydrogen ions are now neutralized in large quantities at *b* by electrons coming from B. Greater strains are set up in *b* by the stronger lines of force. The movement of ions through *b* would have been "leisurely" if B had not been functioning simultaneously with P, but when B and P are active simultaneously hydrogen ions move through membrane *b* in much larger numbers. The ions coming out of P,

[14]When an impulse is passing along a nerve, caused either by electrical, mechanical, chemical, or reflex stimulation, the point reached by the impulse is negative to all other parts of the nerve. This is the well-known "wave of negativity." Some free electrons are always present during stimulation; and the velocity of the nerve impulse and the action current are identical. The nerve always shows an action current when it is active, and the influences which alter the velocity or strength of a nervous impulse affect the velocity or strength of the action current in the same manner. Conduction along a nerve (or, rather, in each neurofibril) has been likened to that taking place in a "core conductor." (See W. H. Howell, *A Text-Book of Physiology*, 1918, 108; E. H. Starling, *Principles of Human Physiology*, 1920, 284. For a brief statement of the various influences which may modify the nerve impulse, see Howell, *op. cit.*, 113-116.)

through *b*, and toward B, knock the molecules of membrane *b* in position. They open up pathways in membrane *b*, and cause a rearrangement of the molecules. The molecules of membrane *b* may now be said to be *oriented*, so that electrons can "flow" through it more readily than before. The action of the whole system thus results in an increased permeability of membrane *b*, which is therefore left in a condition which is more favorable for the passage of an impulse from B to P than at the beginning of our experiment.

Perhaps it is unnecessary to call attention to the fact that the explanation just suggested does not follow point by point the theory of nerve-functions outlined in *II*. The theory developed by Nernst, Lillie, Lucas and Troland attempts to account for the passage of a nervous impulse *after* the association has been formed. Our task is to explain the *formation of the association* in the first place, granting that after it is formed the nerves may function as these writers describe. From some points of view, ours is the easier task.

There are several phenomena in nature similar to the one described above. For example, a piece of iron can be magnetized by rubbing it on a permanent bar magnet, and thus causing (in somewhat popular language) a rearrangement of its molecules. If you give this temporary magnet several sharp blows, the magnetism will be lost, because the arrangement of the electrical forces has been disturbed.

According to the theory of nerve-functions outlined in the previous Section, the passage of a nervous impulse from L to P is accompanied by an oscillation of the permeability and polarization of the membrane around P. Such an oscillation apparently affords even better conditions for a definite alteration in the structure of membrane *b*. This oscillation causes a change in the distribution of the forces which hold the membrane together. The change is further aided by the simultaneous activity of B, which, acting somewhat like a battery of machine guns, shoots more electrons through *p* in the direction of *b*, neutralizes more positive hydrogen ions at *b*, causes more hydrogen ions to pass through *b* from within P, and in general increases the chemical activity around *b*. As Troland suggests, the slight generation of heat may be neutralized by the "evaporation" of hydrogen ions. More carbon dioxide is also given off at *b* than would have been the case if B had not been active.

In the above discussion, we have said comparatively little about the rôle of S (Fig. 3), which represents the "secondary reactions" called out by the bell at the beginning of the experiment, including the dilatation of the pupil. We have seen that the dilating effect of the bell which was present at the beginning of the experiment was overcome by the training, and we do not have to consider it further at this time. We may, however, consider the other effects which were produced in the organism

by the bell-stimulus, such as change in heart-beat, breathing, etc., disregarding from now on the dilatation factor. These reactions are arbitrarily referred to as "secondary" because they are not particularly significant in the mechanisms we describe. The nervous pathways leading to them from B are designated by S in the figure. We are not absolutely certain whether S was present at all in the simple situation which we have been considering for the sake of clearness and for illustrative purposes; and, if present, we do not know whether an impulse was passing from B to S at any time during the experiment. In case S was absent or present and not functioning, we have nothing to explain. In case S was present and functioning, then the following considerations apply. There is apparently nothing in the situation which would *strengthen* the connection between B and S, and we have shown how the connection between B and *P* is strengthened. There is therefore a tendency for energy from B to be diverted from S to P. Since reflex movements called out by a sound-stimulus become adapted or fatigued very rapidly, we have some reason for believing that in many cases an impulse did not continue to pass from B to S for more than a few minutes. In some cases *both* synapses (between B and P and between B and S) may have been functioning at the end of the experiment. In other cases the connection between B and S may have been functioning at the end of the experiment but the synapse between B and P at no time during the experiment; and we are of course not concerned with this alternative, since no connection would have been formed between B and P. There are still other cases, however, where a connection *was* formed between B and P; and it should be remembered that there is a tendency for energy from B to be diverted from S to P. In *some* cases impulses passed from B to P and not from B to S, and these cases are significant from our point of view. The possibility of S being present and active is not necessarily a criticism of the processes which we have outlined above, or which are yet to be described.

While the above considerations seem to be a part-explanation for some forms of *temporary* modification, they may not be a very satisfactory explanation for many kinds of more or less *permanent* learning. The alteration in the structure of membrane *b*, resulting in a situation more favorable for the passage of an impulse from B to P, may be only *temporary* in nature, and membrane *b* may quickly revert to many of its old habits. A slight shock might cause the particles in *b* to drop back into their old positions. Although the molecules of membrane *b* are oscillating back and forth continually (heat or temperature quality), they may not leave the immediate vicinity, but act as if they were tied together by "strings." In the experiment

and general situation referred to above, there is a possibility that the "strings" holding the particles of membrane *b* in position were stretched considerably but not broken. Some shaking-up process might make membrane *b* revert to its old condition—just as iron filings which are scattered on a piece of paper in a magnetic field tend to rearrange themselves if the paper is tapped.

This suggestion in regard to the temporary alteration in structure of the cell-membrane is in harmony with certain external aspects of memory which are commonly observed. A very large percentage of the associations formed in ordinary life are *temporary* in nature. In the two experiments also which the writer carried out on the pupillary and eyelid reflexes, memory for the conditioned reactions persisted for only a short time. Many learned reactions in everyday life are as quickly forgotten, *e. g.*, a person's name, a telephone number, etc. In the ordinary process of reading, the number of reactions which are learned, and forgotten before 10 min. have passed, mounts up into the thousands if the process is continued for just a few hours. The very sudden drop in the curve of forgetting for material that is just barely learned also illustrates the same fact.

There are no *qualitative* differences between the modification of a reflex and the kind of learning which especially involves the "association areas" of the cerebrum. Learning a person's name involves more particularly the *connecting* process, and does not *oppose* well-established reaction-tendencies. An alteration in reflex behavior, on the other hand, generally involves a much greater mass of bodily tissue; it is of a more drastic nature, and is accomplished only by *opposing* integrated neuro-physiological responses; that is to say, by opposing well-established tendencies which are in excellent working order and nearly always ready for business. The physical changes involved in associating the name "John Long" with a rather tall individual involve especially the *connecting* process, because both "John" and "Long" are already familiar, and the appearance of this individual may suggest past experiences to us. All that is necessary in forming this bond is to *connect* two things, either one of which is working smoothly. The "association areas" of the cerebrum afford ample opportunity for such connections being formed. The process is mainly one of shunting nervous impulses in a new direction with practically an open road in front, or of the transference of a comparatively small amount of energy where the distance to be moved is slight. In modifying a reflex response, however, we must *unlearn* a much larger number of connections. Some of these have been functioning since birth, and in some cases before birth. They are well "oiled" and very efficient from the standpoint of pure dynamics.

If we take account of these considerations, it is not surprising that an association may be formed between the pupillary reflex and an originally non-effective stimulus only after prolonged training, whereas a person's name may be learned in a few seconds.

The physical mechanisms discussed in this Section are related to most of the other physical processes which will be described in the three following Sections. The change in membrane *b* is only one of the factors at work in the association process.

IV. Learning and a Gross Movement of Nervous Elements

In the situation represented in Fig. 3, there is a possibility that the nervous elements B and P may actually move toward each other. There are no facts in neurology which militate against the possibility of such a movement, and there are several reasons why such movements probably take place. End-brushes and dendrites are composed of very fine branches practically swimming in an aqueous solution, and it is to be expected that these fine branches would not be entirely stationary. Our explanation of the possible cause of such a movement is as follows.

The electrolytic dissociation process which is at work in the situation represented by Fig. 3 causes positively charged particles in P to migrate toward *b* and *l*, and negatively charged particles in P to move away from *b* and *l*. However, not all of the negatively charged particles in P move away from *b* and *l*; some remain, and assist in forming the electrical double layer referred to in *II*. The situation represented in Fig. 3, however, results in an *increased* movement of positive hydrogen ions toward *b*, which is due to electrons being delivered or freed at this point, partly because of the action of B. The electrical charge on the outside of *b*, as we have already seen, is *positive* in character. Since *b* is comparatively close to the principal mass of the cell-body P, the ionization process in cell-body P can supply *b* with a comparatively large number of positive hydrogen ions. Membrane *p*, on the other hand, is generally situated out near the end of a fine process and at some distance from the cell body of which B is the end-brush. Although the ionization process in the neurone of which B is a part may cause some positive hydrogen ions to appear on the outside of *p*, these hydrogen ions cannot be delivered at this point in as large quantities as at *b*, just across the way. An electrical wave of stimulation in B is also of greater general consequence than one of equal strength in P, because of the difference in volume and also because the hydrogen ions on the outside of *p* are more quickly neutralized than those on the outside of *b*. The free electrons

in B are probably more concentrated than in P. To use rather popular language, it may be suggested that the free electrons in B are in a regular stampede to get through *p* and to get at the hydrogen ions on *b*. The attractive forces present between the positive hydrogen ions passing out of P through *b* and the electrons passing out of B through *p* tend to move *p* toward *b*, and *b* toward *p*. Since B is smaller than P, and also because it is on the end of the very fine axone and not attached to a stocky branch, B will probably move more than P.[15]

The considerations regarding S which were stated in *III*. apply here as well. In the situation which we have described there may be some attractive force between B and S, but there is nothing in the situation which would increase this stress, and the attractive force between B and P *is* apparently increased. If B is suspended somewhere between P and S, its position at any given time will depend upon the relative attractive forces exerted by P and S. Having once moved to P, branch B might conceivably stick there just as a tack will stick to either one of two bar magnets, depending upon the relative position of the tack and the two bar magnets in space. Even if B moves only part of the way toward P, this movement will aid the learning process, since a decreased distance between B and P naturally increases the probability of an impulse passing from B to P.

This explanation of a movement of B and P toward each other may be a part explanation for that type of learning where the association is quickly made and also easily retained. This process also affords a part explanation for sudden forgetting due to *shock*. In such cases it is only natural that those learned reactions which involve the cerebrum especially would be more disturbed than certain well-established reflexes which generally involve fewer successive synapses. The effect of shock on loss of memory, loss of consciousness, etc., is more readily understood by reference to the physical elements described above. Nervous elements are doubtless jarred loose and things are forgotten in somewhat the same general way that a shock jars lead out of a storage battery. The reaction tendencies are changed and forgetting takes place in both cases.[16]

We do not wish to exaggerate the importance of a gross movement of B and P toward each other. Other factors must operate at the same time. This is just another factor at work in the association process.

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[15]See H. H. Bawden, A Digest and a Criticism of the Data Upon which is Based the Theory of Amoeboid Movements of the Neurone, *J. of Compar. Neurol.*, 10, 1900, 243-264; G. T. Ladd and R. S. Woodworth, *Elements of Physiological Psychology*, 1911, 288-292.

[16]W. Ostwald has attempted a chemical theory of memory in his *Vorlesungen über Naturphilosophie*, 1902.

V. Learning and the Substance Within the Cell

We have now considered two groups of physical factors which are involved in the conditioned response. First, there is a temporary change in the internal structure of membrane *b* (Fig. 3); and, secondly, there is a gross movement of B and P toward each other. These two groups of factors do not operate independently, and they are also not to be considered as alternative or exclusive explanations, since both are probably at work in all learning. We have next to consider a third factor, namely, the rearrangement of substances within the cell body itself. We ask the reader's indulgence while we consider first a few facts drawn principally from colloid chemistry, the "promised land of the biological scientist."

Colloid chemistry is principally concerned with the properties of a physicochemical state, rather than with the properties of any single group of substances.[17] If the particles in the disperse system are of microscopic or submicroscopic dimensions, the surface of contact between this phase and its neighbors will be very large, the surface forces will be very apparent, and the dispersoid will have varied and peculiar properties. In the dispersed condition, the vapor pressure, chemical activity and many other properties are different from those obtaining in the form of large aggregates. There is no sharp line of demarcation, however, between colloids and crystalloids.[18]

The adsorption coefficients of H^+ and OH^- ions are very high; and acids are generally better adsorbed than their salts. Colloids are especially well adsorbed and an ion of higher valence will generally be adsorbed more strongly than one of lower valence. In positive adsorption there is generally a fixation of the colloid or of the dispersed material upon the solid or other adsorbing surface, and this increase in concentration may cause an increase in the velocity of some of the reactions of the adsorbed substance.

In the electrolytic dissociation of an acid the charged atom attaches to itself several water molecules. Ions are generally made up of such electrically charged complexes. Colloidal particles may collect around these ions, the ions may be adsorbed by the colloidal particles, or the colloid itself may become ionized. When colloidal particles are electrically charged, the particles behave like ions and migrate through the solution under the influence of a potential difference. The particles in a colloidal solution, however, may be of varying size and they may also carry varying charges of electricity. An indifferent colloid in an acid solution will adsorb H^+ ions and become positively charged with respect to the fluid; while in an alkaline solution the colloid will become negatively charged with respect to the fluid medium. Negatively charged colloidal particles attract foreign crystalloid cations, and repel anions. Colloidal cations behave in the reverse way.

Water is prominent among the constituents of *protoplasm*; and many substances are negatively charged while others are positively charged with respect to water. The movement of colloidal particles in water may obviously be influenced by an electric current or by the addition of electro-

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[17]See E. Hatschek, *An Introduction to the Physics and Chemistry of Colloids*, 1916; W. W. Taylor, *The Chemistry of Colloids and Some Technical Applications*, 1915. The first-named is an elementary treatise.

[18]For some evidence favoring the position that the stoichiometrical laws of classical chemistry apply to colloids as well, the reader is referred to J. Loeb, *Proteins and the Theory of Colloidal Behavior*, 1922.

lytes. The dispersed phase itself may have acid or basic properties, in which case it will yield colloidal anions or cations respectively in pure water. Electrolytes are very common in living cells and the unequal distribution or concentration of ions accounts for differences in potential on the two sides of a membrane or diaphragm and between different parts of tissue. Various substances are carried to these cells in the lymph and blood, and these substances pass in and out of the cell membrane. The addition of alkalies will increase the negative charge if the diaphragm is negative. Increase in hydrogen ion concentration will tend to make such a diaphragm positive. In the case of a positive diaphragm, however, the addition of alkalies will decrease the positive charge, and increase in hydrogen ion concentration will tend to make such a diaphragm more positive. While differences of potential may develop between the aqueous and non-aqueous phases of these colloidal solutions, most animal membranes are the seat of electromotive forces only in faintly alkaline or faintly acid water.

The *structure of protoplasm* has received much attention from biological chemists, and a good many facts have been definitely established. Protoplasm may exist in all states from a liquid to a solid, and all of the complex molecules which compose it are colloidal in character.[19] In fact, all life processes take place in a colloidal system.[20] Protoplasm consists of a non-aqueous phase dispersed in an aqueous phase, and is a loose combination of proteins and lipoids.

The *nerve cell* is made up largely of proteins, while the nerve-cell sheath is composed principally of lipoids which behave quite differently toward acids and salts. Many *proteins* show the phenomenon of coagulation, which may be brought about by a rise in temperature or by chemical means. The adsorption phenomena have been studied most carefully in the case of albumin which has the character of a weak acid and a weak base, with the acid character more or less in excess. Proteins may be strongly adsorbed or they may be strongly adsorbent. There is an interesting parallelism between acid albumin and alkali albumin. When an *acid* is added to amphoteric albumin, the albumin migrates to the cathode, loses its coagulability by heat, its internal friction is increased, and its surface tension is diminished. When an *alkali* is added to amphoteric albumin, the albumin migrates to the anode, it is not coagulable by heat, its viscosity is increased, and its surface tension is diminished. Albumin ions are responsible for the increase in internal friction which has been observed.

The *functioning of nerves*, like that of muscles, is associated with a certain condition of swelling. Nerves lose their irritability when placed in solutions of non-conductors, and recover their irritability when placed in physiological salt solutions.[21] The *insulating sheath of nerve cells* is formed largely from *lipoids*, lecithin ($C_{44}\,H_{90}\,NPO_9$) and cholesterin ($C_{26}\,H_{44}O$).[22] Lecithins and cholesterin are widely distributed throughout the animal organism, and are of considerable biological importance.[23] They play an important rôle in the adjustment of metabolism between cells and their surrounding media. "Lecithin . . . forms an emulsion with water of its own accord; indeed like a protein it swells up in water into a turbid

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[19]See C. M. Child, *Senescence and Rejuvenescence*, 1915, 13-33.

[20]See L. Rhumbler, Das Protoplasma als physikalisches System, *Ergeb. d. Physiol.*, 14, 1914, 474-617.

[21]See H. Bechhold, *Colloids in Biology and Medicine* (trans by J. G. M. Bullowa), 1919, 354. Also C. S. Sherrington, *The Integrative Action of the Nervous System*, 1906, 196-199.

[22]See G. T. Ladd and R. S. Woodworth, *Elements of Physiological Psychology*, 1911, 117-126, for a general account.

[23]Cf. S. Hatai, The Growth of Body and Organs in Albino Rats Fed with a Lipoid-Free Ration, *Anat. Rec.*, 9, 1915, 1.

colloidal solution, without dissolving. It may be said to occupy a place, in respect to its colloidal properties, between the emulsifiable fats and the hydrophile colloids, closely approaching the latter . . . Lecithin acts towards colloids and suspensions (ferric hydroxid, mastic suspension) like any other colloid which migrates to the anode. Similarly charged colloids cause no precipitation. .; oppositely charged colloids produce flocculation in suitable mixtures (ferric-oxid hydrosol). . . *Cholesterin*, according to the investigations of O. Porges and E. Neubauer, is a hydrophobe colloid. Its aqueous emulsion behaves like a mastic suspension in the presence of a large variety of salts. The same is true for its behavior with other colloids. In neutral solution it is precipitated by certain proportions of albumin and saponin. Lecithin may act as a protective colloid for cholesterin."[24]

Althouth no membrane can be directly observed in the case of many animal cells, the cell-pellicle nevertheless has special properties. Lecithin and cholesterin collect in this membrane, and, among other things, change its surface tension. The thickness of the coherent pellicle (lecithin, etc.) may be as small as from 0.3 to 7.0$\mu\mu$. It may be invisible under the microscope and still fulfill its functions. It would be incorrect to consider the cell-membrane as strictly semi-permeable or stable in constitution, since it changes from time to time along with other parts of the cell. It may grow in one direction or in the other, or become thicker or thinner.[25]

There is much evidence from various sources that the transformation of energy in all kinds of matter, "dead" as well as living, is closely associated with electrical phenomena. Starling writes that "every beat of the heart, every twitch of a muscle, every state of secretion of a gland, is associated . . . with electrical changes. . . The uniform character of the electric response in different kinds of tissues suggests that an accurate knowledge of the changes in the distribution of charged ions responsible for the response ought to throw important light on the intimate nature of excitation generally."[26]

Returning now to the situation represented by Fig. 3, we should keep three general facts in mind: first, that an impulse passed from L to P at the beginning of our conditioned reaction experiment, and stimulated P; secondly, that an impulse *would not* pass from B to P at the beginning of the experiment; and, thirdly, that an impulse *would* pass from B to P after some training in the experiment had been given. While the system represented by Fig. 3 is active and while a "negative variation of the current of rest" is present on the outside of P, positively charged colloidal particles in cell-body P not only move toward the original source of stimulation, L, but also toward the new source of negative variation, B. When these colloidal particles reach membrane *b*, their positive charges are neutralized by the flow of electrons from B, and the colloidal substances are

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[24]Bechhold, *op. cit.*, 140-141. Cf. O. Porges and E. Neubauer, Physikalisch-chemische Untersuchungen über das Lezithin und Cholesterin, 2 and 3, *Z. f. Chemie u. Industrie d. Kolloide*, 5, 1909, 193-197.

[25]For an argument against the earlier views concerning osmosis and strictly semi-permeable membranes, the reader is referred to M. H. Fischer, *Oedema and Nephritis*, 1915, especially 3-34, 136-142, 153-164. Many problems of general interest are discussed in this work in addition to the strictly medical material.

[26]E. H. Starling, *Principles of Human Physiology*, 1920, 169.

deposited on the membrane, thereby forming a layer. This action increases the *concentration* of substances around *b*, probably improves the *conductance* in this region, and results in a situation which is more favorable for the passage of an impulse from B to P.

Various substances in aqueous solution are passing in and out of P, fulfilling the general nutritive functions of the cell. The amount of substance which will pass through membrane *b* depends upon a variety of factors, and among these is the nature of the solutions on the two sides of the membrane. Since the concentration of the dissolved substances is increased very markedly and hence the concentration of the solvent on the inside of *b* is diminished, a relatively larger number of the molecules of the solvent on the *outside* of the membrane will strike it in a given time, and because of the general mechanism of osmosis more material will pass into P through *b* than in the reverse direction. Although the osmotic pressure of colloidal solutions is low, it increases with the dispersion. With a very thin membrane this movement into P through *b* may also be influenced by the attractive forces between the substances on the inside and outside of membrane *b*. The movement will be mostly *into* P, since the particles on the outside of *b* are smaller. The meshes of the colloidal membrane may also be opened or closed, and this will naturally affect the diffusion.[27] The operation of the above factors will result in a *swelling* of P around *b*. (This does not necessarily mean that the *pressure* in P will be increased.) When P swells at *b*, B and P are brought closer together; and this improves the chances of an impulse passing from B to P.

Consider the effect of an increased hydrogen ion concentration on the swelling of the colloidal particles in P, especially at *b*. When the system represented by Fig. 3 is active, hydrogen ions are produced in P at *b* in larger numbers. It is well-known that a variation in the concentration of electrolytes within biological limits can cause a considerable variation in the water content. The presence of acids and alkalies increases tremendously the amount of water adsorbed by certain substances. Very dilute acids increase swelling, but when concentrated they have the reverse effect. "The influence of the hydrogen ion is so enormously great that the presence of even such a 'weak' acid as carbonic acid brings about a distinct increase in swelling."[28] Swelling depends upon the influence of the H-ions *minus* the

[27]For a general résumé of membrane-phenomena, the reader is referred to H. Zanggher, Ueber Membranen und Membranfunktionen, *Ergeb. d. Physiol.*, 7, 1908, 99-160.

[28]W. Ostwald, *An Introduction to Theoretical and Applied Colloid Chemistry* (trans. by M. H. Fischer), 1915, 161.

influence of the anions of the acid under consideration. There is an antagonism between anions and cations. Swelling in acids and alkalies is also considerably decreased by the presence of neutral salts. As stated in *III.*, the passage of an impulse from L to P ordinarily results in the liberation of a certain number of hydrogen ions in cell-body P, *but the simultaneous activity of B causes a larger number of hydrogen ions to be liberated around b than would otherwise have been the case.* Water is drawn *from* the space between *p* and *b* *into* that portion of P around *b*. This naturally causes a swelling in P around *b*, decreases the distance between B and P, and favors the passage of an impulse from B to P. There is also a tendency towards an increase in free surface with ionization and swelling, and this causes certain chemical reactions, especially hydrolytic cleavages, to take place more rapidly.

It is not difficult to understand that the chemical changes just enumerated have a more or less *permanent* character. After the swelling has taken place in nerve-cell P the heat produced by this swelling coagulates certain colloids. Colloids of opposite sign may also coagulate the protein substance of the cell. Electrolytes are known to coagulate certain colloidal solutions, and colloids of opposite sign may coagulate each other. While the coagulation of *suspensoids* by electrolytes is generally a reversible reaction, the coagulation of *emulsoids* is frequently irreversible or reversed only with difficulty. Electrolytes commonly cause flocculation in colloids. Small particles come together to form larger complexes. In the case of positively charged colloids, the presence of anions or negatively charged colloids will neutralize or lessen the electric charges, so that the particles will be freer to unite. The H^+ ion has a very powerful flocculating action upon colloids which migrate to the cathode. The reverse of flocculation is peptization, when the particles go back in solution. Peptization may be caused by an excess of electrolyte which renews the electric charge.

Other complicated factors are at work in addition to those described above, and among these may be mentioned the chemical reactions of colloids which are not due to the adsorption phenomenon; the interaction between different colloids; the relations between stability, solubility, viscosity, and conductance; the effect of an adsorbed ion on solubility; the action of protective colloids; the effect of heat and pressure on stability; the effect of a change in temperature, pressure, etc., upon the physicochemical equilibrium; the precipitation of insoluble substances; and the rôle of colloids and various ions in catalysis. The factors which we have mentioned, however, are fairly well defined and are clearly at work in the situation represented by Fig. 3. Our suggestions also seem to reinforce and

harmonize very well with each other. While points of false equilibrium may be established by a variety of causes, there is ample opportunity for the effects described above persisting for a while because of the numerous stabilizing forces at work.

In the above discussion, we have referred to the nerve-cell body as if it were a more or less homogeneous mass of protoplasm, and we have said very little about the differentiated structures which are present in the cell, such as the nucleus, nucleoplasm, chromatin, cytoplasm, neurofibrils, perifibrillar substance, etc. Some of these structures are chiefly nutritive in function, and concern us only indirectly. While different types of nerve cells apparently account for the differences in function, and *vice versa*, much work remains to be done on this subject before we can speak with certainty in regard to the correlations between different types of nerve cells and learning phenomena. There is a certain type of so-called association neurone in the cerebrum, but only a few very general facts can be stated concerning the relation between its structure and function.[29] The nerve-cell membrane has also been considered in the above discussion as if it were composed of a single layer, whereas what we have said refers especially to the superficial layer. Comparatively little is known about the difference in chemical constitution in the differentiated structures of the nerve cell.[30] The nucleus of most animal cells seems to be a complex of hydrosols, containing large particles, and it is poor in water.[31] The chromatin substance of the nucleus seems to have pronounced acid properties.

The complex factors mentioned above afford a plausible explanation for the very great differences observed in the general *impressibility* and *retentiveness* of different individuals.[32] The presence of certain chemicals in different proportions could easily account for a variety of individual differences, and the glands are already being considered in this connection. The facts which have to do with the *ageing* of colloids and the external aspects of memory also harmonize very well with each other. Crystalloids often retain their physical properties indefinitely, but colloids are metastable systems and change after a lapse of time. Age brings about a decrease in the swelling capacity and elasticity of membranes and also causes shrinking. Diffusion takes place more readily through young and fresh membranes

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[29]See E. F. Malone, Recognition of Members of the Somatic Motor Chain of Nerve Cells by Means of a Fundamental Type of Cell Structure, *Anat. Record*, 7, 1913, 67-82; The Nucleus Cordiacus Nervi Vagi and the Three Distinct Types of Nerve Cells Which Innervate the Three Different Types of Muscle, *Amer. J. of Anat.*, 15, 1913-1914, 121-130; G. W. Barthelmez, Mauthner's Cell and the Nucleus Motorius Tegmenti, *J. of Compar. Neurol.*, 25, 1915, 87-128; C. J. Herrick, *An Introduction to Neurology*, 1918, 105-112.

[30]See F. R. Bailey, *A Text-Book of Histology*, 1920, 130-149; W. H. Howell, *A Text-Book of Physiology*, 1918, 129-139; E. H. Starling, *Principles of Human Physiology*, 1920, 300-314.

[31]Cf. A. Kossel, Ueber die chemische Beschaffenheit des Zellkerns, *Münchener medizinische Wochenschrift*, 58, 1911, part 1, 65-69.

[32]Cf. A. F. Tredgold, *Mental Deficiency*, 1920, 77-83; K. S. Lashley, The Effects of Strychnine and Caffeine upon the Rate of Learning, *Psychobiology*, 1, 1917, 141-169.

than through old ones. Since there is a general reduction in metabolism it is not difficult to understand why a young person can learn new things more readily than an old person. The old person finds it difficult to produce a very radical change in his "apperception mass" partly because those portions of his nervous system corresponding to *b* in Fig. 3 are not so elastic, and diffusion does not take place so readily through them. The water content of the body also decreases with age.[33] Since time brings about a shrinkage in the cell membrane, the operation of the recency factor in memory is easily explained. If sufficient time elapses both B and P in Fig. 3 will shrink and the association will probably be weakened.

VI. Learning and Surface Tension

The superficial layer of all biological cells is in a so-called state of tension, and is said to exert a pressure on the substances within the cell. The surface tension[34] tends to diminish the surface, and to compress the substances within the cell. Many changes in the form of living substances are due ultimately to these forces.

Strictly speaking, osmotic pressure is manifested completely only in the case of semi-permeable membranes, and it may be counter-balanced by swelling in some cases. The cells of the organism retain their general form; and the cell content is more subject to swelling than the surrounding membrane. A diminished surface tension at any point on the bounding surface of the nerve cell will cause a bulging at this point. "It must be remembered that the passage of a current through a membrane impermeable to one or other ion in the surrounding fluid will cause an accumulation of the ion at the surface of the membrane, so that this will become polarized. Such an accumulation at any surface will naturally alter the properties of the surface including its surface tension.[35] The construction of the capillary electrometer depends on this fact." Surface tension is very sensitive to a variety of factors; slight traces of various substances, especially colloids, exert a marked influence upon it.

An increase in concentration on the surface film of a true solution (*i. e.*, adsorption) seems to be accompanied by a lowering of the surface tension (Freundlich).[36] Bancroft, in discussing a theory of peptization, writes that "we find experimentally that certain solutions will peptize or disintegrate certain precipitates so that we get colloidal solutions. This is always the result of adsorption. Theoretically there are three possibilities. If an adsorbed film has a low surface tension on the water side and a high one on the other side, it will tend to scrunch up and to peptize the solid as internal phase. If the reverse is the case, the solid will tend to form the external phase. If the two surface tensions are practically equal, neither will prevail. . .If we adopt Freundlich's view that adsorption always

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[33]Cf. C. M. Child, *Senescence and Rejuvenescence*, 1915, 283-284; S. Paton, *Human Behavior*, 1922, 311-313.

[34]See E. W. Washburn, *An Introduction to the Principles of Physical Chemistry*, 1915, 42; H. Bechhold, *op. cit.*, 1919, 13-40; D. W. Thompson, *On Growth and Form* 1917, 205-209.

[35]Starling, *op. cit.*, 172.

[36]Cf. D. W. Thompson, *op. cit.*, 277-281.

lowers the surface tension, a theory of peptization follows at once. We may have peptization by a liquid, by a non-electrolyte, by an adsorbed ion, by a salt, or by a peptized colloid. When a liquid is adsorbed by a solid, it will tend to peptize it and in some cases will do so."[37] Electrolytes which have a readily adsorbed *anion* lower the surface tension of the water phase in colloidal solutions, and increase the permeability of the system to water, but electrolytes which have a readily adsorbed *cation* produce the reverse effect.[38]

In the situation represented by Fig. 3, there is probably a slight compression of membrane *b*, resulting from the presence of the electrical double layer. Although very slight, this pressure according to Lillie is sufficient to alter the permeability. Dissolved substances have different effects on surface tension. In general, salts and strong bases in the solution *increase* the surface tension, whereas nitric, hydrochloric, and hydrobromic acid *decrease* it.

It is clear from the above considerations that in the situation represented in Fig. 3 there will be a *decrease* in surface tension at *b*, and probably also at *p*. The decrease in surface tension may be due to a polarization of the membrane (or to an increase in concentration on the surface film), or to a variety of dissolved substances in solution. This decrease in surface tension will cause a bulging at the point affected, most probably at *b*. With *p* and *b* closer together, a situation will result which is more favorable for the passage of an impulse from B to P.

The relation between surface tension and osmotic phenomena is so complicated and involved that we cannot enter into a discussion of this question.[39] Although there are many things about surface tension and osmotic phenomena which are but poorly understood, all of the general properties which we have discussed are agreed upon fairly well by specialists in this field, and they show a relation between surface tension and the association process.

VII. The Complete Process in Association

We may now attempt to visualize the situation represented by Fig. 3 in *three dimensions*. There are *numerous* points of contact between L and P, since the light-stimulus changed the size of the pupil at all times during the conditioned-reaction experiment; there are probably *several* points of contact between B and S, because of the "secondary reactions" called out; and a *few* between B and P, since conditioned reactions were obtained. The ease with which an association can be formed in this situation will depend very largely upon the number and

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[37]W. D. Bancroft, *Applied Colloid Chemistry: General Theory*, 1920, 166-167.

[38]Cf. Bechhold, *op. cit.*, 38-40.

[39]Refer to A. Findlay, *Osmotic Pressure*, 1919.

arrangement of these neural elements. There may be several different combinations of the neural elements represented by the Fig. Millions of neurones are involved even in some of the simplest movements of the organism; and we have a large number of possibilities to consider. This situation involves not only the fairly well-defined processes which we have described, but other factors also which, although present and doubtless active in the association process, do not invalidate the definite factors which we have described, but merely complicate them to some extent. For example, the nervous elements and processes are influenced by the substances carried in the lymph and the blood, by temperature, etc. The blood supply of nerves, however, is very slight.

It is quite possible that some overt movement which we are attempting to associate with a given stimulus will not take place although a few thousand systems or situations represented by Fig. 3 are operating as described. In the case of the eyelid response used by the writer in some experiments,[40] it is clear that an eyelid reaction is *very large* when compared with the unit-situation which we have been considering. In some cases overt movements were not obtained in our experiment. To explain these satisfactorily we have furthermore to take account of the "all-or-none" character of neurophysiological mechanisms. In such an experiment, some tendency is doubtless left in the nervous system, even though the reaction in question is not evoked; and the nervous system is probably always affected whether a *visible movement* takes place or not. Failure to obtain a conditioned knee-jerk reaction is no proof that a tendency in this direction has not been left in the nervous system.

Some psychologists may have difficulty in harmonizing these external aspects of association and memory. Why, for example, is it easy to learn a person's name but hard to establish a conditioned knee-jerk reaction? We believe that if sufficient data were at hand in regard to the operation of the physical factors involved this difficulty would disappear. Considerable help can be obtained if, when considering any phase of the association process, we consider especially the following factors: (1) the *physical mass* to be moved, (2) the extent to which the reaction is *partly learned* already, (3) whether or not the new movement *opposes* reaction-tendencies which are already well established, (4) the "all-or-none" character of neurophysiological mechanisms,[41] and (5) the structural relations of various

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[40]The Conditioned Eyelid Reaction, *J. Exper. Psychol.*, 5, [illegible]; A Note on the Conditioned Eyelid Reaction, *ibid.*, 6, 1923, 82-83.

[41]Evidence favoring the all-or-none law has been accumulating steadily during the past few years. See E. D. Adrian and A. Forbes, The All-or-Nothing Response of Sensory Nerve Fibres, *J. of Physiol.*, 56, 1922, 301-329.

elements in the nervous system. If all of these factors are considered, we should have no difficulty even now in understanding most of the important phenomena of association and memory.

VIII. Summary

We have now completed our survey of the physical factors involved in forming a connection between B and P in Fig. 3. We have attempted simply to outline the various factors which are at work in this system, and we have gone into detail only where the facts seemed to justify it. All that we can say in regard to a few of these factors is that they are very probably present and operating, but most of the factors described seem to be present and operating beyond reasonable doubt. It has been necessary to make comparatively few assumptions which are not well supported by scientific evidence. The complete picture sketched represents a *complex* situation, and we are convinced that future investigators of this problem should avoid looking for any clear-cut, simple, or *neat* explanation of association and learning,—which has been the custom in the past.[42]

We have attempted to show how the action of the system represented by Fig. 3 causes a change in the structure of membrane *b*, which in turn produces a situation more favorable for the passage of a nervous impulse from B to P. This orientation of the molecules of membrane *b* is probably only temporary in nature, but partly accounts for certain temporary changes in reaction-tendencies which are commonly observed.

There is some reason for believing that B and P (in Fig. 3) may move toward each other when the system is active. Such a gross movement of nervous elements affords a partial explanation of the rapid formation of some associations; and considerations regarding such movements have a bearing on the effect of shock on loss of memory.

Facts gathered from colloid chemistry establish a definite relation between the action of the system represented by Fig. 3 and certain swelling phenomena in the nerve cell. The action of the system results in a swelling in P, around *b*, which is due to certain osmotic phenomena in this region and to the effect of an increase in hydrogen ion concentration on the swelling phenomena in the cell. This swelling, and the increase in the conductance of electric currents in the region of *b*, result in a situation which is more favorable for the passage of an impulse from B to P. Other chemical considerations show how these changes may have a more or less permanent character. We

[42]Cf. H. Cason, The Concept of Backward Association, this JOURNAL, 35, 1924, 217-221; Criticisms of the Laws of Exercise and Effect, *Psychol. Rev.*, 31, 1924, 397-417.

have apparently worked out a partial explanation for many of the observed differences in impressibility and retentiveness of various individuals. The facts which have to do with the ageing of colloids harmonize well with the external aspects of memory. The action of the system decreases the surface tension at *b*, causes a bulging at this point, and thereby assists in the formation of a connection between B and P.

Throughout the above discussion, we have emphasized the combined action of *all four factors*, arbitrarily grouped: the change in structure of membrane *b*, the gross movement of B and P toward each other, the rearrangement of substances within cell body P, and the effect of a change in surface tension at *b*. These partial explanations should not be considered as exclusive or alternative explanations. We have attempted to emphasize the combined action of the whole system, rather than the action of any single factor which cannot function alone.

In conclusion, it should be stated that we have only attempted to treat a very limited phase of the learning problem, *i. e.*, the part which is related particularly to nerve physiology, physics, and chemistry. Learning and habit, however, cannot be adequately expressed *merely* as a function of electrical attractions and repulsions. The logical conclusion of such a crude mechanical view would be as materialistic as it is incomplete, and would deny all human characteristics with which we are personally acquainted. The *biological* point of view places too much emphasis on physiological mechanics and native factors, and is somewhat limited and narrow in its scope. It exaggerates the points of similarity between the meagre learning capacities of the brutes and those of man. Man's knowledge, skill, character, temperament, customs and institutions, however, bear no resemblance whatever to those of a lower animal. Unjustified emphasis on the structure and function of man considered from the narrow biological point of view has frequently led to the most absurd conclusions. It is necessary to take proper account of our central core of conscious reactions, our memories, motives, hopes, fears, and so on. The complete picture will have to include social considerations: the response should be considered in immediate relation with the stimulus. Only by considering the environmental influences can we form anything like an adequate conception of learning and habit-formation, and only in this way can we understand how man through long periods of time has acquired the varied characteristics which he now possesses.

ATONIC ENDINGS IN MELODIES[1]

By Paul R. Farnsworth

Introduction

The tonic effect[2] is a well-known fact of musical aesthetics. As an example of this, consider a melody composed only of the notes c and g. The vibration-frequency relationship here is as 2 to 3. The majority of listeners to this simple melody will prefer its ending on c rather than g,—that is, they will prefer ending on the ratio 2, the tonic. On the other hand, when c and f (frequency 3 to 4) constitute the melody, the ending-preference will now be f. This again shows the preference for ending on 2 (in this case 4 with the powers of 2 extracted).

With the exception of Schoenberg and other ultra-moderns, composers have adhered quite generally to this principle of closing a piece on its key-note which is generally also the tonic. In the key of c, we end on c; in the key of g, on g, etc. A feeling of rest or finality is conceded to accompany this ending. This decided preference for ending on the pure power of 2 has been shown experimentally by Meyer.[3] According to his results, the ratio symbol 2 was preferred over 3 as an ending in 86 instances, and 3 was preferred over 2 in only 26 instances; or, in terms of percent., the ratio symbol 2 was preferred over 3 as an ending in the proportion 77 : 23.

In a composition in which the pure power of 2 does not exist, it is supposed that one ending exhibits as much finality as any other. As it seemed to the writer that this assumption has been accepted without much actual investigation, the following experimental work was undertaken.

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[1]From the Department of Psychology of Ohio State University.

[2]M. F. Meyer (*Psych. Rev.*, 7, 1900, 247) states the general law as follows: "One of the tones being a pure power of 2, we wish to have this tone at the end of our succession of related tones, our melody."

As this notation may be new to some readers, let me again quote from the same article (252). "All pure powers of 2, as 1, 2, 4, 8, 16, 32, etc., will be represented equally by 2. All composite numbers containing a power of 2 will be represented without the power of 2. So I always shall write simply 5 instead of any of the numbers 5, 10, 20, 40, etc., 9 instead of 9, 18, 36, 72, etc."

These ratios with the pure powers of 2 extracted will be designated in this paper as *ratio symbols*.

[3]M. F. Meyer, this Journal, 14, 1903, 459.

Experimentation

Three tones, g, e and b♭ (key of c) were chosen as representing the ratio symbols 3, 5, and 7, in which the pure power of 2 is absent. The corresponding musical notation, vibration-ratios, and actual vibration-rates, are here listed.

Musical Notation	Ratio Symbol	Vib. Ratio	Vib. Rate
g,	3	6	96.89 d.v.
g′	3	12	193.77 d.v.
e′	5	10	162.94 d.v.
b♭,	7	7	115.22 d.v.
b♭′	7	14	230.43 d.v.

These tones were played successively on a reed-organ. The three tones were so arranged that in one group g (3) was the last heard[4], in a second group e (5) was the last, and in a third group b♭ (7) was the final tone given. The groups were sounded in a random order, and were judged by the method of paired comparisons, as is shown in the following experimental programme.

Experimental Programme

Three groups of students in elementary psychology acted as *Ss*.

Test A	58 Students
Test B	62 Students
Test C	62 Students (same group as B)

Thirty judgments were made by every student, as follows.

- Ten judgments with 3 and 5 paired
  - Five judgments with 3 first and 5 last
  - Five judgments with 5 first and 3 last
- Ten judgments with 3 and 7 paired
  - Five judgments with 3 first and 7 last
  - Five judgments with 7 first and 3 last
- Ten judgments with 5 and 7 paired
  - Five judgments with 5 first and 7 last
  - Five judgments with 7 first and 5 last

The instructions were as follows: "You will hear two series of three tones each which you are to compare. Which series sounds more finished, more complete, more at rest? If it is the first, mark 'one' on your paper; if it is the second, mark 'two'". The instructions were repeated so that every *S* understood what was expected of him.

Tests A and B

In Tests A and B, the tones were heard with a falling inflection. The three different ending-possibilities are listed under the titles, Successions I, II, and III. Succession I was paired with II, II with III, and I with III.

[4]In this article, the number in parenthesis in the text will refer to the ratiosymbol.

As an example, let us take the case in which Succession II (b♭ g e) was paired with Succession III (g e b♭). In the first case, b♭ was the highest and e the lowest note in the succession, in the second case g was the highest and b♭ the lowest. In other words, the comparison here was between ending on e (5) and on b♭ (7). The same experiment was given to two groups,—58 in Test A and 62 in Test B.

Ratio Symbol				Vib. Ratio in Time-Order Presented 1st	2nd	3rd
			Succession I	e	b♭	g
5	7	3		10	7	6
			Succession II	b♭	g	e
7	3	5		14	12	10
			Succession III	g	e	b♭
3	5	7		12	10	7

The results for both groups are given in Table I. Here "Pref. 3" refers to the preferences for ending the melody on ratio symbol 3, and "Pref. 5" for ending on ratio symbol 5. The results are expressed in percentages.

TABLE I

Percentage Preferences for Succession I {10, 7, 6 / e, b♭, g} paired with II {14, 12, 10 / b♭, g, e}

Order presented		Pref. 3	Test	Pref. 5
1st. 10, 7, 6		60	A	40
2nd. 14, 12, 10		49	B	51
1st. 14, 12, 10		60	A	40
2nd. 10, 7, 6		53	B	47
	Averages	60	A	40
		51	B	49
		56	A and B	44

Conclusion: when ratio symbol 3 is paired with 5, symbol 3 is preferred as an ending in the proportion 56 : 44.

Percentage Preferences for Succession I {10, 7, 6 / e, b♭, g} paired with III {12, 10, 7 / g, e, b♭}

Order presented		Pref. 3	Test	Pref. 7
1st. 10, 7, 6		60	A	40
2nd. 12, 10, 7		76	B	24
1st. 12, 10, 7		73	A	27
2nd. 10, 7, 6		64	B	36
	Averages	67	A	33
		70	B	30
		68	A and B	32

Conclusion: when ratio symbol 3 is paired with 7, symbol 3 is preferred as an ending in the proportion 68 : 32.

Percentage Preferences for Succession

II {14, 12, 10 / b♭, g, e}		paired with	III {12, 10, 7 / g, e, b♭}
Order presented	Pref. 5	Test	Pref. 7
1st. 14, 12, 10	60	A	40
2nd. 12, 10, 7	64	B	36
1st. 12, 10, 7	46	A	54
2nd. 14, 12, 10	56	B	44
Averages	53	A	47
	60	B	40
	56	A and B	44

Conclusion: when ratio symbol 5 is paired with 7, symbol 5 is preferred as an ending in the proportion 56 : 44.

Total Percentage of Preferences for Successions I, II, and III

	Pref. 3 I	Pref. 5 II	Pref. 7 III
Test A			
Men	42	30	28
Women	43	32	25
Total	42	31	27
Test B			
Men	42	38	20
Women	38	35	27
Total	40	37	23
Average for both tests	41	34	25

Conclusion: the ratio symbols 3, 5, and 7 respectively, are preferred as endings in the proportions 41 : 34 : 25.

Test C

It so happened that g (3), the most preferred note, was also the lowest note. To eliminate this effect of falling inflection, a second combination was arranged. The triad was here sounded in the following order,—first the high note, then the low note, and last the middle note.

Ratio Symbol				Vib. Ratio in Time-Order Presented 1st	2nd	3rd
			Succession IV	b♭	e	g
7	5	3		14	10	12
			Succession V	g	b♭	e
3	7	5		12	7	10
			Succession VI	e	g	b♭
5	3	7		10	6	7

Thus where the succession g b♭ e was paired with e g b♭, g was the high and b♭ the low in the former case, and e was the high and g the low in the latter.

TABLE II

Percentage Preferences for Succession IV {14, 10, 12 / b♭, e, g} paired with V {12, 7, 10 / g, b♭, e}

Order presented		Pref. 3	Pref. 5
1st. 14, 10, 12; 2nd. 12, 7, 10		51	49
1st. 12, 7, 10; 2nd. 14, 10, 12		56	44
	Average	54	46

Conclusion: when ratio symbol 3 is paired with 5, symbol 3 is preferred as an ending in the proportion 54 : 46.

Percentage Preferences for Succession IV {14, 10, 12 / b♭, e, g} paired with VI {10, 6, 7 / e, g, b♭}

Order Presented		Pref. 3	Pref. 7
1st. 14, 10, 12; 2nd. 10, 6, 7		62	38
1st. 10, 6, 7; 2nd. 14, 10, 12		62	38
	Average	62	38

Conclusion: when ratio symbol 3 is paired with 7, symbol 3 is preferred as an ending in the proportion 62 : 38.

Percentage Preferences for Succession V {12, 7, 10 / g, b♭, e} paired with VI {10, 6, 7 / e, g, b♭}

Order presented		Pref. 5	Pref. 7
1st. 12, 7, 10; 2nd. 10, 6, 7		57	43
1st. 10, 6, 7; 2nd. 12, 7, 10		61	39
	Average	59	41

Conclusion: when ratio symbol 5 is paired with 7, symbol 5 is preferred as an ending in the proportion 59 : 41.

Total Percentage Preferences for Successions IV, V, and VI
Test C

	Pref. 3 IV	Pref. 5 V	Pref. 7 VI
Men	42	37	21
Women	35	33	32
Total	39	35	26

Conclusion: the ratio symbols 3, 5 and 7 respectively, are preferred as endings in the proportions 39 : 35 : 26.

Averages

Averages have been computed for all three groups, and are shown in Table III. Here are contrasted the possible melody-endings,—on vibration-ratios 6 or 12 (g, ratio symbol 3); on vibration-ratio 10 (e, ratio symbol 5); and on vibration-ratio 7 (b♭, ratio symbol 7).

TABLE III

Averages for Tests A, B, and C

Ending on 6 or 12 (3)	paired with Pref. 3	Ending on 10 (5) Pref. 5
1st. 3, 2nd. 5	53	47
1st. 5, 2nd. 3	57	43
Average	55	45

Conclusion: when ratio symbol 3 is paired with 5, symbol 3 is preferred as an ending in the proportion 55 : 45.

Ending on 6 or 12 (3)	paired with Pref. 3	Ending on 7 (7) Pref. 7
1st. 3, 2nd. 7	66	34
1st. 7, 2nd. 3	66	34
Average	66	34

Conclusion: when ratio symbol 3 is paired with 7, symbol 3 is preferred as an ending in the proportion 66 : 34.

Ending on 10 (5)	paired with Pref. 5	Ending on 7 (7) Pref. 7
1st. 5, 2nd. 7	60	40
1st. 7, 2nd. 5	54	46
Average	57	43

Conclusion: when ratio symbol 5 is paired with 7, symbol 5 is preferred as an ending in the proportion 57 : 43.

Total Percentage Preferences for 3, 5, 7
Average for All Tests

	Pref. 3	Pref. 5	Pref. 7
Women	39	33	28
Total	41	34	25

Conclusion: the ratio symbols 3, 5 and 7 are preferred as endings in the proportions 41 : 34 : 25.

Summary

(1) When ratio symbol 3 is paired with 5, the difference in the ending-preference is small; but 3 is favored, in the proportion 55 : 45.

(2) When ratio symbol 5 is paired with 7, the difference in the ending-preference is small; but 5 is favored, in the proportion 57 : 43.

(3) When ratio symbol 3 is paired with 7, the difference in the ending-preference is quite marked, and 3 is favored, in the proportion 66 : 34.

(4) When compared with ratio symbols 5 and 7, 3 apparently gives more finality than 5, and 5 more than 7, in the proportion: symbol 3, 41%; symbol 5, 34%; and symbol 7, 25%.

(5) Including the results of Meyer on symbol 2, and reducing his figures to data comparable with the results of these experiments, the proportions would be: symbol 2, 58%; symbol 3, 17%; symbol 5, 14%; and symbol 7, 11%.

(6) There seems to be little sex-difference in the ending-preferences.

THE CONTROLLED *VS.* THE FREE COMPLETION

By Rose S. Malmud, Teachers College, Columbia University

It is the purpose of the writer to present briefly for comparison the type of language completion standardized by Trabue (introduced by Ebbinghaus)[1] and the selective or controlled completion. The graded language completion scale is recognized as a measuring instrument. Mere recognition, however, does not establish its verity, and the questions persist: (1) what does the instrument measure? and (2) how perfect an instrument is it? To answer that it measures language ability is no explanation. For what is language ability, as measured by the completion, and what makes it an index of general intelligence? Clearly it is not mere word knowledge. Sentence completion deals with words not in their isolation but in their connectedness, their refined interrelation and juxtaposition. As Lafcadio Hearn has picturesquely said, "Words are very much like lizards; they change color according to position."

In this connection the classical terminology of association will be abandoned as unfruitful in favor of a terminology of habit formation. The unit of the sentence is interpreted not as the word but as the word-habit. Thus, a word in its relation to other words may be univalent, bivalent, trivalent, or multivalent according as it coordinates or integrates into a larger unit with one or two or more elements in a given sentence. A simple sentence represents a simple integration; a complex sentence, a hierarchy of minor integrations or word-habits. A boy who writes "heart" in the first blank of the completion "One's...... in life........upon so..........factors that it is not........ to state any single............for failure" illustrates the use of a univalent word. The integration was too complex for the boy, and he responded to the only word for which an habitual response was available. Or similarly, when another individual responds to the word "friend" by writing "old" in the third blank of the completion "........ that are .... to one by an .... friend should be pardoned .... readily than injuries done by one ...... is not angry." On the other hand, two discrete integrations may take place; as when a child in response to the incomplete sentence "It is very annoying to .... toothache, ... often comes at the most .... time imaginable," writes "It is very annoying to have a toothache, rain often comes at the most unexpected time imaginable." We may therefore say with some truth that

[1] M. R. Trabue, *Completion-Test Language Scales*, New York City, Teachers College, Columbia University, 1916; H. Ebbinghaus, *Z. f. Psych.*, 13, 1897, 401-459.

a sentence completion measures the extent to which certain hierarchies of word-habits have been formed.

The free completion has been called the creative completion. This description will bear critical examination. The writer, acting as S in L. Hunseker's study, at the Institute of Educational Research, Teachers College, Columbia University, of levels of difficulty as related to speed, had the opportunity of observing her procedure in completing sentences. Two types of difficulty were noted: (1) the difficulty that arose from a great number of omissions which called for complex integration; (2) the difficulty that was the result of lack of familiarity with the implied context of the sentence, *i. e.*, a situation for which no habitual integration was forthcoming. For example, two incomplete sentences may be cited, both of which had an equal number of omissions, but to which the responses were different. (1) "Ibsen in the social .... in .... ways has struck the highest .... of modern dramatic ...." (2) "Throughout the river plains of northern India, two harvests, and .... some provinces .... are .... each ...." To the first the response was that of immediate recognition. It was as if the blanks were not there, and the writer responded to it as a co-ordinated whole, writing the omitted words *in order*. To the second, however, the response was that of a feeling of strangeness, a complete lack of familiarity. The coordination had to be built up from discrete word-habits, a process which may be presented in the form of a verbal graph as follows, indicating the order in which the responses were made and discrete word-habits were integrated:

(1) Harvests are ....

(2) Harvests are .... each ....

(3) Two harvests, and .... some provinces, are ...... each year

(4) Two harvests, and .... some provinces .... are .... each year.

(5) Throughout the river plains of northern India, two harvests, ... in some provinces .... are ...... each ....

This response, or rather series of responses, had all the feeling tone of creative thought: that of unfamiliarity, persistence in spite of unhabitual conditions, attentive examination of all the elements, trial procedure from the known to the unknown, selection of multivalent words—habitual qualifications of time, place, number,—made with a view to possible integrations. Apparent success brought all the elation attending a fresh and individual discovery. Compare the time of 36.8 sec. taken by the first response with 243.8 sec. required by the second. This difference represents the time it took for the piecemeal building of discrete habits into a new integration. It is this type of com-

pletion which, in the opinion of the writer at least, is significant as evidence not so much of language ability as of general intelligence.

The question may well be raised as to the relation of integrations already formed to intelligence. Creative thought as here understood is itself a complicated habit or integration of persistent search, attentive examination, and trial solving. It is the direct measure of this habit therefore that should be of interest, and not its indirect and uncertain manifestations. It is conceivable that if a man who was a stoker because he could be nothing else were placed at one end of a scale, and Newton were placed at the other, with (say) the writer half-way between—it is conceivable that both the stoker and the writer could respond better to a battery of up-to-date information tests so prominent in intelligence measurement than could Newton. Our knowledges and verbal habits are not necessarily or frequently the result of feelings of inadequacy, attitudes of persistent searching, and trial integrations. All this has been short-circuited for us and neatly classified as pre-formed truths. To make our meaning more apparent,—one of Newton's earlier discoveries was the composition of white light. It appears, however, that he assumed that all prisms produced spectra of equal lengths. Any college freshman who had taken a course in physics could tell him something of the varying dispersiveness of varying refractive media. But it was Newton's type of ability that made other investigations, and verifications, and the freshman's erudition possible. Similarly, any modern student of medicine could cite anatomical truths which would be a revelation to Vesalius, but it was Vesalius' habits of original thought, his questionings of the revered oracular utterances of a Galen, which made those very truths an inevitable development. It is possible, further, for a modern farmer to add a cubit to the height of his fruit trees. But it was a Mendel who took thought. All this is by way of pointing out that it is not mere knowledges, ready habits, but the integration of these into unhabitual patterns and new productive combinations, the forging-ahead aspect of intelligence, which is of significance. In the free completion an original integration is sometimes actually penalized. In the I. E. R. completions, for example, children whose geographical training has not been adequately provided for will respond to "A body of .... entirely surrounded by .... is called an ...." by "A body of [illegible] entirely surrounded by people is called an [illegible]," or "A body of man entirely surrounded by police is called an thief," or "A body of man entirely surrounded by earth is called an grave,"- -and will receive no credit.

These conditions lead up to and in part answer our second question: how perfect a measuring instrument is the free completion? An obvious defect is the part that individual judgment in the scoring plays. What would be our reaction to a difference in the reading of a thermometer at a given time by doctors at a sick bed? Assuming that we know what is being measured, the personal equation determines the reading, and makes indefinite the status of the free completion as a measuring instrument.

The controlled completion, we believe, is an improved instrument in what it measures and in the accuracy with which it measures it. It is a better measure of language ability and a finer index of general intelligence. As a language scale it may be more accurately graded. Nuances of meaning may be tested to an extent not possible with the free completion. Thus in the completion "Many people .... their health because ...... do not .... the .... of hygiene," equal credit according to the Trabue key is given to the responses "observe" and "understand." The assumption may be that temporary interference, emotional inhibitions, or what not, may prevent the child from responding with "observe," and that the two words are nearly enough synonymous to give them equal weight. In the controlled completion, however, temporary interference is neutralized, and the selection is equated. It is then possible to give more credit for the use of "observe," which is more discriminating and accurate. A more refined adjustment to the verbal situation is called for than is possible in the free completion. The habitual, unreflective, glib response is not always adequate as it is in the free completion. The controlled completion calls for perception of the difficulty, attentive examination of all the elements, selection of uni-, bi-, or trivalent words as the case may be for possible integrations, the result of which will be a coordinated whole or disjointed parts. The whole controlled completion represents a series of conflicting word-habits only one or two of which fulfill the conditions of the sentence. It does not depend for its scoring on subjective judgment. Inasmuch as it is not subject to the variation of individual opinion, it is more exact. Whatever errors are present are constant and inherent in its objectivity. Incidentally, where many individuals have to be tested, as in large schools or offices where time and labor saving devices are a desideratum, the controlled completion is to be commended. There is no need for complicated keys, subtle interpretations, or harassing wonder at what the individual meant or whether he meant anything at all.

The experimental data available are not as adequate as one would like. They are subject to errors of sampling, insufficient

cases for any one form, lack of equation of conditions at different schools, and inequality of criteria. The experimental procedure may be briefly described as follows.

The 8 forms that make up the Trabue Language Scale (forms B, C, D, E for the elementary grades, and J, K, L, M for high schools) were cast into the controlled form of completion (herewith appended). The number of words offered for selection in each case bears a constant relation to the number of words omitted and the words are arranged in alphabetical order. On the assumption that forms B, C, D and E are practically equivalent, each group tested was given one of the Trabue forms and an alternate form of the controlled completion (*e. g.*, Trabue Form B followed by Malmud-Trabue Form C or, with a succeeding group, Malmud-Trabue Form D followed by Trabue Form E, to neutralize any possible advantage of position). According to Trabue, the language ability of a child cannot be adequately measured with less than 3 of the forms (there are at most 10 graded completions in any one form). Our purpose, however, even if time permitted, was not primarily to measure the language ability of any one individual, but to compare two types of completions. One form of both types was therefore given in most cases. Whatever error this procedure introduces is present for both types of completion. The tests were tried between the months of February and June of 1924 in schools where a general intelligence or achievement test had been given which might be used as a criterion. The directions given for the Trabue Forms were: "Write one word on each inch of dots so as to make the whole sentence true and sensible"; for the Malmud-Trabue forms: "Write one word on each inch of dots so as to make the whole sentence true and sensible. Choose the word from those which are printed at the end of the sentence." One minute longer was allowed for each of the Malmud-Trabue forms than for the Trabue (*i. e.*, 8 min. for B, C, D or E, and 6 for J[2], K, L or M).

The results are tabulated on page 406.

Where feasible, schools which had been given identical intelligence or achievement tests within the same period of time were grouped together. However, Public School 42 and Public Schools 28 and 189, all of which had been given an intelligence test by the Institute of Educational Research, showed very little overlapping in the intelligence scores, although there was considerable overlapping of language completion scores. It was therefore deemed advisable to separate the schools as representing different types of groups. In Public Schools 28 and 189, representing fairly similar groups, three criteria were available: the National Intelligence Test, the Institute of Educational Research Intelligence Test and the Stanford Achievement Test. The groups within the schools taking these tests, however, were not identical: the groups which were given the I. E. R. Tests represent only 6B and 7A classes; the groups which were given the National Intelligence Test and the Stanford Achievement Test represent classes from the 4A to the 8A; classes which overlapped but were not identical. At Public

[2]Because of an error, only 6 of the sentences instead of the 7 in Form J of the Malmud-Trabue completions were made available.

School	Trabue Completion Form	Trabue Completion n[1]		Trabue Completion Pearson r[2] PE[3]	Criterion	Malmud-Trabue Completon Form	Malmud-Trabue Completon n[1]		Malmud-Trabue Completon Pearson r[2] PE[3]
P.S. 28 Manhattan	B	85		.48 ± .06	National	B	76		.34 ± .07
P.S. 189 "	C	78		.53 ± .05	Intelligence	C	109		.35 ± .06
	D	50		.59 ± .06	Test 1923	D	130		.54 ± .04
	E	134		.45 ± .05		E	50		.57 ± .06
P.S. 42 Bronx	B	91		.55 ± .05	National	B	186		.63 ± .03
P.S. 144 Brooklyn	C	198		.58 ± .03	1924	C	96		.55 ± .05
	D	105		.46 ± .05		D	141		.44 ± .05
	E	87		.41 ± .06		E	133		.60 ± .04
P.S. 28 Manhattan	B	49		.90 ± .02	Institute of	B	46		.38 ± .09
P.S. 189 "	C	47		.40 ± .08	Educational	C	51		.51 ± .07
	D	49		.60 ± .06	Research Tests	D	106		.63 ± .04
	E	108		.58 ± .04	of Intelligence	E	49		.48 ± .07
P.S. 42 Bronx	B	48		.62 ± .06	1923	B	58		.71 ± .04
	C	67		.55 ± .06	" "	C	52		.56 ± .06
	D	72		.29 ± .07		D	77		.55 ± .05
	E	74		.56 ± .05		E	69		.44 ± .07
P.S. 28 Manhattan	B	80		.49 ± .06	Stanford	B	77		.35 ± .07
P.S. 189 "	C	79		.47 ± .06	Achievement	C	83		.34 ± .07
	D	50		.50 ± .07	Tests 1923	D	126		.51 ± .04
	E	131		.39 ± .05		E	50		.56 ± .07
Junior High School	J	82	(*r* Unlike-	.43 ± .06	Otis Index	J	72	(*r* Unlike-	.48 ± .06
P.S. 178 Brooklyn	K	72	Signed Prs.)	.43 ± .06	of Brightness	K	82	Signed Pairs)	.34 ± .07
	L	85		.56 ± .05		L	100		.48 ± .05
	M	100		.61 ± .04		M	85		.73 ± .03
Speyer Junior	{L	92	Pearson *r*	.47 ± .05	National 1923	{J	92	Pearson *r*	.43 ± .06
High School	M}					K}			
Graduate Group	{J	19	" "	.65 ± .09	Thorndike	{L	19	" "	.67 ± .09
in Teachers College	K}				Intelligence	M}			
Columbia Univ.					Test for High				
					School Graduates				
					(Forms IE, IIE,				
					IIIAAA)				

[1] = Number of cases
[2] = Correlation with the criterion
[3] = Probable error according to the PE_r table of H. A. Toops and Z. F. Miner, *Journal Educ. Research*, Jan., 1924

School 178, Brooklyn (Junior High School), raw scores made on the Otis Intelligence Test were available, but unfortunately for our purposes some pupils had been given the Otis Test in 1923 and others in 1924. However, an index of brightness had been determined for each pupil. Since the index of brightness was found to correlate .84 with the raw score, the correlation was thought good enough to permit the substitution of the index of brightness for the raw score. To ascertain approximate correlation in this case, therefore, the method of unlike signed pairs was used. The graduate group mentioned in the Table represented a class in mental measurements which had been given the Thorndike Intelligence Test for high school graduates, a fact which the writer was given the privilege of capitalizing for correlation purposes.

This investigation was made possible through the kindness of Professor E. L. Thorndike, who, as head of the Institute of Educational Research, provided the testing material.

Form B

1. We like good boys ...... girls.	along and are ask for said the
2. The ...... is barking at the cat.	angry bear cat child dog good hen
3. The stars and the .... will shine to-night.	clouds dipper moon Orion shoes sky sun
4. Time ...... often more valuable .... money.	also are because for is lost than though pocket with
5. The poor baby ...... as if it were .... sick.	being coos colic is laughs looks pale teething very was
6. She .... if she will.	came can does goes has is might
7. Brothers and sisters ...... always ...... to help .... other and should .... quarrel.	along also always around angrily but each have kind loudly never out should some sometimes try
8. .... weather usually .... a good effect ...... one's spirits.	bad brews but gives has if in is not on pleasant poor then to vane
9. It is very annoying to .... toothache .... often comes at the most .... time imaginable	a and bad best boast but dentist enjoy happy have inconvenient lucky pains therefore which worst
10. To .... friends is always .... the .... it takes.	best betray blame equal false given gifts good longer make our these treachery trouble worth

Form C

1. The sky ...... blue.	are dark deep is light rocket very
2. Men ...... older than boys.	all are be is not see women

3. Good boys......kind...... their sisters.	and are by for girls men not of to words
4. The girl fell and ...... her head.	against cried hurt on over stumbled to
5. The......rises......the morning and ...... at night.	also always bird bread earliest every from in man sets shines sometimes sun therefore with
6. The boy who .... hard ...... do well.	chews candy did enough example made never to will works
7. Men........more........ to do heavy work ...... women.	are brains complain do except for have lack like made money strength than thinking without
8. The sun is so ...... that one cannot .... directly .... causing great discomfort to the eyes.	and anything at big bright distant high it look low measure see setting therefore without
9. The knowledge of .... use fire is ...... of...... important things known by ...... but unknown ...... animals.	all books careful cooking fireman for from how in man often one some the them to when who
10. One ought to .... great care to ...... the right ...... of ...... for one who .... bad habits .... it ...... to get away from them.	acquires bad dislikes easy exercise finds form habits hard impression kind never often oneself pays people shows side smoking thing wrong

Form D

1. We are going .... school.	at big evening high in public to
2. I ...... to school each day.	early go going have hate late like
3. The .... plays .... her dolls all day.	also ball child for house only tag to were with
4. The rude child does not .... many friends.	against apologize care displease has make need
5. Hard........makes........tired.	break candy lazy one sick teeth thing times very work
6. It is good to hear .... voice .... friend.	a and deaf dumb false hoarse loud nasal of poor singing speaking the to with
7. The happiest and........contented man is the one .... lives a busy and useful	also as bee least life most not pleasure poor things time too unhappy who whose
8. The best advice .... usually .... obtained .... one's parents.	against always be been can children except follows from is not therefore to with works

9. ....things are........satisfying to any ordinary .... than congenial friends.	all conditions few friend hardly horrid man more not often one quite the these unpleasant
10. .... a rule one .... association .... friends.	against although and as avoids dislikes enjoys in making not observing refuses scorns therefore with

Form E

1. I see you. Can you see ....?	eyes I look me not no yes
2. Ice is cold, but fire is	big bonfire burns cold hot out water
3. The kind lady .... the poor man a dollar.	advised flings friend gave helps refused said
4. The best .... to sleep is at night.	boy early go thing time way when
5. Children should .... many lessons from .... parents.	cruel do learn never obey other recite show their your
6. The child .... the river .... was drowned.	and at deep fell flowed goes into looked playing said saw so swiftly was who
7. It is a .... task to be kind to every beggar .... for money.	always and asks blind childish daily easy except extorts hard not poor refuses who works
8. Worry .... never improved a situation, but has .... made conditions	bearable better can excellent good hardly has more must never not usually which will worse
9. When one feels drowsy and ...... it .... happens that he is .... to fix his attention very successfully .... anything.	able active alert always do energetic form has invariably must not on or seldom so tired
10. In order .... clearly at ...., it is .... to .... artificial	always coloring flowers foolish last light look manufacture night necessary one see something that think to use unnecessary

Form J

1. Boys and .... soon become ....and women.	big boys children everybody girls grown-up men old poor rich
2. The .... are often more contented .... the rich.	also because boys except girls men parents poor than to
3. The rose is a favorite ..... because of .... fragrance and	also and beauty [illegible] height its kindness not [illegible] scent smallness smell their thorns tree

4. It is very .... to become ....acquainted .... persons who .... timid.	are bad because except foolish friends hard if know like not speak unwise well with wrong
5. Extremely old........sometimes .... almost as .... care as	and attention because bestow exercise healthy infants little much need negligible people rags read say sleep
6. One's .... in life .... upon so .... factors .... it is not .... to state any single .... for .... failure.	because cause death depends easy exception expresses few hard health heart man many money much one's shows success that which woman
7. The future .... of the stars and the facts of .... history are .... now once for all, ... I like them .... not.	although and as astrology astronomy but course facts fixed ignored my or their unknown whether

Form K

1. The boy will .... his hand if .... plays with fire.	cut boys burn ever girls he holds likes use wet
2. Hot weather comes in the .... and .... weather .... the winter.	basement cold comes hot in makes south summer the then this through tropics warm winter
3. The poor little ...... has .... nothing to .... ; he is hungry.	and child cry dish do done dragon eat glutton had never play said wanted wear
4. Very few people .... how to spend time and .... to the best advantage.	also can get know money spend try use want work
5. One .... not, as a ...., ... attention .... uninteresting things.	among can distract does from general give has keep not patience rule thing thinks to usual
6. To eat .... one is .... is a .... pleasure.	and candy dinner full food great hungry nauseous not sad sick something what when with
7. .... they .... us...... not, nature's .... are ...... and unchangeable.	and animals arts cruelty even finished fixed give laws or please since struggles that whether

Form L

1. Children .... are rude...... not easily win friends.	also and but do perhaps sometimes then therefore to who
2. Plenty .... exercise and .... air .... healthy .... and girls.	bad boys children except food for fresh give makes need of sleep to wealthy wise with
3. In .... to maintain .... health, one should have nourishing	also always candy case clothing exercise food one's order poor sleep stead time vain warm

4. .... happiness cannot be .... with money.	borrowed bought contented good if no seen therefore true unless
5. One's .... do .... always express his thoughts.	also clothes good ideas not opinions reading sometimes therefore words
6. To .... to wait, after having .... to go ...., ... very annoying.	along bad have is it makes nowhere out patiently prepared refused said when wish with work
7. It is sometimes .... to .... between .... of action.	but children choose difficult do done go guns kinds said see sit speeches therefore win
8. One can .... do his .... at one .... while .... of another.	also always away best chores dishes doing easily hair not place quarreling seeing sometimes task thinking

Form M

1. One cannot foretell .... will happen in the	anything future history just past that time today what when
2. The dog .... a useful ...... because .... his intelligence and faithfulness.	although and animal barks does guards has is manner of tree trick watches when wood
3. Many people ....their health because .... do not .... the .... of hygiene.	amount best children disease guard have injure if observe protect rules see they things understand when
4. Nothing can .... one's happiness .... effectively than a guilty	as conscience convict criminal destroy enjoy less make man more one so sometimes tell win
5. To .... many things ...... ever finishing any of them ...... a .... habit.	also and bad begin complete fine good is splendid spoils their unless was were when without
6. The .... seems .... and dreary .... a discouraged	because bright cheerful dark discontent except happy in look man manner thing to voice with world
7. .... that are .... to one by an .... friend should be pardoned .... readily than injuries done by one .... is not angry.	and angry bestowed but done faithless favors generous injuries less more therefore unknown very who
8. It is .... that a full-grown man should .... a ghost .... [illegible]	and able become believe but destroy improbable [illegible] unless welcome when wrong

VISUAL PERCEPTION OF MOVEMENT

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In a previous investigation,[1] the perception of movement from two stationary stimuli and the phi-phenomenon proposed by Wertheimer as the extra-sensory basis of this perception have been critically examined; the compulsory conditions for their arousal have been established; and the reflection of these compulsory conditions in consciousness has been shown to "take the form of a primary integration of a visual quality (grey) with a duration which is characteristically brief." This grey flash is independent of the quality of the stimulus, and is directly determined by the temporal interval between the exposures of the two stimulus-members. In the present investigation we undertook to determine the differences between the perception set up by the presentation successively of two stationary stimuli and that set up by the presentation of an actually moving stimulus.

Procedure I

Our apparatus was a form of the Dodge tachistoscope similar to that used by Dimmick, to which we added a further modification to present an actually moving stimulus, which in every way duplicated the fields, positions, and time-intervals of the successive stationary stimulus.

This moving field was actuated as follows. A small wire cable was fastened to the top and bottom of the falling shutter and ran over pulleys above and below, so that the pulleys turned when the shutter fell. One end of a second cable was wound three times around the shaft of the lower pulley. The other end ran through the wall into the dark room. In the dark room was our fourth field, a movable background attached above and below to springs. The lower spring was fastened to the table upon which the apparatus rested, and the upper spring was fastened to the second wire cable. The ratio of the lower pulley to its shaft was such that, while the shutter was falling through its distance which gave an exposure interval of 90 σ, the second wire cable moved the same distance as that between the two members of the stationary stimulus (1 in.). The resistance of the springs was adjusted so that the movable background remained stationary at the beginning and end of the exposure for the same length of time that the members of the stationary stimulus were exposed (30 σ). Above the moving field was a light box which admitted light to the field when a shutter in the bottom was opened. This shutter was controlled by a solenoid with a contact on the falling shutter so placed that the light shutter was open for a time equal to the total exposure-time of the stationary stimulus.

[1]This Journal, 31, 1920, 317 ff.

The stimuli were (1) two stationary black horizontal lines (1 by 1/4 in.) on a white background (the one appeared for 30σ, and after an interval of 90σ the other appeared an inch below for 30σ); (2) an actually moving black line identical with those of (1), which appeared stationary for 30σ, moved down one inch during a 90σ interval, and then remained stationary for 30σ, after which it disappeared.

In our first procedure we used Dimmick's "process instructions:" "A stimulus will be shown you which will arouse a visual perception. Describe this perception in strictly psychological terms as accurately as you can. Report no process of which you are not sure."[2] The instructions were given to every *O* at the beginning of every observation hour. The four *O*s were: W. P. Williamson (W), beginning student in psychology; F. L. Dimmick (D); T. C. Schneirla (S), assistant in psychology; and A. C. Anderson (A), assistant in psychology. Only one *O*, D, knew the problem of the experiment.

O sat in the dark room for 15 min. before beginning his observation. The proper auxiliary shutters were opened, a ready signal was given, and the falling shutter was tripped. *O* gave his report to *E* through a speaking tube. The two stimuli were given an equal number of times in a haphazard order. On coming from the dark room *O* was asked to make drawings of his experiences.

Results

All *O*s gave reports of movements in practically all cases (90%) both with the stationary stimulus and with the moving stimulus. Not only did they experience movement with both stimuli, but the similarity of description was very marked. No *O* noted that there were two kinds of stimuli. The following reports are representative.

Stationary Stimulus

A "A vague bar appeared and disappeared. Then I saw two bars, one near where the first had been and one an inch below and a shadow moving between them. I saw the bars simultaneously but I could not get the time-relation of the movement." (See Fig. 5.)

W "A bar appeared distinctly above the center of the field and moved down. It was a blur all the way down. It apparently stops before the light goes out, for it gets more distinct at the bottom."

S "An object appeared above the center and soon changed position to a point below. The upper position was fainter than the lower one. A blur appeared between." (See Fig. 3.)

D "Very quick, flashy movement. The line was definite in the first position; flashed downward and disappeared; that is, there was a sharp, definite black line, with a grey patch below it and attached to it." (Similar

[2] *Ibid.*, 379 f. The "meaning" instructions were also tried; but owing to their lack of systematic training, the *O*s did not appreciate the point of difference between the two instructions and gave both "process" and "meaning" reports under each.

Moving Stimulus

A "I saw a clear cut bar, then a sort of blur which extended down from that bar about an inch; then a second bar appeared at the bottom." (See Fig. 8.)

W "Above the center the bar appeared quite distinct, clear-cut and black, and moved downward to below the center. The movement was a blur about the same color as the background except that it was a bit darker."

S "The object appeared near the top rather definite, moved rapidly down, and disappeared in this movement; that is, its movement was a grey streak and grew fainter at the bottom, and went out into nothing. The object did not appear below." (See Fig. 9.)

D "A very good movement—the line was definite in the upper and lower positions. None of the intermediate positions was definite. Just below the top black line was a space, and below this was a patch of grey attached to the lower position. The patch was streaked horizontally light and dark grey." (Similar to Fig. 10.)

Procedure IIa

Because the *O*s had made no spontaneous distinction between the two stimuli presented, we altered the instructions to determine, if possible, whether they *could* distinguish them; and, if so, what criteria they used. The procedure was the same as before, with these instructions: "You will be presented two different stimuli; one of the stimuli is moving, the other is not. You are to report first whether the stimulus presented is the moving or the non-moving stimulus; and secondly, you are to report the basis of your judgment."

Results IIa

	Trials	No Perception of Movement	Perception of Movement	?	Reports of Non-Mov. Stimulus	Reports of Moving Stimulus
O		W D A S	W D A S	W D A S	W D A S	W D A S
Stationary Stimulus	1st 10	2 1 1 4	8 9 7 6	0 0 2 0	0 7 5 0	0 2 0 0
	2nd 10	1 0 4 2	9 10 5 6	0 0 1 2	0 6 3 0	0 4 1 1
	3rd 10	2 0 4 2	8 10 6 5	0 0 0 3	0 10 6 0	0 0 1 0
	4th 10	0 – 2 –	10 – 8 –	0 – 0 –	0 – 7 –	0 – 0 –
	5th 10	0 – 3 –	10 – 7 –	0 – 0 –	0 – 7 –	0 – 0 –
Moving Stimulus	1st 10	0 0 0 5	10 10 10 5	0 0 0 0	0 3 0 0	0 7 9 1
	2nd 10	1 0 2 4	9 10 6 6	0 0 2 0	0 4 2 0	0 6 6 0
	3rd 10	0 0 2 5	10 10 7 3	0 0 1 2	0 0 1 0	0 10 7 0
	4th 10	0 0 2 –	10 9 7 –	0 1 1 –	0 1 1 –	0 8 7 –

The Table shows quantitatively the results of this procedure. With the non-moving stimulus all *O*s called the experience "movement" in 50-90% of the cases. The percentages for the moving stimulus were similar. This means that "moving" and "not moving" did not express characteristic differences between the two perceptions. Two of the *O*s (D and A) carried

out the instructions and made the distinction between the two stimuli in 70% of the cases. The other two *O*s (W and S) did not report the distinction, but gave descriptive differentiations in their reports. The outstanding fact is that, even though the *O*s were told that one stimulus was moving and that the other was non-moving, and were required to distinguish between them, they reported movement for both.

The following quotations from the *O*s' reports indicate the difference in the perceptions with the two stimuli.

With the *moving* stimulus the *O*s described their experiences as follows: W "equally intense throughout the movement," "not as fast"; D "movement more deliberate," "continuous," "could see it all the time in the movement," "slow movement," "steady movement," "saw intermediate positions," "perceived line as it moved"; A "no break in continuity," "did not lose track of it at any time," "could see it all the time," "no break," "no flicker, it was continuous," "I can keep stimulus in sight while it changes position"; S "moves slower for streak was rather dark," "not jerky but smooth," "could see object as it moved," "saw it at all points."

For the *non-moving* stimulus the *O*s report: A "a discontinuity of movement"; D "the speed of this movement is greater," "by speed I do not mean a temporal element, but a pattern of qualitative and spatial elements," "there is a clear-cutness of the spatial end-positions and rapid movement between them," "a smooth even patch of grey instead of a mottled one"; W "not smooth," "irregular," "jumped," "fast movement," "hesitation in movement," "rapid," "uneven," "jerky," "unsteady"; S "the object moved rapidly down," "the streak left behind the object was not as clear as before (when the stimulus was moving)," "I saw a streak between the positions; the movement was rapid because the streak was vague."

The *O*s perceived movement as frequently from the non-moving stimulus as from the moving stimulus. They all set up criteria and made distinctions between the stimuli, though in many cases they did not know that they were using them as criteria and did not report that they made the distinction. All of the perceptual experiences were primarily experiences of movement.

Procedure IIb

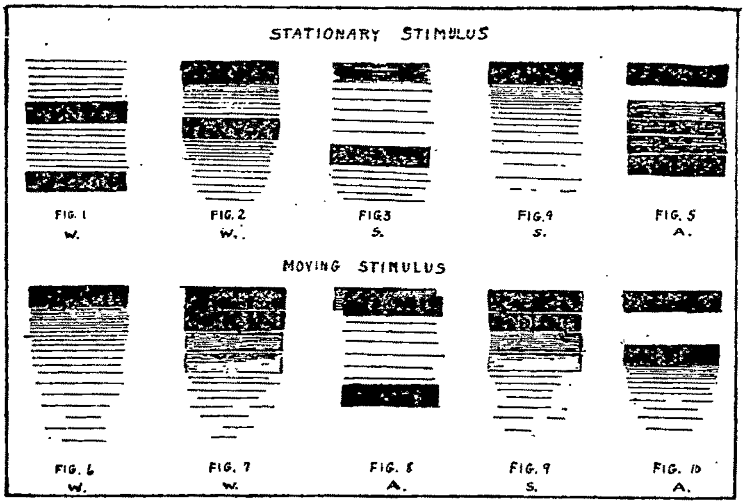
Inasmuch as none of the *O*s formally and consciously distinguished between the two stimuli, we changed the instructions to set up the stimulus attitude ('stimulus error') and thus to emphasize the distinguishing of the two experiences. The new instructions were: "You will be presented with two different stimuli, both of which will appear to move. Try to distinguish between them and give the basis for your distinction." Only two *O*s were available for this procedure.

Results

W was given 30 trials with each stimulus before he began verbally to distinguish the two stimuli. Then for 33 trials he shifted from one criterion to another until with the 64th trial he settled upon "even" and "uneven" as the proper distinguish-

ing characterizations. Of the 89 remaining presentations he was wrong in 4 cases, and there were two others in which he was not sure. This left him correct in 93% of the cases after he had developed the criterion of "evenness." "Even movement" was his characterization of the perception from the moving stimulus.

He described the two perceptions as follows. "In uneven movement, I see places in which the bar is more intense than in others. This leads me to believe that the bar is going slower in some places than in others." Further characterizations were: "it varies in intensity"; "the bar appears more as two flashes than as one continuous movement; at the indistinct



FIGG. 1-10 are copies of drawings made by 3 *O*s to supplement their verbal descriptions. Figg. 1-5 represent what the *O*s saw when the "stationary stimulus" was presented; Figg. 6-10 show the appearance of the "moving stimulus". Their respective verbal descriptions are given in the samples of reports.

points there is just a grey blur against the background"; "it appears as two separate motions—as it moves down it appears more intense at a point half way down,—it fades out, then jumps for a space and gets more intense, yet is still hazy"; "the bar moves down and disappears half-way below center—it seemed to come out of the background and become more distinct until it was black in the middle of the movement; then it faded and then became black again. The two places at center and end of the movement were deep black and clear-cut, while the rest was an indistinct grey of different intensities" (see Fig. 1); "the bar changes in intensity throughout the movement in 4 phases, intense, less intense, intense, less intense" (see Fig. 2). Even movement "keeps the same intensity throughout"; there is "no vibration"; "it is slower movement; its intensity falls off slowly at the end of the movement" (see Fig. 6); "a bar appears in the center, clean-cut and black, and moves downward, keeping this distinctness for about 1/3 of the movement, *i. e.*, it moves by steps; from there on it

decreases in intensity. By steps I mean I could *see* the bar in several positions and they were all seen at once, for at least some few steps" (see Fig. 7).

D was given 31 presentations under this procedure. After the first 6 he made the distinction. Of the remaining 25, 5 were wrong and of 3 he was uncertain. Thus he was correct in 68% of the cases. He used as his criteria "rate of movement" and "distinctness of positions." After his last observation he reported "the rate of movement is not adequate. The pattern of greys in the movement field is more distinctive. Whenever it was possible for me to get the pattern of one or the other distinctly I knew I was right, that is, there were some extreme and distinct patterns but many were intermediate. My other criteria were position in the field, size of the stimulus, outline of the stimulus, color or intensity, the absence of one member. All these factors came in but varied with the two main factors, often obscuring them and rendering my report uncertain." Thus the two stimuli can be distinguished, but only after much practice,—and then the distinction is not that of two different perceptions but only of two modifications of the same perception. The differences were more marked in the process-pattern than in the perception as such.

Conclusions

1. The present work confirms the previous work which brought out the process-pattern of movement in the form of a "filmy grey flash."

2. A moving stimulus does not give a perception of movement different in kind from that set up by a non-moving stimulus. The two perceptions are distinguishable not as experiences one of which moves while the other remains stationary, but as two forms of movement, *e. g.*, fast and slow. The corresponding process-patterns differ not in their essential form but merely in the relative distributions of the various degrees of intensity within the patch of grey, *e. g.*, greater or less uniformity. Physical movement of the stimulus, then, is not an element of the compulsory conditions of the perception of movement.

POPULAR ANSWERS TO SOME PSYCHOLOGICAL QUESTIONS

By H. K. Nixon, Columbia University

1. *Introduction*

The keenest class-room discussions in psychology center around such questions as telepathy, pre-natal influence, external signs of character, and the like. The beginning student especially, while willing to admit ignorance on many technical questions, comes to psychology with certain concepts of causation in human behavior quite firmly fixed. One of the common duties of the instructor is to help him sort over this initial stock of ideas, discarding some and working others into a more scientific formulation. A survey, therefore, of the prevalence of such common ideas may be of decided value to the psychologist as a starting point for his educational programme. The results of such a survey are presented in this article.

This field has been investigated in a more general way by several writers. Conklin (*1*) presents material on supersititious belief and practice among college students. He found that 82% recalled that they had believed superstitions at one time or another, 73% of the men thus reporting and 90% of the women. 40% of the males and 66% of the females admitted present superstitious belief or practice. Conklin thinks that the superstitions of women are more concerned with home, company, social relations, wishes, love, marriage and death, those of men with sports and success in business. In a later report (*2*) he cites the results of a very early investigation (*3*) in the same field to bear out his conclusions. Gould (*4*), Peters (*5*), and Dresslar (*6* and *7*) have likewise made special studies of this subject.

The purpose of the present investigation was somewhat more specific than that of those noted above, in that the primary interest was not in such general superstitions as those connected with black cats, spilled salt, and the like, but in beliefs which have greater scientific sanction. For that purpose, the list of 30 shown in the following questionary was selected, and presented in the form shown.

DIRECTIONS

Below are a number of statements. Some of them you may consider true, some of them false. If you think a statement is substantially true, draw a circle around T; if the statement is false, then a circle around F. Where it seems debatable, mark as seems to you nearest right.

1. T F The number of man's senses is five.
2. T F A child comes into the world with an instinctive knowledge of good and evil. This is his conscience and is born in him.
3. T F Certain lines in a person's hand foretell his future.
4. T F If you will stare at a person's back you can make him turn around. This is a form of telepathy.
5. T F It really is unlucky to have anything to do with the number thirteen.
6. T F A man's character can be read by noting the size and location of special developments of his head.
7. T F People with greenish eyes are not as trustworthy as people with blue or black eyes.
8. T F An expectant mother by fixing her mind on a subject can influence the character of her unborn child.
9. T F Women are inferior to men in intelligence.
10. T F People born under the influence of certain planets show the influence in their characters.
11. T F Intelligence can be increased by training.
12. T F Long, slender hands indicate an artistic nature.
13. T F Beginning an undertaking on Friday is almost certain to bring bad luck.
14. T F If a man but had faith enough he could heal a broken limb instantly.
15. T F Many eminent men have been feeble-minded as children.
16. T F Some animals are as intelligent as the average human.
17. T F No defect of body or mind can hold us back if we have enough will power.
18. T F Adults sometimes become feeble-minded from overstudy.
19. T F All men are created equal in capacity for achievement.
20. T F The marriage of cousins is practically certain to result in children of inferior intelligence.
21. T F Especially intelligent children are likely to be weak and retarded physically.
22. T F The study of mathematics is valuable because it gives one a logical mind.
23. T F A square jaw is a sign of will power.
24. T F You can estimate an individual's intelligence pretty closely by just looking at his face.
25. T F A high forehead indicates intellectual superiority.
26. T F Fear is unnatural. It is a bad habit.
27. T F Women are by nature purer and better than men.
28. T F A person who does not look you in the eye is likely to be dishonest.
29. T F Man is superior because his conduct is very largely guided by reason.
30. T F Any physical or mental disease can be contracted by thinking about it.

Name .... School ....

It will be observed that the ideas here expressed range from some which are quite apparently pure superstitions to some that would occasion considerable discussion among a group of

scientific men. It was the intention to make use of ideas which in the main have had the stamp of disapproval placed upon them by modern investigations. It was, however, found impossible to phrase the statements in such a manner as to avoid all chance for quibbling and charges of ambiguity. In general it seems likely that if the list were submitted to a group of psychologists there would be less than 1% of unqualified affirmative answers. The difference between that small percent. and the averages for the groups which were secured to take part in this investigation may serve as a rough indicator of the present prevalence of unscientific beliefs among the educated members of our population.

The subjects of this investigation were 140 women and 219 men, students in elementary psychology in Columbia University, Columbia University Extension, and evening classes in New York University. About 85% came from the two latter institutions. Students in these groups are on the average somewhat more mature than the ordinary college student; and it is probable that the sample afforded is more nearly representative of the intellectual and cultural level of people of average and above the average level than would have been the case with day-students in colleges and universities.

The questionary was presented to these students without any comment or discussion other than the reading of the directions. They were allowed to take their own time in making the answers. In a few cases there were questions, in which instances the student was instructed to answer as seemed to him nearer right. In general the time taken was about 8 min. The questions usually served to arouse much discussion and demands to be told the answers and to be informed as to scores.

2. The Results of the Questionary

A. Number of the 30 Statements marked as True. We have already noted that presumably not more than one or two of the statements of the questionary would be marked as true by a group of trained psychologists. It is interesting to compare that estimate with the actual performance of the group under consideration. Figures are presented in Table I.

The figures of Table I need little explanation. It will be noted that the males on the average marked between 10 and 11 statements as true, while the women average slightly more than 12. Considering the S. D.'s, these differences are probably significant. The distribution for the men shows a slight tendency to bimodality, probably due to the inclusion of 25 Columbia College men who showed an inclination to mark relatively few of the statements true. The average number marked by this group was 8.08. Along with this may be noted the tendency

TABLE I

Number of the 30 Statements in the Questionary Marked as True

	Males	Females
Number of *Ss* marking 0 Statements as True	4	1
" " " " 1 " " "	5	0
" " " " 2 " " "	4	1
" " " " 3 " " "	3	1
" " " " 4 " " "	10	3
" " " " 5 " " "	12	3
" " " " 6 " " "	11	7
" " " " 7 " " "	17	2
" " " " 8 " " "	18	9
" " " " 9 " " "	11	10
" " " " 10 " " "	14	11
" " " " 11 " " "	14	9
" " " " 12 " " "	14	8
" " " " 13 " " "	18	12
" " " " 14 " " "	16	18
" " " " 15 " " "	11	9
" " " " 16 " " "	4	8
" " " " 17 " " "	10	10
" " " " 18 " " "	9	4
" " " " 19 " " "	4	7
" " " " 20 " " "	9	6
" " " " 21 " " "	1	0
" " " " 22 " " "	0	0
" " " " 23 " " "	0	1
Total	219	140
Av. =	10.5	12.3
S. D. Dis. =	5.0	4.4
S. D. Av. =	3.3	.37

for the males as a group to be somewhat more variable than the females. Considering the groups together, it is to be seen that on the average a little more than one-third of the statements are marked true. Persons marking only 2 as true and persons marking more than 20 will be exceptional. We cannot be sure what it means to mark only a few. It may mean greater familiarity with scientific knowledge, and it may merely mean greater caution. On the other hand, a person who marks 20 or more of the statements true is probably relatively ignorant of the point of view of modern psychology. While no correlations of class-grades or intelligence-scores with the number marked as true on this test have been made, it is interesting to note that some students who marked 20 or more have done very creditable class work.

[illegible] *Believing each Statement.* The question of the average number of statements marked [illegible] sidered from another standpoint, that of the proportion of students believing any particular statement. This question may be studied from Table II.

TABLE II

Relative Prevalence of the 30 Beliefs

Statement No.	Idea involved	% of Ss marking as True: Males	Females	Order of prevalence: Males	Females
11	Intelligence increased by training	79%	80%	1	2
22	Math. gives logical mind	76	81	2.5	1
1	Number of man's senses five	76	78	2.5	3
29	Man superior because of reason	64	58	4	5
18	Adults feeble-minded from study	56	57	5	6
24	Face shows intelligence	50	62	6	4
12	Artistic nature indicated by fingers	42	52	7	9.5
6	Character indicated by head-shape	40	51	8	11
17	Will power supreme	39	52	9.5	9.5
25	High forehead shows intelligence	39	42	9.5	19
8	Prenatal influence	38	44	11.5	17.5
27	Women purer by nature	38	53	11.5	8
15	Early feeble-mindedness of great men	36	25	13.5	22.5
4	Telepathic influence of staring	36	56	13.5	7
26	Fear unnatural. A bad habit	35	47	15	15.5
23	Square jaw a sign of will power	34	50	16.5	12.5
20	Marriage of cousins gives deficients	34	50	16.5	12.5
28	Shifty eye shows dishonesty	33	47	18	15.5
21	Bright children physically retarded	32	33	19	21
2	Instinctive knowledge of good	31	39	20.5	20
30	Physical disease by thinking of it	31	44	20.5	17.5
16	Some animals superior in intelligence	25	48	22	14
19	Men created equal in capacity	23	20	23	24.5
9	Women inferior to men in intelligence	16	5.7	24	26.5
10	Character influenced by planets	15	20	25	24.5
14	Broken limb healed by faith	9	5.7	26	26.5
3	Lines in hand foretell future	8	25	27	22.5
7	Green-eyed people untrustworthy	4	4	28	28
5	Thirteen unlucky	1	0	29	30
13	Friday unlucky	1-	.7	30	29

It will be seen from this Table that about 80% of the *S*s thought that intelligence can be increased by training, this being held true to about the same extent by males and by females. More males believed this statement than believed any other, and it was second in popularity with the females. On the other hand, the ideas connected with thirteen and Friday are relatively unpopular, at least as measured by this method. On the basis of Conklin's work it is probable that many of the *S*s are actually influenced in their actions by these popular superstitions, but most refuse to mark them as true.

There appears a general tendency for the misconceptions most widely held—those at the top of the Table—to be due to ignorance or disagreement with the specific sense in which the psychologist uses the terms involved, as *e. g.* intelligence. The difficulty is then a simple one of lack of information. In the middle of the list fall ideas which are widely held, which are very popular, and which are often exploited by various agencies, such as the newspapers, but which have been definitely dis-

credited by the man of science. It is here that class discussions wax the hottest, and it is on these subjects that the instructor in elementary psychology should fortify himself with as much concrete evidence and as many experimental data as possible. At the lower end of the list appear popular superstitions which are widely known and acted upon, but which are usually quite frankly recognized for what they are.

The two columns at the right in Table II show the order of prevalence of the ideas with men and with women. In general the two orders are seen not to differ in any very marked manner. The greatest difference is in the case of No. 25, the idea that a high forehead indicates intelligence, which is considerably more popular with the males than with the females. Males are also somewhat stronger in the belief that many eminent men have been feeble-minded in childhood, and that the character of the unborn child may be influenced by the thoughts of the mother. The females show a slight preference for the idea that some animals are as intelligent as the average human and for the idea of telepathy. No very convincing explanation of these apparent sex-differences occurs to the author. In general they do not seem to him very impressive or important. Probably the important sex-difference is the tendency of the females to mark a greater number of statements true. On the average, they marked 41.1% of the statements thus, while the men marked 35.1%.

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ON THE SPECIFICITY OF EMOTIONAL REACTIONS

By David Wechsler, New York City

In the last decade much discussion has been devoted to the question whether what is called 'general intelligence' is something dependent upon a single general factor or is merely a convenient expression for the central tendency of a number of more or less independent factors or specific abilities. I wish to call attention to the fact that a similar problem exists in explaining the nature of emotion, and that the assumption of one or the other point of view in the case of emotion is of considerable theoretical and practical importance.

The question as it relates to the problem of emotivity may be stated as follows: Does an individual termed "emotional" react in an emotional way to all affective stimuli, or does he tend to react specifically to only certain types of affective stimuli or situations? In the first case we might infer that his uniform responsiveness is due to a single central factor which might be called 'general emotivity'; in the second, we could conclude that the selective responsiveness is due to some specific factors which might be termed specific emotivities, comparable to special abilities or types of intelligence in the intellectual domain.

No experimental data are at hand to enable us to argue for either alternative with absolute conviction. Nevertheless, some results which I have obtained in connection with certain experiments with the psychogalvanic reflex[1] as well as a number of empirical observations seem to show some evidence in favor of the view that individuals tend to be "specifically" rather than "generally" emotional.

In the case of the experiments referred to, the specificity of emotional responsiveness is indicated by the nature of the correlations between the magnitudes of galvanic responses obtained with each of a number of different types of affective stimuli for some 20 *S*s. The experiment consisted of subjecting 20 individuals to the same series of emotional stimuli, ranking them in order of magnitude of response to each of the stimuli employed, and finally calculating the correlations and intercorrelations between the galvanic responses on the various types of stimuli. If there existed such a thing as general emotivity in the sense of a single common factor, we should

[1] D. Wechsler, The Measurement of Emotional Reactions, *Archives of Psych.*, 1925, No. 76.

expect that the average intercorrelations between different stimulus-series would tend to be high, since the type of stimulus employed, providing it had been previously established as being effective in provoking emotional responses, ought not to alter significantly the relative position of the *S* among the individuals with whom he is being compared. A highly emotional individual, for instance, should react with comparatively large galvanic deflections, whether the excitation is that of pain, surprise, or what not. If individuals, however, tended to be specifically emotional, the *S* might react very markedly to sudden noises (surprise or fear) but only moderately or insignificantly to pain. In this case, of course, an individual's relative emotional standing as measured by the galvanometer would change with the type of stimulus employed, and the correlations obtained would be likely to be very low.

Examination of the actual figures obtained shows that, while many of the correlations were fairly high (as much as .81), some were quite low (around .20) or even negative (—.16). In general correlations were high between responses to types of stimuli evoking psychologically similar affective states, but small or insignificant between responses to stimuli evoking different affective states. Thus, the correlation between the responses to the sound of a Klaxon horn and those to a flash of magnesium (both, probably, evoking surprise) was .77; that between the reactions to the same Klaxon and the responses provoked by a pin-prick (pain), .47; but the correlation between the reactions to the Klaxon and the responses elicited by the situational stimulus of being asked to solve an arithmetical problem (giving rise in most *S*s to a feeling of apprehension or expectancy) was only .20.

The above results I take to indicate, for reasons already given, that individuals tend to react in a specific rather than in a general way to emotional excitation; but the results are not offered as unqualifiedly demonstrative of the conclusion indicated. The interpretation of the results is necessarily restricted by the limitations of the experimental method employed. Among these are, first, the relatively small number of *S*s studied and, secondly, the limited range of the stimuli employed. Further experiments are obviously needed to make the general conclusion more cogent. In the meantime, I would call attention to the fact that the conclusion is not unsupported by many instances that might be recalled from general observation. We find, *e. g.*, people who tremble at the sound of thunder but are quite unafraid of most other things. A nurse who will assist calmly at a bloody operation may shriek at the sight of a harmless mouse. The same man who applauds vociferously at a baseball game may fall asleep at the opera. Unless these

instances are exceptions rather than the rule, it would seem erroneous to use the term 'emotional' as implying a characteristic type of reaction. To be correct, we should in each case have to specifiy 'emotional with respect to such and such stimuli' or 'in such and such situations.' This limitation of connotation in the term 'emotional' is in fact assumed when the term is used in a practical way. If a railroad company, for instance, is selecting motormen and inquires whether the applicant is or is not emotional, it is not interested in finding out whether he is much stirred by music, gets quickly excited in an argument, or is readily provoked by his wife; what it wants to know is whether he is readily frightened by sudden noises, tends to lose his head under danger, and the like. And in using the term 'emotional' it restricts it to precisely these specific types of situations which it has in mind.

In summary of the above observations, it would seem that there is much against the assumption of a theory of general emotivity dependent upon a single central factor. On the other hand, there appears to be some good evidence for the view that individuals tend to be 'specifically' rather than 'generally' emotional. But the two conditions are not mutually exclusive. It is altogether possible that what is termed general emotivity may be determined by both a single central factor, analogous to Spearman's "g" in the case of general intelligence, and a number of less important but contributory independent factors which give emotivity its 'specific' character. Evidence for this point of view is, however, not yet at hand. Until it is available, I think that we are more justified and shall find it more useful to speak of specific emotivities than of general emotivity.

THE VALIDITY OF HEYMANS' LAW

By LLEWELLYN T. SPENCER, Yale University

In 1899 Heymans published his general law of inhibition (*2*) which states that the inhibitory power of a stimulus, measured by the intensity of a second stimulus which it can just completely inhibit, is proportional to its intensity. This means that a simple threshold is raised by the presence of a second stimulus in the environment; and that, given the amount of this increase for one stimulus, we may predict by direct proportion the value of the simple threshold when the second stimulus is varied by known amounts. Heymans found this generalization to be true in vision, pressure, taste, and sound.

The experiments which led Heymans to this general law were exceedingly painstaking but were carried on with only a single *S*. Révész (*9*) reported a proportional relationship between the threshold for various colors and the intensity of a surrounding contrast-field of white light. This is evidently a case of the same phenomenon described by Heymans.

J. E. Anderson (*1*) reported experiments on 8 *S*s showing that the differential threshold for two areas of illumination varied proportionally with the intensity of a third stimulus located midway between the areas differentiated.

In 1923 the writer (*10*) reported that Heymans' law held true for 5 albino rats for white light. The intensity of illumination to which the rats just failed in responding with 80% accuracy varied in direct proportion with the intensity of a second light-stimulus.

The raising of the simple threshold for the human being by a second stimulus has thus been demonstrated to be proportional to the latter only by Heymans with one *S*, although his findings are supported by Révész' contrast-work and by Anderson's study of the raising of the differential threshold. The following experiment was therefore conducted in order to test the validity of Heymans' original generalization with a relatively large number of human *S*s.

The apparatus used was a special slide photometer in which 45 watt Mazda lamps were moved to and from the *S* on individual slides running in separate dead-black, light-tight alleys. The position of each lamp was read at the end of the apparatus opposite the *S* from a scale fastened to the slide. The scale was laid out in logarithmic steps, so that equal readings on the scale represented equal variations in intensity of illumination at the aperture from which the *S* viewed the lights. The illumination fell upon a milk glass circular aperture 1 cm. in diam. and viewed by the *S* from a fixed distance of 1 m. When a lamp was advanced to the maximal position near the aperture the scale read 1,000 units. As the lamp was moved away the scale read by logarithmic steps, giving the fractional parts of the 1,000 light units presented at the aperture. Values given in the results below therefore represent such light-units, *i.e.*, thousandths of the maximal intensity. The lights employed were three in number, in a horizontal line,

with the centers of their respective apertures 6.25 cm. apart. The apertures were all of equal size. As viewed by the *S* the left-hand aperture was screened by a black glass filter to reduce the intensity so that the threshold would fall within the limits of the photometer scale. This light will hereafter be referred to as the *passive* stimulus. The middle aperture was covered with the milk glass only, so that its light, compared to that of the passive stimulus, was relatively intense, although never unpleasantly so. This stimulus is hereafter called the *active* stimulus. The right-hand aperture served throughout as a fixation point and was screened with a ruby glass filter and kept constant at a relatively low intensity. It therefore acted as a constant inhibiting factor comparable, *e.g.*, to the pressure constants of the *S*'s clothing, etc.

The experiment was conducted in a dark room. Fifteen min. for dark adaptation were allowed before any operations were begun. It usually was about 30 min. before any threshold value was fully determined; and as the whole experiment lasted only 60 min. the dark-adaptation factor may be assumed to be approximately a constant, except as varied by the stimulus illuminations as suggested below. After the 15 min. period the threshold for the passive stimulus was determined under three conditions, during all of which the red stimulus to the right remained constant: (*a*) with the middle or active stimulus not present, *i.e.*, =0; (*b*) with the active equal to 5 light-units; and (*c*) with the active stimulus equal to 10 light-units. It should be noted that the light-units in which the active stimulus is measured are not the same absolutely as those measuring the passive, owing to the interposition of the black glass filter before the latter. A light-unit is thus one thousandth of the maximal intensity attainable for the *given* aperture. The use of the red fixation point resulted in the thresholds being obtained for the periphery of the retina.

The procedure used in determining the thresholds was as follows. The passive stimulus was set at 1,000 light-units, *i.e.*, maximal intensity, and the *S* was asked if he could see it. Upon receiving an affirmative answer, the *E* reduced the intensity by fairly large jumps until the *S* reported that no stimulus could be discerned. The stimulus was then returned to the least intensity visible, and the exact value of the threshold was determined by the method of right and wrong cases. A double-throw switch controlled the presence or absence of the passive stimulus. The sound of the throwing of the switch was identical whether the light was present or not. When the *E* threw the switch he gave the signal "Ready," and the *S* fixated the red stimulus and attempted to judge as to the presence or absence of the passive stimulus. Five sec. were allowed to permit of the fluctuation of attention, and the circuit was broken with the signal "Now." The *S* was asked to report as to the presence of the passive stimulus at any time during the 5 sec. interval. Ten such trials were made at a given intensity, and then the next lower intensity used for ten trials, until a report of less than 80% accuracy was made. Between each series of 10 a brief rest was allowed. The threshold was redetermined in the same way for conditions (*b*) and (*c*) as stated above.

The results for the 50 *S*s tested are given below in Table I and Plate 1. A glance at the plate will indicate the rectilinear relationship between the active stimulus and the passive threshold for the group. The figures to the right in Table I are given to indicate how closely each individual approximated a perfect direct proportion. The straight line of best fit for each individual's values was calculated, and his deviations from such straight-line values were tabulated for each of the three thresholds. The means of these deviations for each threshold for the

50 individuals were computed, and are given in the Table as the mean deviations from individual straight lines of best fit. The figures indicate that the deviations from a perfect proportion were in general but fractions of a light-unit.

TABLE I

50 cases

Mean Thresholds			M. D. from indiv. straight lines of best fit		
Active stimulus 0	5	10	0	5	10
Passive " 11.89	43.84	74.44	0.645	1.25	0.729

Two additional cases may be referred to as Part *B* of the experiment. These individuals had only uniocular vision, owing to traumatic loss of vision in one eye. Their results are given in Table II, together with the straight lines of best fit, for purposes of comparison.

TABLE II

2 cases

Uniocular vision

	Observed Thresholds			Best-fit values		
Active stimulus	0	5	10	0	5	109
S A	9	47	80	9.82	45.33	80.8
S B	9	40	70	9.15	39.66	70.1

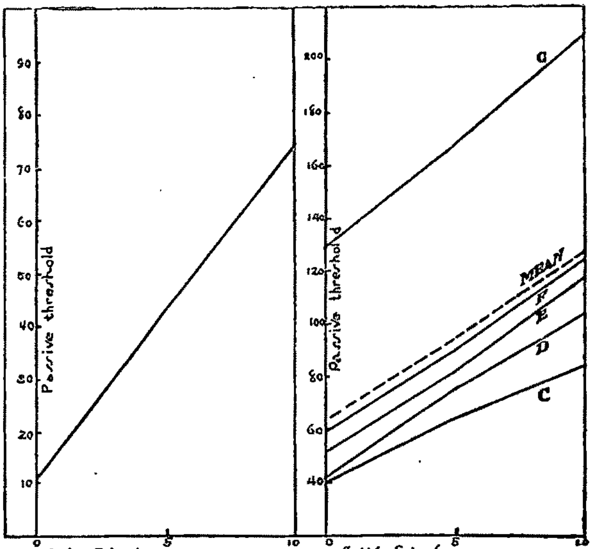
A third variation, Part *C*, was conducted to determine the validity of the law when the active or inhibitory stimulus was presented to the right eye only and the passive stimulus only to the left eye. This procedure should bear on the question whether the inhibitory nature of the phenomenon is peripheral or central, inasmuch as the stimulation and resulting impulses cannot conflict until reaching the optic chiasmus at least. In this experiment the active stimulus was placed at the aperture formerly occupied by the red fixation-point, *i.e.*, the centers of the passive and active stimuli were now 12.5 cm. apart. No fixation-point was used, and the threshold values are presumably for approximately foveal vision. A partition extended from the mid-point between the two apertures to the bridge of the *S*'s nose. Five *S*s served in this experiment. Their results are given in Table III and Plate II. The black filter before the passive beam was inadvertently altered slightly so that the illumination was decreased. This may account in part for the somewhat higher thresholds obtained, as compared to the first and second Parts of the investigation.

TABLE III

5 cases

Passive confined to left eye; active confined to right

Observed Thresholks				Best-fit values		
Active stimulus	0	5	10	0	5	10
S C	40	65	85	40.79	63.33	85.87
S D	41	75	105	41.65	73.66	105.68
S E	52	82	118	50.96	84.	117.04
S F	59	90	125	58.21	91.33	124.35
S G	129	168	210	128.5	169.	209.5
Mean	64.2	96	128.6	64.06	96.26	128.46



Active Stimulus Plate I Data Table I — Active Stimulus Plate II Data Table III

The results of all parts of the investigation uphold the generalization first published by Heymans. The relationship holds for uniocular as well as for binocular vision. It also holds true when the active and passive stimuli fall upon different eyes. The present investigation, then, although it is subject to various sources of error which have not been completely controlled, as suggested below, serves as contributory support of Heymans' generalization. Part *A* of the experiment is significant, especially, because it represents the first confirmation of the phenomena for a relatively large number of cases. The closeness with which not only the mean values but also the individual values conform to a straight-line relationship is also very striking.

The evidence collected in this Study supports the general conclusion that the simple threshold is related to the intensity of a second stimulus simultaneously presented by a direct proportion. This may not necessarily support Heymans in his statement of the phenomenon as one of central inhibition. In order to consider the question of the actual nature of the phenomenon it will be necessary to discuss the sources of error in the present experiment and also other possibilities of explanation which might make the phenomena interpretable upon other grounds than those of central inhibition. Since the question raised concerns not merely the interpretation of the present experiment but also the significance of Heymans' experiments as well, the various critical cases studied by Heymans and others may also be cited as factors in our discussion. Let us first consider other possible explanations of the phenomena.

(1) A physical interference, *i.e.*, the interference of the light-waves before reaching the retina is hardly conceivable as an explanation. Part *C* effectually disposes of this possibility. Furthermore, Heymans' study of pressure stimuli (*3*) where no such interference can occur reveals the same phenomena as in vision.

(2) The disappearance of the passive stimulus when the active is presented may be caused by diffusion of the illumination by the refractive media of the eye, so that the increase in illumination caused by the presenting of the passive may be below the differential threshold for discrimination from the surrounding areas stimulated by the diffused active stimulus. Again, experiment *C* refutes this contention, or at least refers such an occurrence to a theoretical combined retina which is certainly not peripheral. In addition the experiments of Heymans on the inhibition of one mode of excitation by another (*4*), *e.g.*, of sound by electrical stimulation, are not compatible with this view. Jacobson also reports the inhibition of sound by pressure (*6*) and odor by sound and sound by odor (*7*), although his results do not deal with the quantitative relationship which forms Heymans' law.

(3) The phenomena appear akin to those of simultaneous contrast which is known to decrease the apparent brightness of an area contrasted with a brighter area. This explanation may be accepted; but Heymans suggests that simultaneous contrast is really a case of inhibition such as he describes, and by a series of careful experiments shows that the brightness perceived varies from the extreme of negative contrast to the extreme of positive contrast in a regular and continuous manner with the brightness of the contrasting field (*5*).

(4) The phenomena may be considered to occur because of a withdrawal of attention from the passive stimulus. Several

investigators find support for the importance of this factor (*6*, *8*). The direction of attention will, *e.g.*, affect the amount of interference or may under circumstances completely counteract it. But attention itself is a form of inhibition—or at least a facilitation at the expense of other processes. Heymans' experiments were carried on with the voluntary attention held constant so far as possible on the passive stimulus.

Each of these alternatives can thus be shown to be inadequate to cover all of the cases studied, or may itself be interpreted as identical with a central inhibitory process. It is not denied that certain of these, *e.g.*, (2) under uniocular stimulation, may be partial influences in determining the absolute amount of increase necessary in the passive threshold; but it seems evident that in addition to these possibilities there is an underlying phenomenon identical throughout the various situations studied, which is one of inhibition and at least under certain circumstances is central.

The experimental evidence reported in this paper is not, of course, fully conclusive. Various sources of error must be considered. Each *S*'s thresholds were determined only by a single value although the method used gives such values a fair degree of reliability. Again, the improvement-factor in reacting to very weak stimuli would act to lower the second and third thresholds. The average line for the 50 *S*s of Part *A* does indicate a variation from a straight line in the direction of lower thresholds with the active stimulus equal to 10 light-units, although this variation is, of course, barely more than a single light-unit. The improvement-factor is accompanied by another factor whose effect would be in a similar direction, *viz.*, the increasing dark adaptation of the *S*. As indicated above, this factor is not perhaps particularly disturbing except as noted below. Both improvement and increasing dark adaptation are compensated—although to an undetermined degree—by the effect of fatigue. Although frequent rests were allowed, as already mentioned, it is probable that fatigue can not be neglected as a possible disturbing factor.

The most serious weakness of the experiments as an attempt to test conclusively the validity of Heymans' law as a phenomenon of central inhibition inheres in another aspect of the dark-adaptation factor. It is a possible interpretation of the values herein reported that, by the introduction or increase of the active stimulus, the degree of dark adaptation is altered in such a way as to make a formerly visible passive stimulus now undiscernible. Such a contention may indeed be urged against Heymans' own experiments with light, even though many of his experiments were done with the light-adapted eye. Such an influence is exceedingly difficult to eliminate in visual experi-

mentation. Although such an explanation does not seem sufficient to invalidate Heymans' conclusions with regard to the fields of taste, pressure, and hearing investigated by him, it does prevent us from making any final statement with regard to vision. The present investigation substantiates Heymans' results in vision, but does not finally settle the question whether such results are similar in nature to those which he obtained in other sensory fields. A conclusive answer to this problem remains to be established by further investigation.

Summary and Conclusions

A group of 50 individuals show both individually and collectively that the visual threshold for white light increases in direct proportion with the increase in intensity of a second white light placed 6.25 cm. distant in a horizontal line, a constant red stimulus beyond the second white light serving as a fixation-point. Two uniocular *S*s under the same conditions also confirm the relationship. Five individuals exhibited the same relationship when the threshold stimulus was restricted to the left eye and the second stimulus was restricted to the right eye, the fixation light being discarded and the distance between the stimuli being doubled.

These results confirm those of Heymans and support the quantitative statement of his law. The question whether the relationship found is one of central inhibition is discussed, with the conclusion that other explanations do not seem adequate to subsume all the facts or are not incompatible with the inhibitory view. The desirability of further experiments in vision, especially with the factor of dark adaptation controlled, is indicated.

References

(*1*) J. E. Anderson, *Psych. Bull.*, 21, 1924, 99–100. Cf. this JOURNAL, *infra.*

(*2*) G. Heymans, *Z. f. Psych. u. Phys. d. Sinn.*, 21, 1899, 321-359.

(*3*) *Id.*, *ibid.*, 26, 1901, 305-316.

(*4*) *Id.*, *ibid.*, 34, 1904, 15-28.

(*5*) *Id.*, *Z. f. Psych.*, 41, 1906, 94-100.

(*6*) E. Jacobson, *Psych. Rev.*, 18, 1911, 24-53.

(*7*) *Id.*, this JOURNAL, 23, 1912, 345-369.

(*8*) S. M. Newhall, *Archives of Psych.*, 61, 1923, 75.

(*9*) G. Révész, *Z. f. Sinnesphysiol.*, 41, 1907, 1-36.

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THE FIDELITY OF REPORT OF NORMAL AND SUBNORMAL CHILDREN

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It is still an open question whether or not intelligence conditions the ability to report. It is, at the same time, a question of no small practical importance, as well as one of scientific interest. The most definite statement to be found regarding the dependence of report-ability on intelligence is that of Whipple (1): "There is no conclusive evidence upon the relation between good report and general intelligence." There are, of course, numerous general statements unsupported by evidence.

Breukink's work (2) suggests that intelligence may play a positive part in report, since he found that persons of culture, as represented in the educated professions, gave more extended and accurate reports than relatively uncultured observers, such as nurses and working men. Franken (3) found that his more gifted and cultured *S*s tended to excel through a greater tendency to caution. V. C. Hicks (4), in a practical case at Vineland, where the veracity of the testimony of two girls about a certain abuse was in doubt, gave the Stanford Revision of the Binet tests and, finding that the I. Q. of each was 75, that neither could repeat the 6 digits of the 10-year group, and that both were lacking in memory and imagination, concluded that they were not capable of making up such a story as they told, and accepted their testimony. Duprée (5) gives a general account of the poor testimony of defectives and warns "emphatically against the peculiar danger that exists when the defect is latent or mild and unsuspected by the court."

The most extensive experimental work on the subject has been done by Pear and Wyatt (6). They took the report, on an event test, of 65 normal children and 78 defectives. They define their defective children as "children of such a degree of mental subnormality that they had been adjudged to require teaching in 'special' schools (in the sense usually attached to the word in England)." Their normal children are those attending regular schools. Such a method for the selection of *S*s leaves a doubt how defective their defectives were, and perhaps how normal their normals were. Their study indicated that the normals were superior, in report-ability, to the defectives.

The purpose of the investigation here reported was to compare the fidelity of report of two groups of children of known intelligence—one normal and the other definitely subnormal—on three different *Aussage*-tests, the Binet object-card, a picture and an event. The two groups to be compared are from the East St. Louis (Ill.) school system. One is a group of 44 normal children from the Froebel School. The other contains 58 subnormal children from the Ungraded School, to which are sent the most pronounced subnormals of the school system.

The subnormals have an average chronological age of 13.69 years, S. D. 1.75. For comparison with these, a group of very nearly equal chronological age—average 13.81, S. D. 1.00—was chosen from the 8^1-8^2 grades. Each group was given the National Intelligence Tests, Scale A, Form 1. In addition, a second intelligence test was given to each to make more certain that a fair measure of intelligence had been obtained. Scale B, Form 1, of the National Intelligence Test was given to the subnormals, and Army Alpha, Form 5, to the normals.[1]

The mean score of the subnormals on Scale A was 75.70, S. D. 23.35, and on Scale B it was 74.76, S. D. 20.00. The mean score of the normals on Scale A was 125.22, S. D. 16.15, and on Army Alpha 86.90, S. D. 16.55. The norms for year 13^2 are 124 for Scale A and 123 for Scale B. The subnormals, therefore, lie 48.3 points below the norm for Scale A, and 48.24 points below that for Scale B. The normal group have a mean score which is practically at (one point above) the norm. On Army Alpha the normals stand above the norm for year 13, which is variously given as 76, 83, and 84.[3] It thus appears that we are dealing here with one group which is definitely subnormal and another which is definitely normal.

The relative mental status of the two groups may be expressed in a fashion more generally comprehensible, perhaps, if the scores be translated into mental ages, although this procedure is not strictly accurate. So translated, the National Intelligence Test average mental ages are, for the subnormals, 10 years 6 months on Scale A, and 9 years 11 months on Scale B; and for the normals 13 years 7 months on Scale A. Interpolating Alpha scores into mental ages is dangerous, but as it stands the average Alpha score of the normals gives them an average mental age of somewhere between 13 and 14 years at least. In terms of mental age, therefore, we are dealing with two groups which, although practically equal in average chronological age, differ by at least 3 years in mental age. In so far as the average indicates, the classification of the subnormal group is borderline deficiency. This group included, however, a few very feeble-minded cases and a few who are low normals. In the normal group there are a few low normals, but when the distribution curves for the two groups are drawn, they overlap scarcely at all. We have, then, for comparison two groups of distinctly different mental status.

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[1]Army Alpha was chosen as a second test for the normals at the request of the principal of the school.

[2]Given in Supplement No. 2 to the Manual of Directions for the National Intelligence Tests, Table 4.

[3]Given in the Manual of Instruction for the Army Alpha Intelligence Tests, prepared by J. C. De Voss.

With each of these groups three different report-experiments were conducted.

The material for the first was the Binet object-card,[4] used by Binet (7) in some of the early experiments in the psychology of report. It is a rectangular sheet of orange-yellow cardboard, 33.5×40.5 cm. Attached to it are two photographs, a red and white label, a white button, a penny and a one-cent postage stamp. The *S*s were given the following directions: "I want to find out how much you can see and how much you can tell me about what you have seen. I am going to show you a large piece of cardboard with several objects fastened on it. You will have just thirty seconds, or half a minute, in which to look at it. Half a minute is a pretty short time, so you will have to look pretty hard, because afterwards I shall ask you to write out a description of what you have seen; and then I shall ask you some questions about what you saw, and I want you to write the description and answer the questions as accurately as you can." The *S*s were then shown the card for 30 sec., at the end of which time the card was again covered. The instructions for the writing of the narrative or description were: "Write out an account of everything you saw; describe it so clearly that, if I had never seen the card, I should know all about what was on it. You will have as long a time as you wish in which to do it."

When the narrative was completed, the *S* was given a mimeographed sheet containing 50 questions, with spaces after them for the answers, and was told to answer the questions. The questions were, with two or three exceptions, those suggested by Whipple (1) for use with the object-card.

The material for the second experiment was the picture entitled "A Disputed Case," 15 × 17 in. The objects represented in it are practically all of good size, practically all colored, and are not too numerous to be carefully noticed in the time allowed. The directions for this were the same as those for the object-card, except for the modifications necessary with the changed material. The children were not, however, told how long they were to have to look at it, but were told only that it would be a "short" time. The time allowed was 20 sec. The narrative was taken, followed by an interrogatory of 50 questions taken mainly from Whipple (1).

The third experiment was an event after the fashion of those used by Jaffa in von Liszt's seminar at Berlin (8), by Lipmann (9), by Morgan (10), McGeoch (11), and Pear and Wyatt (6). Two persons participated in the event, the conditions of which had been prearranged with the teachers of the various rooms. The persons were a student, skilled in amateur dramatics (K), and the writer (M). The children had never seen the former, but had seen the latter several times before, when he was giving the intelligence tests and the two report tests mentioned above. On the day of the event, however, he was dressed otherwise than usual.

While class was in progress, M knocked at the door and entered the room, carrying a dark-brown Boston bag, followed by K, attired in brownish grey knickerbockers, with light grey stockings and tan oxfords, and carrying a raincoat and an umbrella. Just as both were clearly inside the door, they exchanged the articles they were carrying. Then K turned to the teacher and said, "Are you the teacher of this room?" She replied "Yes." Then K walked to the desk, placed the bag on top of it, in plain sight of all the children, and began taking out of the bag several articles—a red book, a number of sheets of paper (the interrogatory blanks, which were left on the desk), a light grey cap, and a book with a yellow cover and blue back, called the "Slant Book," its cover being cut on a slant, so that when it is open it looks like a pair of butterfly wings, with a large V

[4]The modification of the original Binet object-card furnished by C. H. Stoelting Co. was used.

at the top. The articles were held up for a moment before being deposited on the desk, K giving the impression that he was searching for something. When at last he came to the "Slant Book," he opened it and held it out so that the teacher could look into it, its back being toward the class. Then he said, "I have here a new book in arithmetic. Would you like to buy some for the class?" The teacher replied, "No, not to-day." K bowed and slowly replaced in the bag each object which he had previously taken out, with the exception of the papers, but in reverse order. He then took up the bag and walked out, followed by M, who had been doing nothing, during the interval, except to stand where he had stopped upon entering the room, being careful to hold the umbrella and raincoat in plain sight. As it was a clear day in spring, these articles were somewhat out of the ordinary. M carried a stop-watch, which was started upon entering the room and stopped as he left. The time for the event before each of the groups was about 40 sec., with only 2 or 3 sec. variation in any case. The event was made brief, in order to keep it from becoming complicated, and to make it conform in time to the brief occurrences upon which children are often called to report in everyday life.

As soon as the participants had left the room, the teacher asked the children to write out an account of everything that had been said and done, according to written directions which had been given to her previously. An interrogatory of 25 questions was used. In general the procedure was the same as for the object-card.

With each of the three types of material, both narrative and interrogatory reports were taken, and the children were given unlimited time in which to write the narrative and to answer the questions. These were, therefore, of the nature of "power" tests, the element of speed being eliminated. Each type of material was given as a group test. There were three groups of the subnormals and two of the normals. The event was enacted in the front of the room and in full view of each group. With the two other types of material, the *S*s were told to come two at a time to a table in the front of the room, where they were allowed to look at the material for the time allowed—30 sec. for the object-card and 20 sec. for the picture. Immediately afterwards they were told to start writing the description. This method made it possible to give the test to all the members of a group in a comparatively short time. Each type of test was given to all the *S*s in the same building on the same day and in such a fashion that there could be no communication between groups in the meantime. The Froebel School and the Ungraded School are sufficiently far apart to render intercommunication regarding the tests highly unlikely.

In one part of the experiment, with the picture, the members of each group were asked to underline each item of the narrative of which they were so absolutely certain that they would be willing to take oath in court that it was correct, the meaning of such certainty being further explained to them. In the interrogatory they were asked to circle the number of each question of the answer to which they were certain. This procedure was followed in only one part of the experiment with the subnormal group, because the extra time involved in going back over narrative and interrogatory to check the items of which they were certain rendered it impracticable to carry it further. Even after they had performed previously with the object-card and were therefore somewhat used to the general method, the attestation on the picture test consumed a large amount of time.

The narrative for each test was scored by counting each separate item, [illegible] Interpretations and indefinite statements were not counted. The interrogatory was scored by counting the [illegible] question, right or wrong, as one. It would be better if some qualitative method of scoring could be used to supplement the quantitative, but no such method appears to be feasible. As it is, the quantitative score represents with accuracy the absolute fidelity of report and the range of report,

together with items incorrectly reported, without taking into account the fact that some items are of more value in a report than others.

The prevailing method of taking reports in the past has been to have the *S* give his report orally to the *E*, while the latter writes it down. This consumes a great deal of time. The method of having written reports makes possible the gathering of a much wider range of data than would be possible if each narrative and deposition had to be recorded by the *E*. It is doubtful if there is any decrease in the value of written reports; but if there is, it is more than compensated for by the greater number of cases that can be investigated.

To discover the relative worth of oral and written reports, and to ascertain whether or not the written method could be safely used, 31 oral and 27 written narratives and depositions were taken at the Ungraded School, at the beginning of the investigation. The method used in selecting the *S*s for the oral reports was to take one pupil at random to start with. As soon as he had finished the test, he was told to go back to the room and tell someone else to come to the office where the testing was being done. This was repeated with each *S*, previous arrangements having been made with the teachers to permit it.

When the oral testing, which occupied several afternoons, had been completed, the children who had not been tested were collected in two groups and given the object-card test, this time the reports being written. It would seem that by the method used a fair random selection has been obtained. Table I contains a comparison between the two methods in the number of items correctly and incorrectly reported, together with the total number of items reported. Scores are averages in all cases.

TABLE I

Binet Object-Card

Narrative	*Cases*	*Right*	*Wrong*	*Total*
Oral	31	14.1	2.5	16.6
Written	27	14.5	2.4	16.9
Interrogatory				
Oral	31	27.1	12.1	39.2
Written	27	25.9	13.4	39.3

No very significant differences appear between the results obtained by the two methods. The reports with each method are practically equal both in range and in accuracy. Consequently, written reports were used exclusively throughout the rest of the experiment. Figures for the object-card with the subnormals cited hereafter are for both oral and written reports, as given in Table I.

In Table II are given the comparative scores for normals and subnormals on the object-card; in Table III the score on "A Disputed Case"; and in Table IV those for the event.

TABLE II

Comparative Scores on Binet Object-Card

Normals 44 Cases	*Right*	*Wrong*	*Total*	*% of error*
Narrative	22.9	3.5	26.4	13.2
Interrogatory	32.2	11.7	43.9	26.6
Subnormals 58 Cases				
Narrative	14.3	2.5	16.8	14.8
Interrogatory	26.7	12.6	39.3	32.0

The range of report (total number of items reported) is greater for the normals in both narrative and interrogatory; the difference being 9.6 on the narrative and 4.6 on the interrogatory. In number of items rightly reported the normals average 8.6 higher on the narrative and 5.5 higher on the interrogatory. For both groups the range is greater on interrogatory than on narrative, and the percentage of error is slightly more than twice as great for the interrogatory. The percentage of error is greater throughout for the subnormals. It should be pointed out, however, that in actual number of errors there is no very great difference between the two groups.

For the picture, attested and unattested items are shown separately. Averages for these items include only those cases who gave attested or unattested items. Some *S*s attested all items; some failed to attest any. When the averages take this fact into account, they are more significant with respect to the accuracy and range of attestation and the lack of it. However, this makes the total averages given fail to agree with the total of the averages for right and wrong items. This must be borne in mind, when the Tables where attested items are given are interpreted.

TABLE III

Comparative Scores on "A Disputed Case"

		Normals			
Narrative	*Cases*	*Right*	*Wrong*	*Total*	*% of error*
Attested	37	28.0	4.3	32.3	13.3
Unattested	36	5.2	3.3	8.5	38.8
Total	39	31.4	7.1	38.5	18.3
Interrogatory					
Attested	35	18.0	10.4	28.4	36.6
Unattested	39	9.7	11.6	21.3	54.4
Total	39	25.9	20.9	46.8	44.6
		Subnormals			
Narrative					
Attested	27	12.0	2.5	14.5	17.2
Unattested	35	8.0	1.5	9.5	15.7
Total	41	14.8	2.9	17.7	16.3
Interrogatory					
Attested	14	10.8	7.1	17.9	39.6
Unattested	41	16.6	14.3	30.9	46.2
Total	41	20.3	16.6	36.9	44.9

The range of report of the normal group is greater than on the object-card in both narrative and interrogatory. For the subnormals it is slightly higher on the picture narrative and slightly lower on the picture interrogatory. Here also the normals excel the subnormals in range of report, this time by 20.8 on the narrative and by 9.8 on the interrogatory. In number of items rightly reported the normals excel also. In percentage of error there is a slight balance in favor of the subnormals on the narrative and a practically equal percentage on the interrogatory. In both groups it is high on the latter. The much greater range of the normal group is a significant advantage; but in so far as the actual number of errors is concerned, the subnormal group is superior, having fewer actual errors than the normals.

When attestation is considered, it is seen that the normals attest more items and with a smaller percentage of error than the subnormals on the narrative. The normals, in fact, attest the majority of the items, and the percentage of error in the remaining and unattested items is remarkably high. It is significant that the percentage of error in the items unattested by the subnormals is smaller than that for the attested items. It would not appear that attestation with them is any more an assurance of accuracy than lack of attestation. The same general conclusions hold true for the interrogatory, except that there the subnormals have a smaller percentage of error in attested than in unattested items.

TABLE IV

Comparative Scores on Event Test

Normals 41 cases	*Right*	*Wrong*	*Total*	*% of error*
Narrative	45.0	6.1	51.1	11.9
Interrogatory	13.8	8.9	22.7	39.2
Subnormals 31 Cases				
Narrative	26.9	6.6	33.5	19.7
Interrogatory	11.5	10.0	21.5	46.5

Inspection of the above Table shows that, for both groups, the range of report in the narrative is higher on the event than on either object-card or picture. The children grasped more details and described with more minuteness than on either of the two other tests.

The range of report on the event is uniformly higher for the normal group, being higher by 17.6 on the narrative and 1.2 on the interrogatory. It should be remembered that in the interrogatory for the event test there were but 25 questions. The percentage of error is greater throughout for the subnormals, and for both groups it is larger by far on the interrogatory than on the narrative. The subnormals also have a greater average number of actual errors.

The sigmas of all the differences, hitherto cited, between normal and subnormal groups are small enough to indicate that the differences are significant. In every case the difference is at least three times the sigma, and in most cases is much more than three times the sigma.

An analysis of the number of individuals giving errorless reports shows that there were a few such reports among the narratives, but none at all among the interrogatories. The percentages of individuals giving errorless reports, together with the comparative ranges of their reports, are given in Table V. Under "Range" the first figure means average number of items given by those having errorless reports; the second figure means average number of items reported by the remainder, whose reports showed some error. The same thing is shown for attested items, reported on the picture, in Table VI.

TABLE V

Errorless Reports

	Object-Card	*Range*	*Picture*	*Range*	*Event*	*Range*
Normals	.06	17.0–22.4	.05	24.5–39.4	.02	16.0–52.0
Subnormals	.22	15.9–17.1	.07	9.6–18.4	.06	16.5–34.6

TABLE VI

Errorless Reports—Attested and Unattested Items

	Attested	*Range*	*Unattested*	*Range*
Normals	.09	20.2–33.8	.16	2.8– 9.8
Subnormals	.11	9.3–15.2	.25	6.4–10.5

On all the narratives for both groups a small percentage of errorless reports was found. It is highest in the case of the subnormals with the object-card. When the attested and unattested items reported on the picture are taken separately, it is found that for both groups there is a smaller percentage of errorless reports in attested than in unattested items. Throughout, however, the range of those giving errorless reports is much smaller than that of the others. A strictly accurate report, therefore, seems necessarily to be less extended. The subnormals give a greater percentage of errorless reports than the normals; but when their greatly lessened range of report is considered, it may be concluded that they are not, on the whole, as good reporters.

Coefficients of Correlation.—From the foregoing data it seems established that the normal children give a much wider range of report and a smaller percentage of errors than the subnormal; and that, when they attest their reports, their reli- [illegible] be remembered, however, that on the object-card and the picture the subnormals have a smaller average number of actual errors than the normals, with the one exception on the object-card

interrogatory. As a general conclusion, therefore, it can be safely said that normality of intelligence positively conditions range of report, percentage of accuracy and reliability of oath. It is worth while, however, to inquire more specifically what is the relationship between fidelity of report and intelligence, within the groups. The coefficients of correlation between *Aussage*-scores and scores on the National Intelligence Tests are presented below. The product-moment method has been used.

TABLE VII

Coefficients of Correlation

National Scale A with	No. Right	No. Wrong	Total No.
Narrative			
Normals			
Object-Card	$-.165 \pm .103$	$-.063 \pm .105$	$-.149 \pm .103$
Picture	$.000 \pm .115$	$-.111 \pm .113$	$.000 \pm .115$
Event	$.028 \pm .115$	$-.216 \pm .109$	$-.028 \pm .115$
Subnormals			
Object-Card	$.364 \pm .081$	$.159 \pm .091$	$.331 \pm .083$
Picture	$.424 \pm .077$	$.335 \pm .095$	$.396 \pm .079$
Event	$.413 \pm .104$	$.164 \pm .120$	$.393 \pm .106$
Interrogatory			
Normals			
Object-Card	$.030 \pm .109$	$-.044 \pm .105$	$-.092 \pm .108$
Picture	$.223 \pm .108$	$-.277 \pm .106$	$-.118 \pm .113$
Event	$-.181 \pm .111$	$-.011 \pm .115$	$-.196 \pm .100$
Subnormals			
Object-Card	$.216 \pm .089$	$-.141 \pm .092$	$.172 \pm .091$
Picture	$.578 \pm .062$	$.204 \pm .102$	$.530 \pm .067$
Event	$.435 \pm .102$	$.289 \pm .113$	$.486 \pm .096$

The coefficients of correlation between Scale A *plus* Scale B and report, in the case of the subnormals, and between Scale A *plus* Army Alpha and report, in the case of the normals, have been computed, but they are so nearly the same as those given above that it is hardly worth while to give them here. Their computation, however, is valuable as affording a check on the accuracy of the intelligence measurement, and gives added assurance regarding the validity of the results.

Some very unexpected results emerged when the intelligence scores and the results of the report experiments were correlated. For the normals the relationships are either slightly inverse or practically zero. In most cases the probable error is nearly as large as, and sometimes larger than, the coefficient of correlation. There is a very slight tendency for the coefficients between intelligence and the number of items rightly reported to be somewhat higher than those between intelligence and the total number of items given, and more often positive. Coefficients of correlation between intelligence and the total number

of items given are slightly negative throughout for the interrogatory, and with the exception of one zero coefficient the same thing holds true for the narrative. The relationships between intelligence and number of errors in report are slightly inverse throughout.

The coefficients of correlation for the subnormals are, with one exception, all positive and indicate a moderate degree of relationship between intelligence and ability to report. Except for the event interrogatory, the coefficients of correlation are higher between intelligence and total number right than between intelligence and total number of items reported. Thus, for both groups, ability to report correctly is slightly more related to intelligence than is the ability to give a wide range of report. There is also some tendency for errors and intelligence to be related positively in the subnormal group. Table VII shows a tendency for the amount reported to increase as intelligence increases up to about normality, above which intelligence differences have little effect. The data of Tables V and VI show that freedom from error and limited range of report go together. The scatter diagrams for the relations between errors and total number of items reported show a low relationship with the normals and a moderately high one with the subnormals. In each case the relationship is greatest at the lower ends of the distributions. The slight positive correlation between errors and intelligence among the subnormals seems accounted for, therefore, by the fact that range tends somewhat to increase with intelligence and errors tend to increase with increase of range. With the normals, just as range and intelligence do not tend to vary together, so neither do errors and intelligence, nor range and errors.

Inspection of the scatter diagrams shows that, at the lower end of the distribution scale of intelligence, report and intelligence vary together much more closely than in the normal ranges of intelligence, and the coefficients indicate a moderate positive correlation in the case of the subnormals and a practically zero relationship in that of the normals. Thus, when all the *S*s are normal or better, individual variations, even those well above the norm, do not greatly affect report-ability; while, when all are subnormal, the deeper the subnormality, the greater is the tendency to poor report. This may be analogous to the situation with respect to intelligence and college grades, where the intelligence tests can predict marked failure or marked success, but cannot so definitely place the individuals who rank between. There were no markedly supernormal [illegible]

[illegible] but it may hold for the subnormal cases. Inspection of the scores of the subnormals indicates this conclusion. Or, to put it [illegible], a very poor level of intelligence may render an

individual unable to report either very much or very accurately; but when one begins to consider normal individuals, whose intelligence level lifts them above the *report-threshold*, as it were, individual variations in intelligence cease to be related with variations in report-ability.

Conclusions

(1) The comparative results obtained from oral and written reports indicate that they are of practically equal value in both range and accuracy, and that the method of taking written reports from children can be used with safety.

(2) The range of report, on object-card, picture and event, is greater for the normals than for the subnormals.

(3) The normal group shows, on the whole, a smaller percentage of report-error; but in the actual number of errors there is no outstanding and consistent difference between the two groups.

(4) When attestation is asked for, the normals attest more items and with a smaller percentage of error than the subnormals, on both narrative and interrogatory.

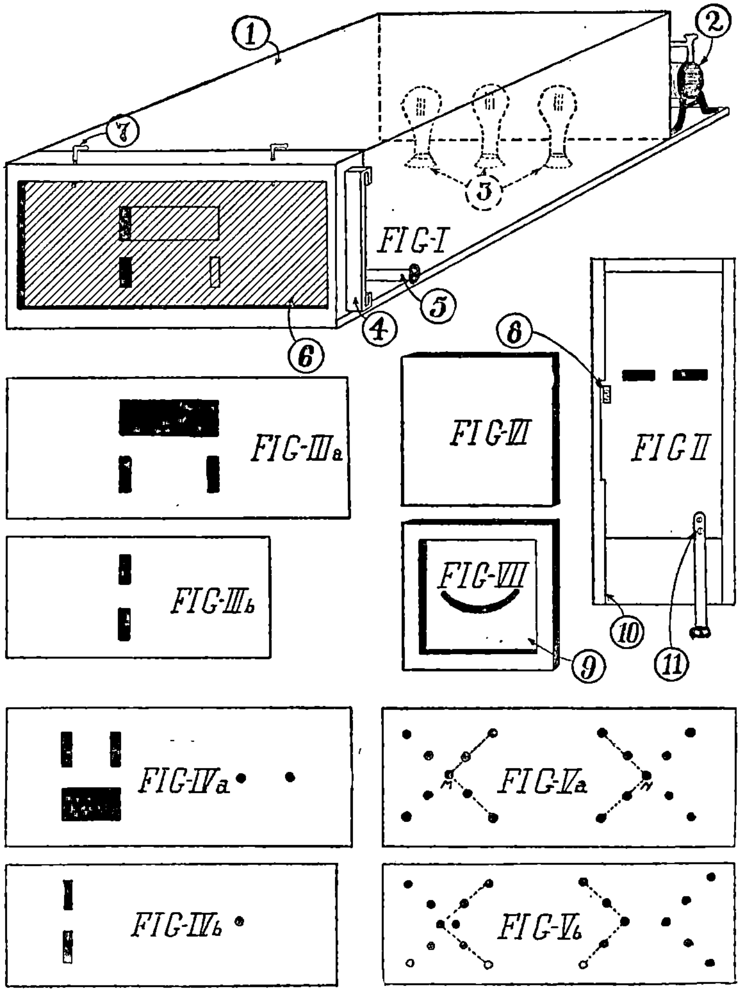
(5) Both groups describe the event with more minuteness, with a greater range of report, and with more apparent interest, than in the case of either object-card or picture. The event test would, therefore, seem to be the better type of *Aussage*-test for children.

(6) No errorless reports were found in any interrogatory. In the narratives of both groups, on all three kinds of report-test, a small percentage of errorless report was found, but the range of the errorless reports is uniformly less than that of the reports in which errors appear. The subnormals give a greater percentage of errorless reports, but with a more restricted range.

(7) The correlations between intelligence and report-ability, and intelligence and errors in report, show a tendency to zero or inverse relation for the normals, and a tendency toward a moderate positive relationship among the subnormals.

(8) Within the limits of the material and *S*s of this investigation, it may be concluded that intelligence is positively related to report-ability, in the sense that a certain degree of normality of intelligence is necessary for a normal range and accuracy of report; but that, when this degree of normality, or *report-threshold*, is present, there is no definite relationship between intelligence and ability to report.

fastened by means of thumb-screws (7). A frame, Fig. VII, holds a second translucent screen made of paper. On this paper screen may be placed certain opaque figures or *Nebenreize* which influence in an illusory manner the perception of the other presented stimulus-figures. This frame may be fastened in the front ledge in the same manner as the opaque shield.



Figs. III, IV and V suggest forms in which the slide-units may be made. In each case the figure with the subscript *a* is the front face, while that with the subscript *b* is the sliding member of the unit. In Fig. II the sliding member (11) is seen in place.

In Fig. III, and in the left design of Fig. IV, the stimulus-forms are so placed that real and apparent movement can be compared at the same

time. Thus, when the device is prepared as shown in Fig. I, pulling the handle (5) will cause the lighted stimulus-forms (shown in solid black), which are each 4 cm. by .5 cm. in size, to move across the translucent screen. The upper lighted form, showing real movement, will travel continuously across the 10 cm. slot. The lower stimulus-form, on the contrary, will be extinguished during the time required to move the 10 cm., until it flashes out again at the extreme right.

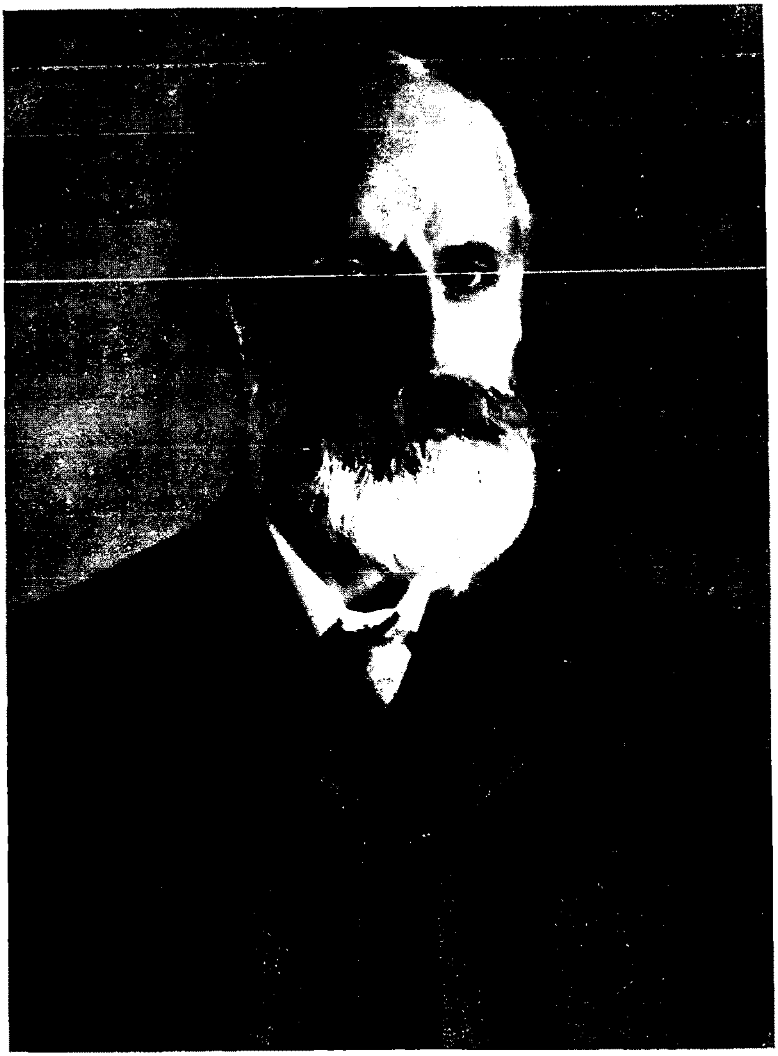
This device makes a most convincing demonstration; for under proper conditions of speed and illumination the *O* can seldom distinguish the real movement of the continuously seen member from the apparent movement of the two non-moving flashes. When the slide is moved slowly, however, the two discrete figures give no suggestion of movement, but appear merely as successive spatially separated light-forms. Similarly, when the slide is moved very fast, the two stimulus-forms are seen without motion, and simultaneously present. The distance between the stimulus-figures may be varied by inserting different slides into the box. Thus, there is a distance of 5 cm. between the forms shown on the left side of Fig. IV, while the comparable distance in Fig. III is 10 cm. When the left side of Fig. IV is to be observed, the right side is covered with the shield shown in Fig. VI.

The design on the right side of the slide-unit shown in Fig. IV under proper conditions gives the illusion of the movement of a circular light of 6 mm. through a straight path of 5 cm. By adding the translucent screen of paper, Fig. VII described above, this straight path will apparently be deflected and most *O*s will report the movement of the light through an arc. This experience may be compared to that of a marble rolling in a bowl.

Fig. V shows an adaptation of the Müller-Lyer illusion. The oblique or acute angles of the "wings" are shown upon the translucent screen by means of circles of light which are shown in black in the diagram. The circles which produce the acute angles when the slide is in position under the front face are shown conventionally by broken lines, which, of course, do not appear in the apparatus. When the slide is moved, the circles M and N appear to have first acute and then oblique "wings." If a fixation point is taken midway between M and N as the figure is changed the dots M and N, although motionless, will appear to jerk back and forth through a considerable distance. Different fixation-points give different illusions as this slide is moved; for example, the "wings" themselves may be seen to fold in and out.

Some little skill is required to move the slides in the various units at the desired speed, and for more exact work this factor might well be mechanically controlled by an electric motor. For determining new designs for the apparatus—those suggested here are merely samples of the large number that have been used in the work upon apparent movement and the "phi phenomenon"—small cardboard models similar to the slide-units described above may be made. Such little models give excellent apparent movement. The fundamental idea, indeed, for the apparatus described here was derived from a simple cardboard device of this kind which is described and figured by Wertheimer.[3]

[3]M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Z. f. Psychol.*, 61, 1912, esp. 262-265.



Yours sincerely
James Ward

JAMES WARD 1843-1925

By F. C. Bartlett, Cambridge University

On a Saturday afternoon in February 1914 a number of the late Professor Ward's pupils, colleagues and friends made him a public presentation of his portrait. No one who was there is likely ever to forget the address by which he acknowledged the gift. It was almost wholly autobiographical. Many men cannot tell, for they do not know, the truth about themselves. But I think that all of us who sat in that Cambridge lecture room, in the growing gloom of the late afternoon, and listened to him as he spoke in a perfectly simple, straightforward and unpremeditated way, knew that Dr. Ward saw clearly, and was able to show to us, the springs of his life and of his own activity.

He was a delicate boy and was early away from school. Then, often alone, he wandered about among the sand-hills of the Lancashire coast—he always said that few good things came from south of the Trent—watching the gulls wheel, their wings flashing in the sunlight, and searching the sand-dunes for the curious forms of life that lie hidden there. So there grew up in him a love for nature and wild life that not only remained strong in him to the end, but that had very much to do also with his philosophy. He was, indeed, a first-rate field naturalist, and he did not believe that people could write truly about things of the spirit which cannot be seen, if they knew nothing of the things of life that lie open to all men's vision. Later he took many walking and climbing holidays into the remoter parts of England and Scotland to see wild things in their homes. He made pets of all kinds of living creatures, insects, birds and beasts. Several generations of Cambridge students knew the famous "Jan," a collie dog with a beautiful head, who could "almost speak." Ward was equally enthusiastic about flowers and trees, and made a beautiful garden and worked much in it. The love of nature as it grew stimulated an intense interest in all observational and experimental science. It was this which made him hesitate for long between a career given to the study of life and one devoted to the study of mind. And when he made his choice, it was this which took him first of all to psychology.

James Ward's first venture into serious life was when, still rather young, he entered an architect's office in Liverpool. But he was not very happy there. He started, and did a great deal to maintain, a debating society among his colleagues. They

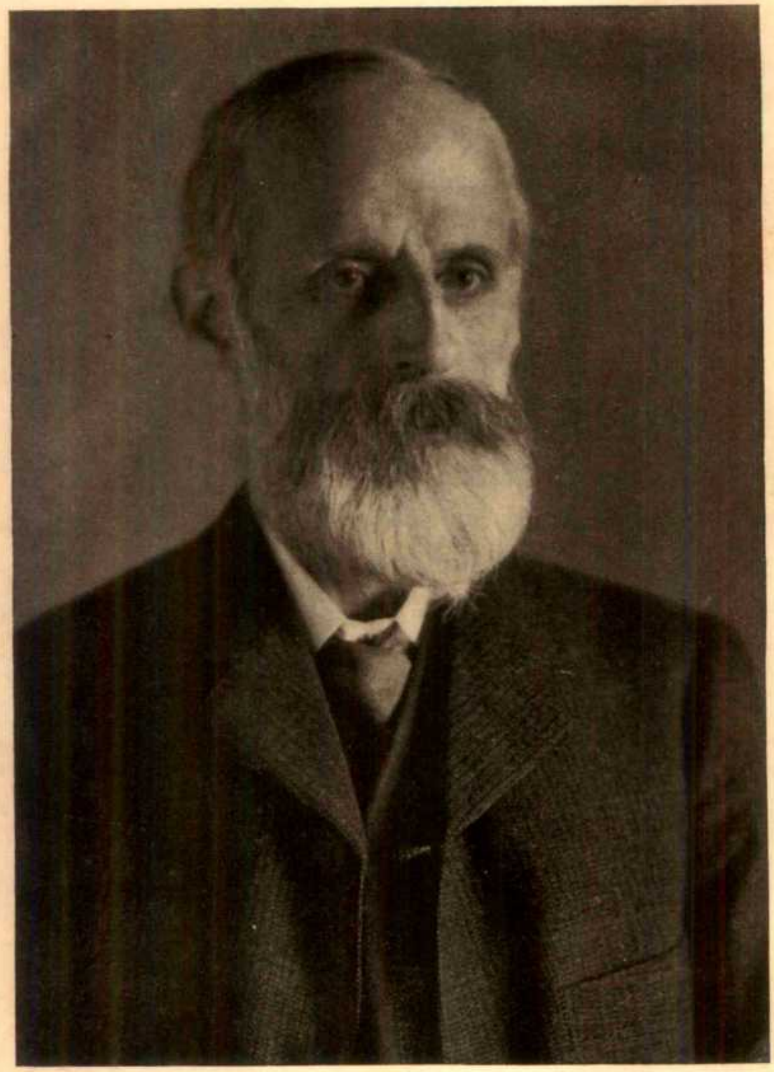
talked about all the usual subjects, and some unusual ones: Is Logic a Science or an Art?, various educational questions,—and once Ward indicated the trend of his political development by stoutly upholding universal suffrage for women. They said to him: "You are not much of a draughtsman, but you have the gift of the gab. Why not be a lawyer or a parson?" And a parson he determined to be.

He became a student at the Congregational Training College which was then at Spring Hill, near Birmingham, and remained there nearly six years. His study of theology stimulated and developed the philosophical interests which were later to become dominant in his life. Towards the end of his student career he came to Cambridge to preach at the Congregational chapel and was invited to its pastorate. But about the same time he obtained a studentship which gave him the chance of study in Germany, and he proceeded to Berlin and later to Göttingen. There, although he continued to pursue his theological studies, he became more and more interested in physiology and philosophy, and worked at the first with Ludwig, and at the second with Lotze.

Lotze attracted and influenced him profoundly. The wide scientific interests and attainments of the elder man, his power of keen psychological analysis, his constant reiteration of the fundamental importance of life and activity, and the ethical strain underlying his philosophy all appealed strongly to the personality of Ward and became an abiding force in his own life and thought.

In due course he returned to England, to find the Cambridge pastorate still open; and the invitation to him to fill the post was renewed. He accepted with much hesitation, feeling that he no longer held orthodox religious views; and he refused to be ordained. For nine months he was a Nonconformist minister. Again he was not happy. His theological views were far in advance of those of his congregation and he was too intellectually honest to attempt any disguise of the difficulty. He resigned the pastorate, and became a Non-Collegiate student at the University. Then he learned that an open scholarship in "Moral Science" was offered by Trinity College. He entered for the examination, won the scholarship, and thereafter his work lay mainly in academic circles.

This was in 1872. In 1874 he became M. A. of London University, winning the gold medal in philosophy. The year following he was placed by himself in the first class of the old Moral Sciences Tripos at Cambridge, and in the same year was elected to a Fellowship at Trinity College. In 1881 he became a Lecturer at Trinity, and in 1897 was elected to a Chair in



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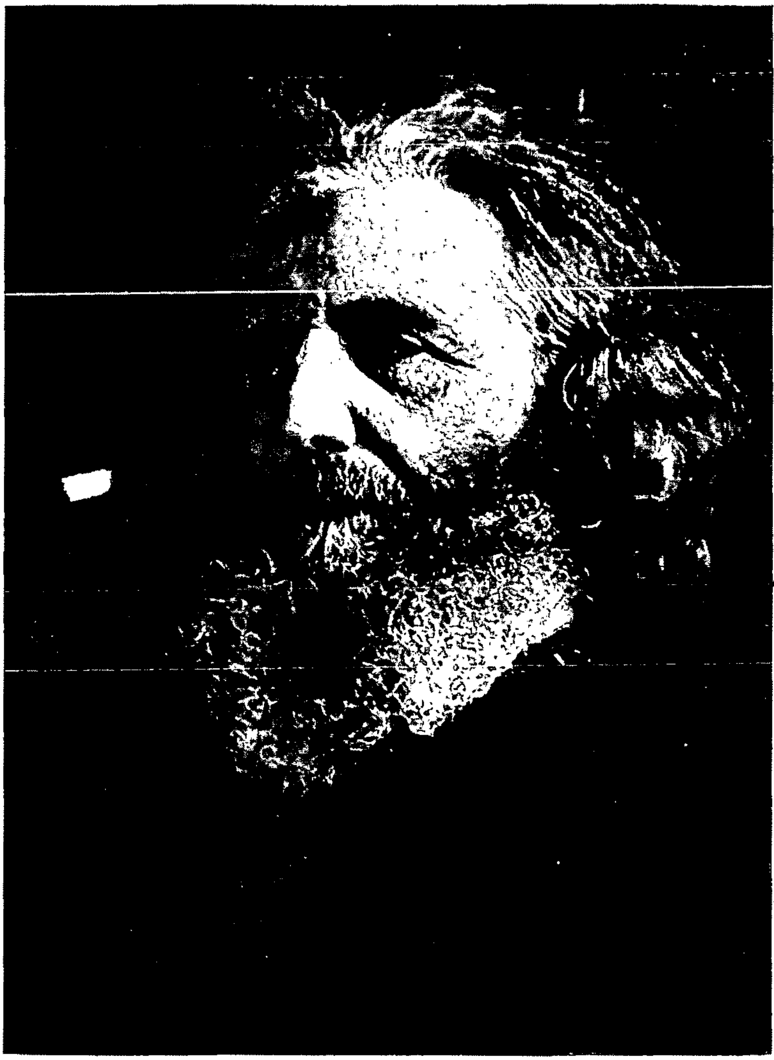

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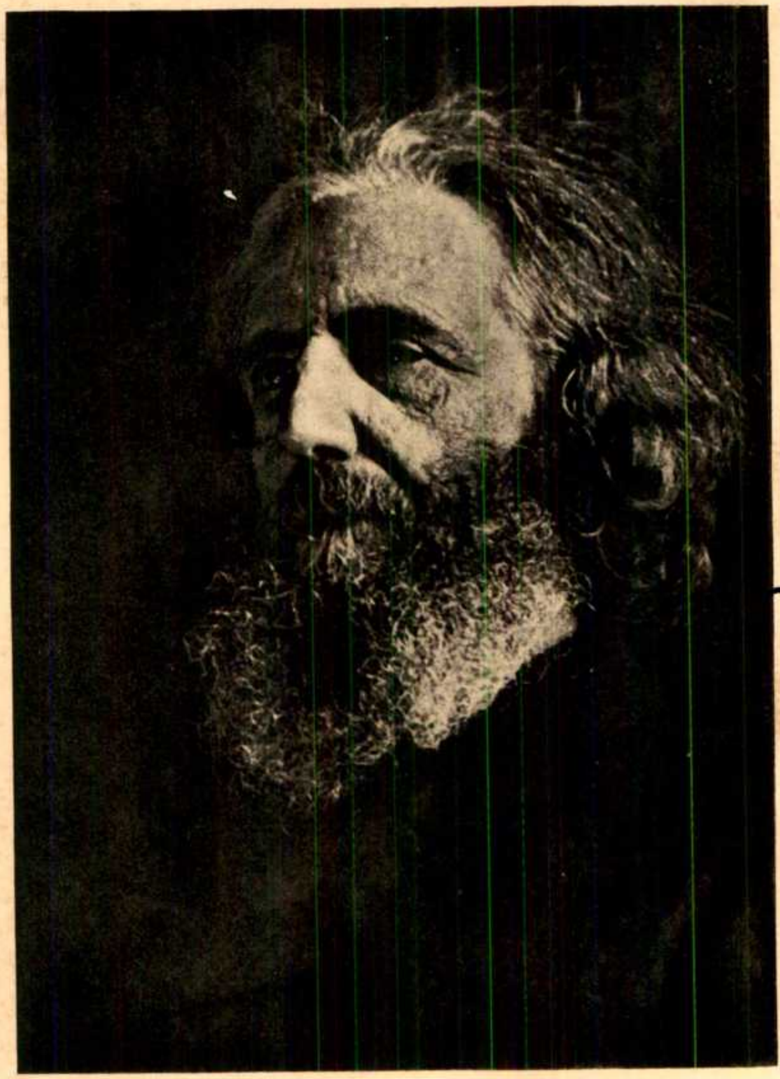

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Mental Philosophy and Logic, a post which he held until his death on March 4th, 1925.

This is the story which he told, lingering over the early part of it, hurrying over the second and academic part, on that late afternoon in February. "What I am," he said at the end, "two men have made me: Hermann Lotze and Henry Sidgwick."

This is not the place, and I am not the writer, to attempt to say anything in detail about Ward's philosophical and metaphysical teachings. But it is an interesting thing to consider the course of his development as it is indicated by the Bibliography of his published writings to that date which was printed in this JOURNAL in 1912.[1] The earliest papers dealt, all of them, with biology, physiology and experimental work in psychology. As early as 1882 his interest seemed to be turning more towards problems of general psychological theory, and the few years following that saw the development of his complete psychological system: the famous *Encyclopaedia Britannica* article was first published in 1886. In 1896 he began his first period as Gifford Lecturer, and his work became more metaphysical in its bent. From then, to the conclusion of the period covered by the published Bibliography, philosophy in the broadest sense took up more and more of his thought and energies. In the end it absorbed him almost wholly, so that some years before his death he even gave up lecturing upon psychological topics, and confined his public lectures to metaphysics. The history of his work remarkably exemplifies the nature of his outlook upon the world. The study of life is a stepping-stone to the study of mind; the study of the facts of mind is a stepping-stone to an all-embracing philosophy. The last without the two first is barren, the two first without the last are incomplete.

This view is foreshadowed even in the dissertation which helped to win him his Fellowship at Trinity College in 1875. A part of the thesis was published later in a privately printed essay on *The Relations of Physiology and Psychology*. The work is said to have caused his referees very great heart-burning. They knew and could make nothing of its subject-matter, but were broad-minded enough to see that Ward ought to be given a chance to pursue his interests. It was, in fact, chiefly concerned with the development, by Weber and Fechner, of an experimental and quantitative psychophysics. Ward expounded and criticised—mainly expounded—their work with remarkable clarity and incisiveness, and with unmistakable enthusiasm. Thus he began his official career in Cambridge by a

[1] 23, 457-460.

study of that intractable problem which, though in his case it was approached through the avenues of science, almost inevitably leads to philosophical speculation, that of the relation of body and mind. And every student who has attended his lectures on psychology knows with what fascination Professor Ward again and again approached that subject. There was another part of psychology which had for him a particularly irresistible attraction: the study of memory. And in both cases the underlying interest seemed to be the same. Here, if anywhere, was to be found actual evidence of the integrity and independence of mind, and of the indissoluble character of individual experience.

It is this philosophical motivation, underlying almost every part of Dr. Ward's psychology, that makes it peculiarly difficult to estimate his exact position in relation to contemporary psychology in this country. On the face of it, it would perhaps appear that those psychologists whose mode of approach is scientific rather than philosophical have tended to ignore his work, and have remained but little influenced by his thought. Undoubtedly he appeared somewhat antipathetic towards recent movements in psychology. He regarded the mass of experimental work as on the whole chaotic, more often than not ill-planned, and directed by no great and governing ideas. The tendency to preoccupation with the psychopathological also seemed to him to be definitely misleading and dangerous. But I believe it true to say that, to a degree very rare indeed within the limits of a man's own life, a great deal of his most important work was assimilated, and passed so completely into current ways of thinking that its source often remained unappreciated. For example, his onslaught upon atomism in psychology had a more complete and far-reaching influence in scientific than in philosophical circles. But because he still continued to talk predominantly in terms of presentations, because he remained far more interested in the materials of mental life than in the reactions themselves, because, in spite of his activism, the bulk of his discussions concerned the nature of the content of intellectual processes, scientific psychologists have often tended to look upon him as an intellectualist who treated all too lightly the actual springs of conduct.

Thus views which in some respects look as if they might have come straight out of the pages of his *Encyclopaedia* article or of the *Psychological Principles* have been put forward as if they are quite new. Nowadays most psychologists tend to lay stress upon functions and their conditions. But it was he who, more than any other English thinker, insisted upon the importance of "the subject acting." He was alive to the significance of bias, interest, specific reaction-tendencies of different kinds.

He did not ignore the affective side of life nearly as much as is commonly imagined; and if he held that the current anxiety to explain almost everything in terms of feeling is symptomatic of the excitement of a war and a post-war period, he was not alone in that view.

It is useless to speculate as to what might have happened if he had held his psychology more free from his metaphysics. As it is, the first to him was a preface to the second, and it can hardly be doubted that in some instances the text was allowed to dictate the introduction. But he, first among English psychologists, clearly saw the mental life as an integrative process throughout. In this he was not merely in line with the best of the more recent developments in the scientific study of life and mind, but he may be said to have prepared the way for those developments and to have contributed powerfully towards them.

I should like to bring this brief notice to a close by a few remarks about Ward as a teacher. The first time I ever saw him was at a lecture, and I remember it well. He came slowly into the small room—there were only about eight or nine students—his long, spare form struggling into his gown, his very keen and penetrating eyes taking us all in. It was the first lecture of the year, and there was a certain ritual to go through before he could begin to talk. He must find out who we were, and what we proposed to do, and he must discourage us a little so as to put us into a proper frame of mind. "Well, I don't know why you come to me; I don't know what you expect to get out of me; but whatever it is I expect you'll be disappointed." Then he sat down. Out came half a sheet of note-paper which he by no means needed, and for an hour he talked and we were literally held in spell. There was no hesitation, the right word seemed always to come, the illustrations were frequent, brilliant and human, the asides and reminiscences were full of fun. There was little formality. Nobody could do much in the way of taking notes. But I do not think he ever sacrificed a serious point to make a quip, although he never strangled humor to preserve solemnity.

That was not all. We met him at other times in his own home. Then, when he was in the vein, he would talk, as few can do, about anything and everything, but best of all about flowers and trees, and birds and beasts and people. Two things shone through all the time. The first was his keen interest in life and its great march forward; the second was his belief in the value of the individual and his intolerance of all bars to freedom.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

L. Further Experiments on Directed Recall as a Test of Cheerful and Depressed Temperaments

By M. F. Washburn, Lilian Harding, Henrietta Simons, and Dorothy Tomlinson

In this Journal, 30 (1919), 302-304, there were reported some experiments from our laboratory whose object was to see whether any correlation would appear between temperamental tendencies to cheerfulness or depression, and tendencies to recall pleasant or unpleasant ideas in connection with verbal stimuli, when the *O* was instructed to continue thinking, after the stimulus word was given, until either a pleasant or an unpleasant idea occurred to her, in case the first suggestion was neutral in affective tone. The results showed enough correlation with estimates of the *O*s' temperamental tendencies by their friends to encourage further study of the method.

In the present investigation, we proceeded in the following way. Each of the members of a large class in psychology was asked to judge herself by using one of the four terms: steadily cheerful; variable tending to cheerfulness; variable tending to depression; steadily depressed. From this large group, two approximately equal groups were formed, one containing those who had rated themselves as steadily cheerful, the other those who had judged themselves to be steadily depressed and those who had called themselves variable tending to depression. There were only very few who classed themselves as steadily depressed; hence the depressed group had to include the 'variable-depressed' class. The cheerful group numbered 33; the depressed group, 34.

In addition to these self-judgments, for each *O* the judgments of three friends were obtained, using the same descriptive terms. It was then possible to rate the *O*s numerically with regard to cheerfulness. The judgment 'steadily cheerful' was counted as four points; 'variable tending to cheerfulness' as three points; 'variable tending to depression', two points; 'steadily depressed', one point. If an *O* was judged steadily cheerful by herself and all three of her friends, her cheerfulness was rated at 16 points, that is, 4 4-point judgments; if considered steadily cheerful by herself and two of her friends and variable tending to cheerfulness by the third friend, her total rating was 15 points; and so on.

The experimenting was done as follows. Three series of 50 stimulus words each were prepared, an effort being made to avoid words with an obvious pleasant or unpleasant suggestion. An *O* was given one of these 3 series on each of 3 consecutive days. In the 1919 investigation there were 5 series, used on 5 consecutive days. The reason for experimenting on more than one day was of course to minimize the influence of temporary mood. The preliminary instructions were as in 1919: "When I pronounce a word to you, observe what idea that word first calls to your mind, and report whether it is a pleasant or unpleasant idea. If it seems neither pleasant nor unpleasant, but indifferent, continue thinking until a pleasant or an unpleasant idea is suggested and report which it is."

The results were scored for each *O* in terms of the total number of pleasant associations she experienced in the 3 series.

Results

(1) *In groups based on self-judgment.* We have seen that our original selection of *O*s comprised those who judged themselves as steadily cheerful, 33 in number; and those who judged themselves as either steadily depressed or variable tending to depression, 34 in number. In these two groups combined, the median score in number of pleasant associations was 97.5. Of the 33 in the cheerful group, 21, or 66%, had more than the median number of pleasant associations. Of the 34 in the depressed group, 22, or 64.7%, had less than the median number of pleasant associations.

The highest quartile in number of pleasant associations included those who had scores of 114 and over. Sixteen out of 17 or 94% of the *O*s who fell in this quartile belonged to the cheerful group. The lowest quartile included those who had scores of 80 and less. Twelve out of the 17 belonging in this quartile, or 70%, were in the depressed group.

(2) *In groups based on self-judgment plus judgment by others*; that is, on cheerfulness scores as above explained. The median cheerfulness score was 12.5 points. Of those who were above the median score in number of pleasant associations, 21, or 62.6% of half the number of *O*s, were above the median cheerfulness score. Of those who were below the median in number of pleasant associations, 22, or 65.6%, were below the median cheerfulness score.

The highest quartile in cheerfulness scores included those with scores of 15 and 16 points. Eleven out of the 17 *O*s, or 64.7%, who were in the first quartile in number of pleasant associations, were also in the first quartile in cheerfulness scores; 13, or 76.4%, were above the median. The last quartile in cheerfulness scores could not be found, as there were 15 persons with scores of 9 or less and 20 with scores of 10 or less. Of the 15 with scores of 9 or less, only 33% were in the last quartile, and 60% below the median, in number of pleasant associations.

It thus appears that, if we use the median score in pleasant associations as the dividing line, we shall find about two-thirds of the cheerful group lying above it. There is less correlation between the depressed group and the number of pleasant associations. This may well be due to the fact that our depressed group was largely composed of persons who had rated themselves not 'steadily depressed' but 'variable tending to depression', while the members of the cheerful group had all rated themselves 'steadily cheerful'. Or, to put the matter more accurately in terms of cheerfulness scores, the median for cheerfulness scores ranging from 4 to 16 inclusive in a group of 67 persons should have been about 10.9, and our actual median was 12.5.

Some Galvanometric Tests

In the case of about half our *O*s we took readings with a d'Arsonval galvanometer during the experiments. Our procedure was the simple one of tying cylindrical electrodes securely to the palms of our *O*s' hands, who sat in a comfortable position, and recording the maximum change of position of the bright disk that occurred while the *O* was recalling the pleasant or unpleasant experience. These deviations for pleasant and unpleasant recalls were recorded in separate columns, and for each *O* and each set of stimulus words the average 'pleasant' deflection and average 'unpleasant' deflection were found. For the cheerful group of *O*s 93 such averages were obtained, of which 40% were of more than 10°. For the depressed group, 72 averages were found, of which only 4.1% were more than 10° in extent. In the cheerful group, 35% of the average deflections were of 5° or less; in the depressed group, 66% were of 5° or less.

Of the 10° or more average deflections in the cheerful group, 17 were averages for pleasant associations and 21 were averages for unpleasant associations. Of the 3 cases of averages amounting to 10° or more in the depressed group, all were averages for pleasant associations. Of the 5° or

less average deflections in the cheerful group, 16 were averages for pleasant associations and 17 were averages for unpleasant associations. Of the 5° or less average deflections in the depressed group, 25 were averages for pleasant associations and 23 were averages for unpleasant associations.

These figures, as far as they go, suggest the following conclusions with regard to the galvanometer effects: (1) the cheerful group of *Os* produces more effect on the galvanometer than does the depressed group; (2) there is no difference in effect between pleasant and unpleasant associations.

LI. Memory Revival of Emotions as a Test of Emotional and Phlegmatic Temperaments

By M. F. Washburn, Frederica Giang, Margaret Ives, and Martha Pollock

A. *Revival of Single Incidents*

Two previous Studies from this laboratory (this Journal, 34, 1923, 99ff.; 35, 1924, 113ff.) dealt with the process of recalling emotions, and compared the intensity of recall, the speed of recall, and the remoteness in time of the original experience, in the case of different emotions. In the present Study our aim is to see whether the method used in these earlier investigations may not furnish some data for the psychology of individual difference; in particular, whether it can serve to some extent as a test of emotional and phlegmatic temperaments.

At the outset, the members of a large class in psychology, all young women, were asked to judge themselves, using one of the following terms: emotional; rather emotional than calm; rather calm than emotional; calm. On each of those who complied with this request, the judgments of 3 friends were obtained, the same terms being used. Where the judgment 'emotional' was made on an individual, either by herself or by one of her friends, 4 points were credited to her score of emotionality (if the word may be pardoned); when the judgment 'rather emotional than calm' was made, it counted 3 points; 'rather calm than emotional' counted for 2 points, and 'calm' 1 point. Thus an individual's score might be anything between 16 (4 judgments of 'emotional'), and 4, produced by 4 judgments of 'calm'.

There were 3 *Os* whose total score of emotionality was 16 points each; one whose score was 15 points; 3 whose scores amounted to 14 points each; 11 whose scores were 13 points each; and 11 whose scores were 12 points each. There were 3 *Os* whose scores were 4 points each; 4 whose scores were 5 points each; 12 whose scores were 6 points each; 6 whose scores were 7 points each, and 9 whose scores were 8 points each. It will be seen that we could not, without a random selection from the 8-point group, make equal groups of emotional and calm *Os*. We experimented on all of the above *Os*; that is, on an emotional group of 29 comprising those with scores from 16 to 12 inclusive, and on a calm group of 34, comprising those with scores of from 4 to 8 inclusive. To obviate the disadvantage of inequality between the groups, we stated our results in terms of percentages of each group.

Each *O* was experimented on according to the method used in the Studies above referred to: that is, she was given the following directions: "I want you to recall some occasion when you were very angry, (or "felt great joy," or "felt very much frightened"). Let the emotion develop again as far as it will, so that as far as possible you relive the incident and its accompanying emotion. Do not speak, but give a signal when you feel the emotion as fully as you can in this revived form."

A stop-watch was started when the *O* began to fulfill these instructions, and stopped when she signalled that the recall was complete. She was then asked whether the revived emotion was as intense as the original one, in which case it was credited with 4 points of intensity; whether it was somewhat less intense, counting 3 points; very much less intense, counting 2 points; or whether there was merely the recollection that the emotion had once been felt, which counted 1 point. She was also asked how long ago the original incident had occurred. Each *O* thus recalled an incident causing anger, one causing joy, and one causing fear.

Results

I. Intensity of Recall. (*a*) *Anger.* There were 19 cases of the revival of anger at intensity 1, the weakest grade. Of these, 8 were made by the *Os* of the emotional group, constituting 27.5% of that group; and 11 by members of the calm group, constituting 32.3% of that group.

There were 27 cases of the revival of anger at intensity 2: of these 10 were made by emotional *Os*, or 34.4% of that group, and 17 by calm *Os*, or 50% of that group.

There were 18 cases of the revival of anger at intensity 3: of these 10 were made by emotional *Os*, or 34.4% of that group, and 8 by calm *Os*, or 23.5% of that group. There was no case of the recall of anger at intensity 4.

It thus appears that, while there is some tendency for the calm *Os* to recall anger with less intensity, the difference is far from being sufficient to separate the groups.

(*b*) *Joy.* There was only one case of the recall of joy at the lowest intensity, 1. This was by a member of the calm group. In 16 cases joy was recalled at intensity 2; 3 of these recalls were made by emotional *Os*, or 10.3% of the emotional group; 13 by the calm group, making with the *O* who recalled joy at intensity 1, 41.1% of the calm group recalling joy at an intensity less than 3. In 43 cases joy was recalled at intensity 3; these comprised 25, or 86.2%, of the emotional group, and 18, or 52.8%, of the calm group. In 3 cases, one emotional and two calm, an intensity of 4 was reported. Summarizing these results: 10% of the emotional group and 41% of the calm group revived joy with an intensity of 2 or less: 80% of the emotional group and 59% of the calm group revived it with an intensity of 3 or more. The best dividing line for test purposes, so far as these results go, would be revival at an intensity of 2 or less; a group thus formed would contain about 4 times as many calm as emotional *Os*.

(*c*) *Fear.* Fifteen persons revived fear with an intensity of only 1: 7 of these were emotional *Os*, 24.15% of the emotional group, and 8 were calm, 23.5% of the calm group. Twenty-five persons revived fear with an intensity of 2: 12 of these were emotional, 41.3% of the emotional group, and 13 were calm, 38.2% of that group. Twenty persons revived fear with an intensity of 3: 8, or 27.6%, of the emotional group and 12, or 35.3%, of the calm group. Three persons, two emotional and one calm, revived fear with an intensity of 4. To summarize: 24% of the emotional group and 23% of the calm group revived fear with an intensity below 2: 34% of the emotional group and 38% of the calm group revived it with an intensity above 2.

Reviving joy at an intensity of 2 or less is thus the only approximation [illegible] to be obtained from introspective reports of the intensity of a revived emotion of [illegible]

II. Speed of Recall. As was noted in the earlier Studies made on revived emotions, two individuals can be only roughly compared in regard to the speed with which they recall emotions [illegible] be sure that they will consider that the same degree of development of

revived emotion constitutes that full development at which they are instructed to give the signal. Our results show no significant difference between the emotional and the calm groups as regards speed of recall.

III. Remoteness of Original Experience. (a) *Anger.* Twenty-seven *Os* recalled anger from a date less than one year before: 18 of these were emotional, constituting 62% of the emotional group; 9 were calm, constituting 26.4% of the calm group. Twenty-three *Os* recalled anger from a date two years or more before: 6 of these were in the emotional group, constituting 20.6% of that group; 17 were calm, constituting 50% of the calm group. Thirteen *Os* recalled anger from a period of more than 5 years before the time of recall: 2 of these were emotional, that is, 6.9% of the emotional group, 11 were calm, that is, 32,3% of the calm group.

It appears that there is a fairly well-marked difference between the groups as regards the recency of the anger-incidents recalled. The proportion of calm *Os* recalling anger from two or more years is more than twice as great as that of the emotional *Os* recalling from the same time-distance: for a remoteness of 5 or more years the proportion of calm *Os* is nearly 5 times that of the emotional *Os*.

(b) *Joy.* Here the diagnostic mark is recall from very recent date. There were 17 *Os* who recalled joy from a period less than one month before: of these 11 were emotional, constituting 37.9% of the emotional group, and 6 were calm, constituting 17.6% of the calm group. Recall from a remote period was about equally distributed between the two groups: 13 emotional *Os* recalled joy from a period of over 6 months before, constituting 47.8% of the emotional group, and 17 calm *Os*, or 50% of the calm group, did likewise.

(c) *Fear.* Here also recall from a recent period was fairly diagnostic. Twenty-six *Os* recalled fear from a period of less than one year; of these, 18 were emotional, constituting 62% of the emotional group, and 8 were calm, constituting 23.5% of the calm group. Twenty-eight *Os* recalled fear from a period of more than one year; of these 10 were emotional, constituting 34.4% of the emotional group, and 18 were calm, constituting 52.9% of the calm group.

B. The Number of Emotionally Toned Incidents Recalled. If an *O* is asked to recall as many occasions as he can when he was angry, or felt joy, or was frightened, will the number be greater if the *O* belongs to the emotional group?

Thirty persons whose emotionality score was 12 or above, and 35 whose emotionality score was 8 or less, were experimented on as follows. They were each asked to recall as many instances as they could when they had been very angry, and to give a signal as each one occurred to them. A stop-watch was started when they began the process of recall, and stopped when they declared they could think of no more cases: the number of signals given and the total time occupied in recall were recorded. The same procedure was followed for the recall of joy and fear.

Results

In the emotional group 11 persons, or 36% of the group, recalled more than 10 *angers*: in the calm group 9 persons, or 25% of the group, recalled more than 10 angers.

In the emotional group 5 persons, or 16% of the group, recalled fewer than 5 angers: in the calm group 17, or 50% of the group, recalled fewer than 5 angers.

In the emotional group 16 persons, or 53% of the group, recalled more than 20 *joys;* in the calm group 19 persons, 54% of the group, recalled more than 20 joys.

In the emotional group 4 persons, 13% of the group, recalled fewer than 10 joys; in the calm group 15 persons, 43%, recalled fewer than 10 joys.

In the emotional group 11 persons, 36%, recalled more than 15 *fears*; in the calm group 6 persons, 17%, recalled more than 15 fears.

In the emotional group 4 persons, 13%, recalled fewer than 5 fears; in the calm group 11 persons, 31%, recalled fewer than 5 fears.

Considering the total of emotional recalls: in the emotional group 16, 53%, recalled more than 40 cases of emotion: in the calm group 17, 48%, recalled more than 40 cases.

In the emotional group 3, 10%, recalled fewer than 20 cases of emotion: in the calm group 13, 37%, recalled fewer than 20 cases.

In the emotional group 5, 16%, recalled fewer than 30 cases of emotion: in the calm group 21, 60%, recalled fewer than 30 cases.

These results so far as they go indicate the following conclusions. (1) A larger proportion of emotional than of calm *O*s recall many angers and many fears: about the same proportion of emotional as of calm *O*s recall many joys. This fact, along with the absolutely greater number of joys recalled, should be considered in connection with the highly favored situation of our *O*s, all young, well, and in good economic circumstances.

(2) A decidedly larger proportion of calm than of emotional *O*s recall few angers, joys and fears.

(3) The best diagnostic test is a total recall of fewer than 30 emotions. The proportion of calm *O*s recalling this number is more than 4 times as large as the proportion of emotional *O*s.

LII. Galvanometer Experiments with Revived Emotions as a Test of Emotional and Phlegmatic Temperaments

By M. F. Washburn and Frederica Pisek

Two groups of 21 *O*s each, a calm group having emotionality scores of 7 or less, and an emotional group having scores of 12 or more, were experimented on as follows. The *O* was comfortably seated, and cylindrical brass electrodes attached to a d'Arsonval galvanometer were securely tied to the palms of her hands. She was then asked to recall anger, joy, and fear as reported in the preceding Study, and to report the intensity of the revived emotion and the remoteness of the original experience. The *E* noted the extreme deflection of the galvanometer during the process of recall.

When the maximum deviations for all the recalls obtained were arranged in order of their size, it was found that, among those *O*s who gave deflections above the median amount, 80% were emotional and 20% calm: for *O*s giving deflections below the median amount these figures were of course reversed.

Of the *O*s giving deflections in the highest quartile, 90% were emotional: of those giving deflections in the lowest quartile, 70% were calm.

There were 21 *O*s whose maximum deflections for the three emotions, anger, joy, and fear, averaged above 10°. Of these 80.9% belonged to the emotional group.

[illegible] Of these 75% belonged to the calm group.

[illegible] to see whether and how far the galvanometer disturbance confirms the *O*s' report as to the intensity of the revived

emotion. In the case of the calm group, no correlation appears. If we select in the case of each O that recall which gave the largest galvanometric deflection and record the degree of intensity (1, 2, 3, or 4) which she introspectively ascribed to it, the average of such intensities is for the calm group 2.02. If we select, similarly, the recalls which gave the smallest galvanometric deflections and average their intensities, we find that for the calm group this average is 2.18; a difference in the wrong direction, though negligible in amount. In the case of the emotional group a positive correlation appears. The average intensity of their recalls giving maximum deflections is 2.79; of those giving minimum deflections it is 2.17.

Possibly an emotional person is a better introspective judge of the intensity of emotions, since he experiences them oftener and through a higher range of intensities.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF YALE UNIVERSITY

IV. On Inhibition of the Differential Threshold in Accordance with Heymans' Law

By John E. Anderson

In 1899 Heymans[1] formulated a general law of inhibition, which stated that the inhibitory power of a stimulus, measured by the stimulus whose effect it can just completely inhibit, increases in direct proportion to its intensity. By a series of very careful and complete researches, during the following years, he was able to demonstrate the validity of this law in a number of sense-fields and to work out many of its implications, including its application to the differential threshold. The major part of his work was done with a single *S*.

Spencer[2] recently took up the problem and was able to demonstrate that the quantitative relation held for the simple threshold of the white rat. He measured the simple threshold of 5 rats for a white light, in the absence of and in the presence of another or inhibiting light (the inhibiting light being of two intensities, one double the other), and found that the simple threshold was a linear function of the intensity of the inhibiting stimulus.

In Heymans' terminology, the inhibiting or concurrent stimulus is called the active stimulus, and the inhibited stimulus (the one for which the thresholds are obtained) is called the passive stimulus. His law states that the threshold for the passive stimulus increases directly with the intensity of the simultaneously presented active stimulus.

Heymans, in his original work, used specially designed rotating discs for both active and passive stimuli, obtaining the thresholds by the method of minimal changes. Later some of his work was done with slide photometers. For our purposes the slide photometers seemed to offer much greater ease of adjustment. A device was constructed consisting of three parallel slide photometers, in blackened tunnels, which illuminate apertures covered with milk glass. When the slides are moved toward or away from the apertures the intensity of the light increases or decreases in accordance with the law of inverse squares.

When the *S* seated in a dark room looked into a hood 1 m. from the apertures, he saw before him 3 circular fields of light, a central one 3 cm. in diam. (the active stimulus) on either side of which were two smaller fields, each 1 cm. in diam. (the passive stimuli). The distance between the centers of the fields was 7.3 cm. His task was to observe the two small fields and to report the instant he noticed any change in intensity by stating which of the two was the brighter. At the beginning of the judgment, the two outer lights were objectively equal in intensity. The *E* moved the slide corresponding to one of the two outer lights away from the *S* a unit at a time, thus decreasing the intensity. The slide was marked off in logarithmic units, so that the intensity of the light could be decreased by uniform steps. When the judgment was made, the *E* stopped moving the slide,

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[1] G. Heymans, *Z. f. Psych. u. Phys. d. Sinnesorg.*, 21, 1899, 321-359; *Z. f. Psych.*, 53, 1909, 401-415.

[2] L. T. Spencer, *Jour. Comp. Psych.*, 3, 1923, 389-408.

and turned a switch which cut off all three lights. After recording the position and resetting the slides, he turned on the switch, the click being the signal that the apparatus was ready for the next judgment. The method is, then, one of the continuous change of one stimulus moving from objective equality in the direction of a lower intensity, the additional requirement being set up that the judgment which stimulus was the brighter must be correct. As the *S*s were cautioned to be relatively sure of their judgments practically no incorrect judgments were made, the maximum number of errors being 8 by one *S* in 100 judgments. When an incorrect judgment was made the trial was repeated at the close of the series. Fifty judgments were made a day, in 5 series of 10 each, the series being separated by a rest period. A preliminary practice period was given before the experiments, and a short practice was given before each experiment. Each series of 10 judgments contained 2 judgments at each intensity of the active stimulus, the order of appearance for the different intensities being arranged so that practice and adaptation effects due to the temporal arrangement were balanced. The standard passive stimulus remained at the same intensity throughout each experiment.

Measurements of the intensity of stimuli and of the thresholds are expressed in terms of comparable light-units, a 60 watt Mazda lamp at 10 cm. from the milk glass aperture being designated 1,000 units. The slides of the photometers were marked off in these units. The source-lights were three 60 watt tungsten lamps, between which the maximum difference was .050 light-units. Two sets of experiments were made, the first set on 3 *S*s, with the center light or active stimulus at 40, 80, 120, 160, and 200 units respectively, with the standard outer light or passive stimulus at 111 units. Although 2 of these active stimuli are below the intensity of the passive stimulus as measured in light-units, the greater area of the active stimulus prevents us from concluding that the stimulating value in these cases is less than that of the passive standard stimulus. The second set of experiments was done on 5 other subjects, with the active stimulus at 100, 200, 300, 400, and 500 light-units respectively and the standard passive stimulus at 50 units. In the first set of experiments each *S* worked one day until a total of 50 judgments or 10 for each intensity of the active stimulus was obtained. In the second set of experiments each *S* worked 2 days, so that a total of 100 judgments or 20 for each intensity of the active stimulus was obtained.

The results of the experiments are presented in the following Tables.

Experiment 1. Differential Thresholds with Standard Passive Stimulus at 111 Light-Units

Intensity Active Stimulus	40	80	120	160	200
Mean (3 *S*s)	19.7	22.9	25.8	27.6	31.3
Straight Line of Best-Fit Measures	19.86	22.66	25.46	28.26	31.06

Experiment 2. Differential Thresholds with Standard Passive Stimulus at 50 Light-Units

Intensity Active Stimulus	100	200	300	400	500
Mean (5 *S*s)	9.11	9.96	10.78	11.95	12.99
Straight Line of Best-Fit Measures	9.00	9.98	10.96	11.94	12.92

For the line of best-fit values of the first experiment, the differential threshold with the standard passive stimulus at 111 light-units is increased by 2.8 light-units for each increase of 40 light-units in the active stimulus from 40 to 200 light-units. In the second experiment with the standard passive stimulus at 50 light-units, the differential threshold is increased by .98 light-units with every increase of the active stimulus by 100 light-units between 100 and 500 light-units.

As an incidental check on the reliability of the values obtained, the differential threshold with no active stimulus present was calculated by extending the line of best fit to the zero abscissa. For the first experiment with the standard stimulus at 111 light-units a differential threshold of 17.06 light-units is thus obtained; and for the second experiment with the standard stimulus at 50 light-units, a threshold of 8.02 light-units. In the first experiment the threshold is 15.34% of the standard stimulus, in the second 16.04% of the standard stimulus,—a striking agreement in the two experiments as to the ratio of the differential threshold to the stimuli compared.

The introspective reports of the *S*s are interesting, in that several were unaware of the fact that the central light or active stimulus had changed at all during the series of experiments, but believed that the central light had remained constant while both the passive stimuli changed.

Summary and Conclusion, The differential threshold for light-stimuli in the presence of an inhibiting stimulus was measured by the method of continuous change. The results obtained on 8 *S*s show that the differential threshold varies directly with the intensity of a concurrently presented visual stimulus within the range of stimuli studied.

BOOK REVIEWS

La psychologie des femmes. By G. Heymans. Tr. by R. Le Senne, avec preface du traducteur. Paris, Félix Alcan, 1925. pp. xl, 315.

This is a translation of the second Greman edition (1924) of Heymans' *Die Psychologie der Frauen* which first appeared in 1910. The second edition (which we have not seen) is said to be "carefully and completely reviewed by the author, and brought into line with certain recent investigations." A comparison, however, of the first edition with this French translation shows little change; the only new title in the bibliography is O. Lipmann, *Psychische Geschlechtsunterschiede*, and the author's original and cautious conclusion remains the same. Although the book is based upon results obtained by the biographical method, its background is furnished by the earlier investigations of Heymans and Wiersma on heredity, and on the development of character in children of high-school age, in which the questionary method (*la méthode d'enquete*) was employed. For the convenience of the reader, both questionaries were published in an appendix to the first edition, and are now translated into the French.

Apart from the excellent translation, the particular feature of this French edition is La Senne's introduction of 40 pages, which is a study of Heymans' 'Special Psychology', particularly as regards its logical setting, its two principal procedures, and the specific nature of its subject-matter. The purpose of this summary is to furnish an account of, and to create an interest in, the work of *l'école de Groningue* taken as a whole. Le Senne's point of view is philosophical; he has caught the spirit of Heymans' work and, while enthusiastic, writes about it with intelligence and restraint. Although he has little to say of *La psychologie des femmes*, he has, nevertheless, furnished an admirable, if unusual, introduction to the work.

H. P. W.

Psychology: In Theory and Application. By H. W. Dresser. New York, Thomas Y. Crowell Company, 1924. pp. 727.

The subtitle of this voluminous work is more significant than are subtitles usually. The book is an attempt to set forth from a scientific point or view the theory of human nature, and to show how this theory is being applied to certain aspects of life. The book is not suitable as a textbook, but it should form a foundation for the reading of every professor, student, or layman interested in psychology. Its value to a library is inestimable. In fact, the book is encyclopedic in nature, and since this is the case, it is natural that the author should bring together the views of most of our leading psychologists and philosophers. Rarely, if ever, does he take a definite stand on any controversial question; instead, he presents impartially the views of leading men and leaves the reader to draw his own conclusions. In the preface he emphasizes "the need of a book which approaches the task in the spirit of science for the sake of coordination, but without the partisanships which too readily prejudice either the theory or its practical values."

The divisions of the book are five in number. Part I is devoted to a brief treatment of general psychology, and is based largely on James, Warren, and Woodworth. Part II, called the "Psychology of the Hidden Self," offers a psychoanalytical treatment. The most frequently quoted authors are Brill, Ewer, Freud, Jung, Prince and Rivers. The subject-matter of Part III is vocational and industrial, with frequent reference to Hollingworth and Scott. This section contains two excellent chapters,

one on vocational guidance and one on advertising and selling. One chapter is devoted to mental tests; but the author is content to tell of the army results, while the practical question how tests are, or may be, applied to business and industry is hardly more than mentioned. "Social Psychology" makes up the fourth Part, and the fifth Part is a sociological treatise headed "Social Organization." The names of Ellwood, Hobhouse, McDougall, Royce, Santayana, Trotter, and others are sprinkled throughout the text and appear in the selected reference-list which follows each chapter.

Pennsylvania College for Women J. S. KINDER

Reklame-Psychologie (2nd Edition). By Theodor König. München and Berlin, R. Oldenbourg. 1924. pp. vii, 224.

The Psychological Institute of the University of Würzburg under Marbe's direction has been carrying on practical work in advertising; and in this book Theodor König, an associate of the Institute, attempts a systematic treatise on the subject. The greater part of the book is devoted to a description and discussion of the experimental work in psychology which has a direct bearing upon advertising and of experiments carried on in advertising. In this field, comparison with books published in this country is inevitable; and, as in our texts, the topics of perception, attention and association receive the greatest attention. In the main the results are in accord with the studies carried on in America and reported by Starch, Hollingworth and Kitson. There is, however, in this text as little reference to research conducted in this country as there are references in the American texts to the work carried on in Germany. A new edition of König is promised that will bring it up to date as regards American research. On the whole, the work is satisfactory, especially in the importance which it attaches to methodology, a topic which tends to be ignored in the American literature of the subject.

The writer is somewhat surprised to find that König disregards the experimental work in visual perception and movement carried on by Jaensch and by the members of the "Gestalt" school. In particular, chapters upon the size and form of objects and the spatial conditions of the stimuli would offer a very fertile field for the application of these newer experimental results. In so far as the writer knows, such application has not yet been made in the field of advertising.

Cornell University SETH WAKEMAN

Medical Education: A Comparative Study. By ABRAHAM FLEXNER. New York, The Macmillan Co., 1925. pp. ix, 334.

In this study of medical education from the comparative point of view Dr. Flexner has concerned himself largely, although not exclusively, with internal medicine. The study is, in itself, valuable as a well directed enquiry into the aims, methods and values of medical training. It is of more interest to the reviewer as a justification of the value of the comparative method of study in educational problems.

Cornell University SETH WAKEMAN

NOTES

The Control of Experimental Procedure

In reviewing the experimental work of Ernst *et al.*[1] (performed under my direction), Professor Robinson[2] takes exception to that part of the procedure in which certain stimulating conditions were eliminated because they introduced an additional variable. He says: "In general there is increasing recall with decreasing rate of presentation. But this is not strange, since as soon as it was discovered that the second subject did less well at the 1.25 sec. and 1.50 sec. rates than at that of 1.0 sec., these slower rates were discontinued for that subject. The arguments for discarding the slowest rates are that they give results in disagreement with what has been found by others and that, at those slow rates, a new factor of wandering attention was introduced. Are we to believe that attention wandering enters the situation suddenly after a given rate of presentation has been passed? Inversions of the second order appear in the functions for both observers."[3]

It would seem that Professor Robinson has failed to grasp the purpose of this Study,—although he does recognize that it was primarily methodological, for he remarks: "The real interest of these investigators apparently was not so much in the memory process as in the possibility of treating the data of a memory experiment by means of the refined statistical procedures of psychophysics."[4] In the experimental study of any group of phenomena which can be quantitatively treated, two main phases of investigation may be found. In the first stage, the variables are defined and their relationships are worked out in general terms. This is the stage of extensive experimentation and does not require great refinement of method. It is followed by a second stage of intensive investigation in which refinement of method is of utmost importance, and which is predicated upon the results of the extensive studies. In probably no field of psychology has the first stage been completed. Yet there are few fields in which some general relationships have not been sufficiently well worked out to warrant their intensive investigation. So it is with memory. There are many relationships between the numerous variable conditions affecting the memory-process concerning which we do not have even a general knowledge. If a failure on our part to investigate one of these unknown relationships is to be interpreted as a lack of interest in the memory-process, we must plead guilty. On the other hand, the various methods that have been used in the investigation of memory show (as must all pioneer and extensive work) many crudities. The time seemed ripe to take certain variables whose general relations were known and to apply to them, within a limited range, refined methods of intensive experimentation. This is what Ernst and his co-workers did. Two known relationships, those of number of repetitions and recall and of speed of presentation and recall, were chosen and studied intensively by means of an adaptation of psychophysical methods.

From this point of view the dropping of certain speeds of presentation became, under our conditions, an absolute necessity. A careful investigation of the relation between speed of presentation and amount of immediate recall required that all other factors be kept constant. We founded this investigation upon the general result which had been previously worked out, that the amount of recall decreases as the speed of presentation increases. We did not attempt to investigate whether or not this general

[1] J. L. Ernst, F. E. Smith, L. R. Moessner, E. S. Rudisill, and M. J. Atwater, Further Data for an Associative Limen, this Journal, 35, 1924, 255ff.

[2] E. S. Robinson, Memory, *Psychol. Bull.*, 21, 1924, 572f.

[3] *Op. cit.*, 573. [4] *Op. cit.*, 572.

relationship existed, but to measure it by certain methods. In so doing we built upon previous experimental work in this field. Only as more specialized experiments are predicated upon the results of more general experiments can a science advance. We then found that at certain speeds the general relationship which we were measuring did not appear to hold for one *O*. A new variable was suspected, and we were fortunate enough to obtain introspective evidence that it was a factor of attention. Had this clue not appeared, there would have been no justification for discarding any of the stimuli. But as soon as it was evident that an additional variable was influencing the results, it was also evident that the figures obtained at slow speeds would be incomparable with those obtained at high speeds unless we could in addition measure this new variable. As we were unable in the time at our disposal to investigate in detail the effect of attentional wandering, we could only keep conditions constant by avoiding those situations that appeared to involve it.

This leads to Professor Robinson's question whether we are to believe that the factor of wandering attention enters the situation suddenly at a given rate of presentation. That, of course, we do not know. The relation of speed of presentation to wandering attention is a separate problem requiring experimental investigation. If one should hazard a guess, one would expect that as the speed of presentation decreased the shifts in attention would affect a greater and greater percentage of associative connections. Where the effect may have started we did not know, since we did not attempt to measure it. As a matter of practical procedure, all we could do was to eliminate those situations in which we had any evidence that the effect was present. To have failed to eliminate them would not have added to our results but would have made the data worthless.

The writer is unable to understand whether or not Professor Robinson means that the presence of inversions of the second order in the curves obtained indicates that the attentional wandering may not have been completely eliminated. If he does, it should be pointed out that mathematically all curves that are ogival in type must contain a single point of inflection, that is, an inversion of the second order. This includes the curves we obtained for recall as conditioned upon number of presentations (where the attentional factor was not found) as well as the curves obtained in psychophysical studies of sensory qualities. The presence, then, of such inflections cannot be given any significance save on the basis of a more detailed analysis of 'mnemometric' curves, an analysis which must wait until further experimental results are available.

Gilbert J. Rich

Intelligence in an Intelligence Test with an Auditory Distractor

Although it is usual to asume that a distractor will adversely affect performance in an intelligence test, the assumption is scarcely safe without experimental demonstration. Supposedly adverse conditions do not always have as great an effect as might be expected (*e.g.*, loss of sleep[1]), and intended distractors are notoriously perverse in their frequent disposition to spur the attention and thus to improve performance.[2]

In 1923 the writer of this Note sought to test experimentally the effect of an auditory distractor upon the success of 56 students at Clark University in the Otis intelligence test. Two electric bells at different sides of the examination room were rung intermittently as the distractor.

[1] [illegible] *Exper. Psychol.*, 1922, 5, 19-[illegible]; E. S. Robinson and F. Richardson, [illegible]

[2] *Cf.* M. A. Tinker, this Journal, 1922, 33, 578-583, and references to Münsterberg and Kezaki, Hamlin, Evans, and Cassel and Dallenbach there cited, 580.

The students were divided into two groups of 28 each. At the first session Group I was given Form A without distraction and Group II Form A with distraction. One week later Group I was given Form B with distraction and Group II Form B without distraction. Thus the tests with the distractor could be compared with those without the distractor with the elimination of constant differences between the two groups and of practice effects from the first to the second session.

There was an average gain in score with the distractor of 0.09%,—not a significant difference. On the average such a distractor neither aided nor hindered.

The general average, however, is here the least significant value. Apparently the better men were actually hindered by the distractor and the poorest men were aided. If the groups are divided into quartiles, the effect of the distractor is as follows:

Upper quartile	4.4% loss
Third quartile	0.5% gain
Second quartile	0.2% gain
Lower quartile	6.0% gain

Men of all quartiles lost in accuracy when the distractor sounded, but men of the upper quartile lost most (2.7%, as against 0.8% for the lower quartile). Hence in accuracy as well as in score the group gave a more uniform performance with the distractor.

From these general tendencies there are individual deviations. One man in the lower quartile gained 30% in score and another in the upper quartile lost 15% with the distractor. On the other hand, two men made the same score with and without the distractor. The coefficient of correlation of the scores with the distractor with those without it is $r = .86$ (P.E. = .02), and the corresponding correlation for accuracy is $r = .60$ (P.E. = .05).

The group is small, and the present finding can only be suggestive. It would be well to see if, with larger groups and with other kinds and degrees of distractors, a similar reduction of the dispersion of a group is found.

MILES A. TINKER

SANFORD'S *LABORATORY COURSE*

The publishers of Sanford's *Laboratory Course* tell me that they are willing to reprint a small edition of the book (to sell at $2.40), if I guarantee them a sale of one hundred copies. I hope that this offer may meet with a wide response; I shall be glad to receive orders for the reprint.

E. B. T.

EIGHTH INTERNATIONAL CONGRESS OF PSYCHOLOGY: FIRST ANNOUNCEMENT

The eighth international congress of psychology will be held at Groningen, Holland, Sept. 6-11, 1926. The congress will be limited to about two hundred well-known psychologists to be invited by the committee; psychologists who have not received an invitation, and who wish to attend the congress, are requested to apply to the First Secretary, Prof. Dr. F. Roels, Maliebaan 86, Utrecht, Holland. The membership subscription is 15 gulden; the recognised languages of the congress will be English, French and German.

Members of the committee of organization are Professor G. Heymans, president; Professor E. D. Wiersma, vice-president; Professors F. Roels and H. J. F. W. Brugmans, secretaries; and Professors L. Bouman, G. van Wayenburg and H. Zwaardemaker.

The photograph of Professor James Ward, of Cambridge University, which appears in this number, was taken by Messrs. Elliot & Fry, London.

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THE ACQUISITION OF ABSOLUTE PITCH*

By Helen K. Mull, Vassar College

History of the Problem

The problem of absolute pitch came to psychology from the musical world, where, as early as Mozart's time (b. 1756), the ability to identify notes upon hearing them was recognized among musicians as an unusual gift. It was Stumpf[1] who introduced absolute pitch anecdotally, theoretically, and to some degree experimentally, to psychology, which only within the last few years, however, has brought it into the laboratory in true scientific fashion. Here the work has been chiefly upon individuals who already possess absolute ear, and who, as it turns out, can furnish no introspective clue to the basis of their judgments, which come too easily and immediately for inspection. Neither are the objective results of different *O*s similar or clear-cut enough to point unequivocally toward any particular explanation. This state of affairs turns psychologists again theoryward in their search for an understanding of the nature of absolute judgments, and discussion centres around tonal attributes. There is naturally little agreement as to what attribute, or combination of attributes, is chiefly accountable for such judgments, inasmuch as there is no unanimity of opinion as to what actually are the attributes of tone. Even the meaning of [illegible] defined by either an objective or a subjective criterion. For if we say, as [illegible],

*From the Harvard Psychological Laboratory.

[1]C. Stumpf, *Tonpsychologie*, i, 1883, 280, 305ff; ii, 1890, [illegible].

that it is the ability "to identify pitches without having recourse to any process of comparing or relating "[2] (to the pitch of a known standard note), we are still being indefinite upon the matter of the *degree* of ability a person must have in order to qualify. Again, positive definition by a subjective criterion is impossible, since the experimental basis of judgment is unrecognized by those who have absolute ear, and is consequently the subject of most varied speculations on the part of theorists.

In presenting the historical setting of the problem of absolute pitch, it seems advisable to give first place to a discussion of the principal facts which have come out in the course of observation and experiment upon it, and afterward briefly to mention various systematic accounts of the phenomenon.

Degrees of Precision. That there are degrees of precision among the "gifted" individuals, is generally recognized, but it is stated nowhere more convincingly and explicitly than in Baird's[3] work upon 9 such persons. Here are exhibited all degrees from 100% to 0%, both by the group and by individuals, under the various experimental conditions given by different kinds of instruments, different pitch regions, and different notes to be judged. Baird claims that his *O*s possess absolute ear, however, because their averages on each of the instruments are in every case higher than would be due to chance, which he says is the extent to which the normal individual would judge correctly. Boggs[4] also found that the percentage of correct judgments made by her 6 "gifted" *O*s upon piano notes chosen at random from a certain arbitrarily selected number of octaves ranged from 100% to 16%. Both of these scores were made by the same individual.

What degree of precision shall be reached seems, then, to depend both upon the experimental conditions and upon the individual himself. We are now concerned to discover, further, upon just what external and subjective factors absolute ear may rest.

Pitch Region. Typically, the greatest precision occurs in the middle pitches—typically, but not invariably; for Boggs[5] found that pitch region makes very little difference to accuracy in the case of some of her *O*s. Nor does the middle region seem particularly favorable to Meyer[6] and Heyfelder, if we are to judge from the results of the experiments in which they took part. On the other hand, Baird[7] found quite generally that the middle range is easiest to judge. Von Kries[8] also, who has absolute ear, finds this to be true in his own case. He attributes greater precision in the middle region to its greater musical value, and to the fact that pitch discrimination is there finest. Stumpf[9] likewise recognizes this regional superiority, but attributes it rather to greater use and consequent familiarity, discounting fineness of relative pitch discrimination as an explanation, since, for his *O*s, regions of greatest accuracy for absolute pitch were not always coincident with those of greatest accuracy for relative pitch.

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[2] J. W. Baird, Memory for Absolute Pitch, *Titchener Commemorative Volume*, 1917, 44.

[3] *Ibid.*, 53.

[4] L. P. Boggs, Studies in Absolute Pitch, this Journal, 18, 1907, 194-205.

[5] *Ibid.*, 202.

[6] M. Meyer, Is Memory of Absolute Pitch Capable of Development by Training? *Psychol. Rev.*, 6, 1899, 514-516.

[7] Baird, *op. cit.*, 75.

[8] J. von Kries, Ueber das absolute Gehör, *Ztschr. f. Psychol.*, 3, 1892, 263, 268.

[9] Stumpf, *op. cit.*, I, 313.

Révész[10] notes the greater familiarity of the middle register, but speaks also of a possible fundamental superiority which it may have. Just what he means by "fundamental superiority", or what von Kries means by "musical value", is difficult to say; but the terms seem to emphasize the nature of the stimulus. All of the writers, however, recognize that individual, subjective differences are also of importance; there must be some "individual coefficient", as Stumpf[11] calls it, which enables an individual to respond adequately to the stimulus. Otherwise there would be nothing unusual about absolute ear. We shall see in the systematic accounts what each writer considers the "individual coefficient" to be.

Naturals vs. Accidentals. It is not only pitch region that influences accuracy. According to Baird[12] certain notes, namely "f", "e", and "g", are generally more favorable for absolute judgments, though these notes are not easiest for every one. Generally, also, naturals are better judged than accidentals, though there seems to be no reason for believing in the tradition that each, as a class, has a distinctive character. Boggs,[13] it is true, found one of her *O*s able almost invariably to distinguish naturals from accidentals, but she attributes it to the fact that in this case the naturals had names of their own, whereas the accidentals did not. In the case of the other *O*s, the accidentals had distinctive names as well as the naturals. In her opinion, one essential requirement for absolute pitch is that there shall be invariable association between a note and its name.

As early as Helmholtz[14] we find a discussion of the allied subject of key-character, which Helmholtz himself believes to exist, but not to be a matter of absolute pitch in so far as it depends upon incidental physical characteristics of the stimulus. Incidental characteristics, however, will not explain all cases. The most general explanation Helmholtz offers, and the only one which he considers really to be a matter of absolute pitch, is that the cue may come from g'''', which is the "proper tone of the human ear", and so has a special brilliance. The extent to which this tone is present as an overtone in other notes may be a determining factor, though he does not believe it to be a very important one.[15] This latter explanation might be applied to absolute pitch for single notes, which Helmholtz himself does not discuss, as well as to key-character.

Timbre. We noted, in treating of degrees of precision, that some timbres are more accurately judged than others. It may be that overtones are in some way important *per se* or, on the other hand, it is possible that it is the familiarity of the clang which should receive the emphasis. Or it may be that both factors contribute.

Stumpf[16] admits the importance of timbre and, consistently with his views concerning pitch region, sets some store here also by familiarity. The most familiar timbres are the easiest to judge. But there are other factors as well. "Alles hängt hier von der Übung, vom Gedächtnis, eben damit aber auch von einem individualen Koeffizienten, ab." Von Kries[17] is not entirely in accord with Stumpf; for, though he recognizes the importance of timbre and of the individual coefficient, he minimizes the rôle of frequency. If richness of overtones, which according to von Kries need not be consciously present to the *O*s, is in some way important for accuracy in absolute pitch judgments, then chords ought to be easier to judge than single tones. For von Kries himself they are, especially harmonious chords.

[10]G. Révész, *Zur Grundlegung der Tonpsychologie*, 1913, 101.
[11]Stumpf, *op. cit.*, I, 315.
[12]Baird, *op. cit.*, 61.
[13]Boggs, *op. cit.*, 202.
[14]H. L. F. von Helmholtz, *Tonempfindungen*, 1870, 485-488.
[15]Helmholtz, *op. cit.*, 487.
[16]Stumpf, *op. cit.*, I, 305.
[17]von Kries, *op. cit.*, 261, 264.

Nevertheless, they are by no means as frequently heard as single tones[18]. Boggs[19] also finds her *Os* keen in hearing out overtones, and more adequate to chords than to single tones. But, as von Kries points out, overtones cannot be all-important, since pitches produced by the human voice, rich in overtones, are for him harder to identify thar those produced by tuning-forks which are practically lacking in overtones.

Another argument against the sufficiency of an explanation on the basis of familiarity is the fact that there seems to be a determinable order of difficulty among the various instruments. Von Kries[20] finds general agreement among musicians that the order, from least to most difficult, is as follows: piano, strings, wind-instruments, voice, whistling, tuning-forks, and bells. Baird's[21] order is strikingly like that of von Kries: piano, organ (diapason, reed, string, flute), voice, and tuning-forks. Though it is possible that this order of difficulty might be, in general, the order of familiarity as well, neither Baird nor von Kries thinks that familiarity has much to do with the situation, and both cite in support of this view the fact that the voice, which is most familiar, is among the most difficult to identify.[22] The argument seems fallacious, however, because it is not a single human voice which is repeatedly heard, but many different human voices, among which there is much less uniformity than among mechanical musical instruments. Moreover, as Köhler[23] points out, any particular voice is unstable because of vowel differences which considerably affect its tone-body. He finds that persons with absolute ear are inadequate to unfamiliar clangs. And, unlike von Kries' and Boggs' *Os*, his *Os* were not more adequate to chords than to single notes.[24]

Timbre, then, is probably important both *per se* and also by reason of its familiarity to the individual; at least we do not seem justified in leaving out either factor in an explanation of absolute pitch in general. In any individual case a different conclusion might be reached.

Errors. Errors in absolute judgments, we have seen, are made in all degrees, depending upon the individual and upon the conditions under which he is rendering his judgments. But there are certain alleged peculiarities in the nature of errors made by "gifted" individuals which are sometimes considered important. Foremost of these is the fact that a note is often given its proper name, but is assigned to the wrong octave. Baird[25] bases his explanation of absolute ear largely upon these "octave-errors." He discusses the possibility of their occurrence by reason of the extreme community of overtones in notes an octave apart, but rejects it finally, in the face of the fact that the next most frequent errors are thirds and sixths, which do not have as high a degree of community of overtones as fourths and fifths, which come after thirds and sixths in order of frequency. He considers this fact to be evidence which justifies the assumption that there is a certain something, "quality" he calls it, after Révész, which is alike for all "c's", alike for all "d's", but different as between "c" and "d". It is an attribute, "obscurely present" in all tones, but clearly experienced only by those who have the gift of absolute pitch. Such an attribute would explain very neatly the likelihood of one "c" being mistaken for another.

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[18]von Kries, *op. cit.*, 271.
[19]Boggs, *op. cit.*, 202, 204.
[20]von Kries, *op. cit.*, 264, 272.
[21]Baird, *op. cit.*, 50, 74.
[22]von Kries, *op. cit.*, 275, 276; Baird, *op. cit.*, 52.
[23]W. Köhler, Akustische Untersuchungen, *Ztschr. f. Psychol.*, 72, 1915, 168.
[24]Köhler, *op. cit.*, 168.
[25]Baird, *op. cit.*, 65, 66, 76.

There are, however, other explanations of octave-errors. Stumpf[26] considers them to be due simply to misapprehension or ignorance as to what pitch-regions the different octave designations signify. Accordant with this explanation is Köhler's[27] belief that such errors do not occur on familiar instruments. To Watt[28] they are an indication of the extent to which absolute ear is subordinated to relative ear. The placement of the note within the octave, that is, its interval, is of primary importance, and the placement of the octave a secondary matter. Ogden[29] believes that the reason errors of fourths and fifths are of comparatively infrequent occurrence, as against thirds and sixths, is that they are more prominent divisions of the octave, and so get more attention and individuality. As may be surmised, neither Watt nor Ogden explains absolute pitch without reference to the musical interval, upon which they both base their systems of hearing.

Since it is Baird who places most importance upon the nature of the errors, let us examine his results somewhat more closely. Usually, he finds, errors do not exceed three semitones, and those of one semitone are most frequent. He gives no data to show the relative frequency of octave-errors to the other errors. Errors of thirds and sixths are frequent, but one can not be sure in the case of sixths that the *O* did not mean to place the note, say, in the octave above, and judged it a minor third too low. Again, he gives no data to show the relative frequency of errors of tritones and sevenths. It is also possible that a different statistical treatment of errors might result in a different order of frequency (*cf.* pp. 484f).

Kinaesthesis and Imagery. If one were not disposed to call in a special attribute of tone, which only those persons gifted with absolute ear could clearly perceive, one might think first of kinaesthesis as a possible explanation of the phenomenon. Absolute judgments might be based simply upon the remembrance of the vocimotor feeling of the various notes, or of the feeling of one note, to which all other notes are related by interval. It is hard to disprove the sufficiency of such an explanation, especially when one remembers the "implicit" responses urged by the behaviorists. But if kinaesthesis is important, its influence is, to say the least, often very obscure and unconvincing. With regard to the relating of all notes to one note, there is ample testimony that there are many persons, who possess absolute ear, who do not have good sense of interval.[30] And with regard to the possibility of each note having a different feeling, it is not clear why kinaesthesis should take precedence over auditory sensations. But let us examine more closly the evidence for and against the importance of kinaesthesis in absolute judgments.

There are many cases where recognition of a given pitch is possible without reproduction of the same pitch when it is designated by name.[31] This fact certainly obscures the rôle of kinaesthesis, though it proves nothing against it; for recognition is always an earlier stage in association than recall. Again, von Kries[32] affirms that he can judge female voices, whose pitches he cannot reproduce, better than male ones, and that he can never estimate his own vocal or whistling tones correctly. A third argument against kinaesthesis is the fact that absolute ear often dates from childhood, and children's voices change as they grow older, although it is possible that readjustments might occur. A more conclusive argument lies in the fact that some individuals can judge notes correctly without

[26] Stumpf, *op. cit.*, I, 311.
[27] Köhler, *op. cit.*, 172.
[28] H. J. Watt, *The Psychology of Sound*, 1917, 85.
[29] R. M. Ogden, *Hearing*, 1924, 163, 164.
[30] Köhler, *op. cit.*, 160; Stumpf, *op. cit.*, I, 269; von Kries, *op. cit.*, 276.
[31] Köhler, *op. cit.*, 174.
[32] von Kries, *op. cit.*, 271, footnote 2.

being able to carry a tune, to reproduce a given pitch, or even to misrepresent it consistently. Nevertheless, this argument is not entirely conclusive, for there may still be some consistent kinaesthesis adequate to the single note. Granting this, we have still to account for the undeniable fact that some timbres are more easily judged than others.

When we turn to the introspective reports, we have Stumpf's[33] evidence that the transposition of a tone to the middle register sometimes occurs to advantage. Boggs[34] found that there may be various accompaniments, such as humming and visual or motor imagery. But she also finds that there is a positive correlation between immediateness and accuracy of judgment and that, typically, accurate judgments are immediate both in time and in experience. Baird[35] found that, though humming is sometimes successfully employed, nevertheless, in test series in which his *O*s were given all the time they wanted to make their judgments, a smaller degree of accuracy was shown than in those experiments in which only sufficient time for immediate judgments was allowed. Like Boggs, he found that, even without the necessity imposed by time, judgments were immediate.

This is enough evidence to show that, although kinaesthetic sensations and imagery of various kinds have been known to occur in judgments of absolute pitch, accompanied sometimes by success and sometimes by failure, they are usually lacking. What, now, can be the basis of the typical judgment? Has it anything to do with musical ability?

Musical Ability. Discussion on the preceding topics has indicated that good sense of interval, fine pitch discrimination, and ability to carry a tune are not necessary accompaniments of absolute ear. Stumpf[36], however, considers interval-sense to be the only dispensable musical ability. Ogden[37] and Boggs[38] also think it likely that all persons "gifted" with absolute ear are also gifted with musical ability. On the other hand, Köhler, von Kries, and Watt emphasize rather its unimportance, and von Hornbostel states that it is not an invariable accompaniment. We can hardly estimate the musical talent of parrots and starlings, but it is not likely to be as highly developed as it is in human beings. They might be said, however, to possess absolute pitch, since they repeat melodies only in the keys in which they were learned.[39]

The foregoing facts which have been ascertained with respect to absolute pitch leave us chiefly with a realization of their contrariness. Conclusions drawn from them can be only of the most general kind. It does seem, however, that (1) something depends on the stimulus, and that (2) something depends on the individual.

To account more explicitly for the phenomenon displayed, two general explanations have already been indicated: (1) the individual may possess a special ability, differing in kind from anything possessed by the average person; or (2) he may possess merely a high degree of some ability which is found to some extent in all persons.

The most unconditioned exponents of the first explanation are Boggs, Révész, and Baird. They believe that quality is the distinguishing feature of a tone in absolute pitch judgments. Boggs[40] was at first inclined to think that ability to hear out overtones was the cause of the absolute ear, but later changed her view from timbre to quality. Quality, she holds, enables

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[33]Stumpf, *op. cit.*, I, 308ff.
[34]Boggs, *op. cit.*, 204.
[35]Baird, *op. cit.*, 69, 72.
[36]Stumpf, *op. cit.*, I, 289, 306ff.
[37]Ogden, *op. cit.*, 161.
[38]Boggs, *op. cit.*, 204.
[39]E. von Hornbostel, Ueber vergleichende akustische und musikpsychologische Untersuchungen, *Ztschr. f. angew. Psychol.*, 3, 1910, 465-487.
[40]Boggs, *op. cit.*, 202-204.

a person to hear tones in much the same way as those with color vision see colors.

Baird's[41] view is similar. He bases it chiefly on octave-errors, as we have seen, and on immediateness of judgment, which he thinks did not become immediate by practice, but was originally immediate, like the recognition of red when once it has been pointed out as red.

Révész[42] makes a definite division between those who recognize tones by pitch, and those who recognize them by quality. Both have genuine absolute ear, but it is the latter group whose judgments are most immediate and most correct. Ability to distinguish quality is innate, like color-vision, and cannot be acquired. Pitch-memory, on the other hand, may be acquired and perfected by diligent practice. Those who have quality usually have pitch also, and the two work in conjunction, so that the octave-errors are comparatively infrequent and can soon be eliminated by regional pitch-memory. Qualitative memory may be present for certain regions only. To explain the influence of timbre, he says it is possible that, when other parts of a tonal complex are changed, the quality cannot be recognized in the new combination.

Révész' position is based largely upon the results of his investigations on an abnormal *O*, von Liebermann, for whom "pitches" were abnormal, so that he could not judge intervals correctly, while "qualities" of the component notes remained unchanged. Von Liebermann's case allows, however, of other interpretations. Ogden[43] attributes his confusion of the fifth with the octave, for instance, where the judgment of their "qualities" remained intact, to "a reduction of von Liebermann's musical ability to that of an unmusical person." The defect is an integrative one, of central origin presumably, since the disturbances were binaural.

Stumpf and von Kries hold similar positions, though that of Stumpf seems less extreme. Both speak of *Anlage*, which they do not further define. For von Kries[44], however, it is something which cannot be acquired, while Stumpf leaves the question more open, and gives more weight to familiarity, musical ability, feeling, and interest, as displayed in practical musical activities.

It is almost impossible to disprove theories of special ability, but it is certainly undesirable to multiply entities unnecessarily. Rich[45], in 1919, made an experimental study of tonal attributes by a method of independent variability. He finds some evidence for an attribute of "tonality," defined as "that attribute by which (musical) tones are named." His *Os*, it is true, had difficulty in knowing what to observe when asked to observe tonality; but that proves nothing against the tonality (quality) which we have been discussing, since the *Os*, who were not gifted with absolute pitch, would not be expected to observe it easily. But Rich found that the DL for what his *Os* took to be tonality increases steadily without following Weber's Law. Just why the middle pitch-region should be easiest to identify in absolute pitch is not explainable by this fact, if the *Os* in his experiments were judging quality, and if judgments of absolute pitch rest upon the same attribute.

The most convincing proof of the spuriousness of tonality, in Baird's sense of the term, would be success in training average individuals in absolute pitch to the point where they were similar to the "gifted" individuals in every respect.

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[41] Baird, *op. cit.*, 77, 78.

[42] Révész, *op. cit.*, 90-101.

[43] Ogden, *op. cit.*, 161.

[44] von Kries, *op. cit.*, 261, 262.

[45] G. J. Rich, A Study of Tonal Attributes, this Journal, 30, 1919, 121-164.

Explanations of absolute pitch which do not rest upon a special ability are held by Meyer, Köhler, Watt, and Ogden.

Meyer[46] holds that every one has absolute ear to some extent,—that there is no sharp line of distinction, which, if it existed, would indicate a physiological difference between normal and "gifted" persons. He considers some features of the tone-body, rather than pitch, to be the basis of absolute judgments.

The theory that tone-body, and not pitch, is important, has been developed by Köhler.[47] Tone-body includes brightness, vocality, volume, and intensity. It is recognized immediately, that is, without the intervention of a memory image. Experiments on himself, in which he tried to learn the white notes between C and h‴, showed a great improvement in correct pitch-naming after he changed from a pitch *Aufgabe* to one of tone-body. In judging upon this latter basis he found himself relying upon gross brightness for regional placement and, further, upon a characteristic which was different for each note. Some notes were "geschlossen", others "zwiespaltig", and so on. These features of tone-body were not present in pure tones, and pure tones are notably difficult to place. (On an attributive explanation it would be difficult to explain why pure tones should offer any special difficulty, since they have the same attributes as those presented by complex ones.) For unfamiliar timbres, recognition is quite impossible, Köhler holds. He explains his errors of fourths and fifths by claiming that, when attention is upon tone-body, it is away from pitch, and if two pitches, not too different, have similar tone-bodies, one may quite easily be taken for the other.

Watt[48] disagrees with Köhler's list of attributes and considers that he has merely shown that timbre is important. Watt believes the attributes of tone to be two, pitch and volume; pitch is ordinal, volume extensive. Both are taken into account by absolute ear, where there may be either a special refinement of sensory apparatus or a better auditory memory. But he thinks that in many cases of absolute pitch there is required also an absolute point of reference in auditory orders (pitches). The problem is analogous to that of localization on the skin, where, however, the great influence of relativity does not exist. It is due to this lack of relativity that tactual localization is possible for all persons. Auditory localization is possible only to a few who have not allowed the relative ear to dominate the absolute ear. According to this view, we all have had a chance to possess absolute pitch, but because of our education in musical relationships we have lost it through disuse. We do recognize noises and the sounds of our friends' voices, where the influence of musical relationships does not impose itself.

Ogden, who, as we noted, bases absolute ear upon a refined sensitivity to different clang-patterns, also insists upon the necessity of reference to a scale with which the individual who possesses the talent is familiar. "The tone must find its place within the setting of an octave and thus establish its interval before its pitch can be named."[49] This explanation is essentially like Watt's. With Ogden, however, the point of reference seems to be introduced for purely systematic reasons and to be considerably overemphasized, especially when one remembers that good interval-sense is not a requisite for absolute pitch and that relationships are generally considered to be a hindrance rather than a help. A note must have a name, and the name is likely to imply musical relationships; but unless the relationships are properly comprehended and help to make the perception richer or more definite, they seem gratuitous.

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[46]Meyer, *op. cit.*, 514; also Köhler, *op. cit.*, 173, quoting Meyer.
[47]Köhler, *op. cit.*, 161ff.
[48]Watt, *op. cit.*, 85, 263ff.
[49]Ogden, *op. cit.*, 163.

Problem and Observers

Problem. From the foregoing discussion, it seems that there is little reason for calling in a special attribute of tone to explain absolute pitch. There would be even less reason if average individuals could be trained in absolute pitch until they were just like those "gifted" with it. The following experiments were undertaken to test this possibility, and to discover, if possible, the basis of absolute judgment. The results point to still another explanation than any so far given, —to one which stresses the importance of attention. Stumpf hints at it when he speaks of interest and practical musical ability. Boggs also implies it when she remarks that there is great need for concentration in judging pitches absolutely, as is shown by requests for repetition of the notes, and by the distraction caused by outside disturbances. Ogden thinks the most favorable clangs are "striking and uniform." Köhler too notes that one direction of attention is more effective than another, but his emphasis is chiefly upon that which is attended to. What justification we find for emphasis upon attention itself will be discussed in connection with the results.

Observers. There were 12 *Os* in these experiments. Miss E. W. Amen (A), Mr. A. M. Brues (B), Mr. J. G. Beebe-Center (B-C), Mr. L. R. Frazier (F), Mr. W. S. Hulin (H), Mr. J. M. McGinnis (McG), Mr. K. E. Zener (Z), and the writer (Mu), were graduate students in psychology at Harvard University. Dr. A. A. Roback (R) and Dr. M. B. Pratt (P) had the doctorate from Harvard and Clark Universities respectively. Mrs. E. C. Gilbert (G) and Miss K. Miller (Mi) were college seniors 'majoring' in psychology. These particular individuals were selected in order to make up a group representing various degrees of musical ability and training.

Preliminary tests soon made it apparent that none of the *Os* was "naturally" gifted with absolute ear. Three, B, R, and McG, had never had any muscial training outside of that afforded by secondary schools. Moreover, these three *Os* had the greatest difficulty in reproducing vocally a given pitch that was well within their vocal compass, though one of them (R) had no difficulty whatever in carrying a tune. A and G had had only a few piano lessons, but did not "keep up" their music. The others had had considerable training, and still engaged in some sort of musical activity. The Seashore tests of musical ability show Mi, B, Mu, F, P, and Z, as ranking in the first quartile in the order given; H, G, and R, in the second; and A, C, and McG, in the third.

General Procedure. The experiments and the *Os* fall into three groups, according to the phase of the problem with which they are particularly concerned.

The first group dealt with the acquisition and perfection of absolute ear for one note. The *Os* were trained with respect to one note only, in the belief that a greater though limited training would yield more rapid and more precise results than the casual acquaintance with many notes. The assumption was that, if one note could be learned, all could be learned, in time. In this group the problem of the transfer of the acquired ability to some instruments other than the one learned also arose.

In the second group, the ability to judge notes correctly, before special training, was investigated more thoroughly than in the case of the first group. The rate of forgetting after special training, and the correlation between speed and accuracy of judgment, were also determined.

The third group of experiments dealt with the acquisition of absolute ear for all the notes of the chromatic scale of an octave in the middle pitch-region.

Results: Group 1

Procedure. Six persons, A, B-C, F, H, Mi, and R, took part in the first group of experiments. Each observed one hour a week throughout most of the college year, excepting that A and R came once a week through the first semester, and twice a week during the second semester.

Preliminary tests, consisting in the presentation of various single notes of the diatonic scale, selected from three octaves in the middle pitch-region, were made to make sure that the *Os* were not already possessed of absolute ear. After it was sufficiently demonstrated that none of them was so "gifted", intensive training was begun.

The Appunn tonometer was used throughout all of the experiments, excepting that in the first few, when the tonometer was not available, the Ellis harmonical was used.

With *Os* B-C, H, and Mi, middle c (c′; 264 d. v.), the note selected to be learned by this group, was sounded once every minute for 15 min. Each presentation lasted 5 sec. To the other three *Os*, A, F, and R, it was sounded continuously for 15 min. In these trials attention was called to the note once every minute for five sec., and between attentive periods the *Os* were instructed to distract themselves by reading or studying. These two modes of training were adopted in order to discover whether a stimulus unattended to would be of advantage in the acquisition of absolute pitch. During the 5 sec. attentive periods, all the *Os* were allowed to sing the note, or to "do" anything else they pleased with it; and they were encouraged to observe everything they could about the note. Introspections were required at the end of the 15 min. Then, after a short but effectual period of auditory distraction, which pre-

vented the continuance of after-images, the test experiments were begun.

In the test experiments the *O*, seated with his back to the instrument, was presented with a series of notes chosen from among the naturals from c to b′ inclusive. The possibility of melodic clues was avoided by making the series as musically nonsensical as possible; by using, in any series, only some of the notes; and by the occasional introduction of accidentals in extreme regions. The *O* was to respond by saying "yes" when he judged the note presented to be c′, and "no" when he judged it to be a "not c" note. Any note was repeated upon request, and requests turned out to be frequent, for outside disturbance was very distracting. The series ended automatically with a "yes" judgment. Then the *O* was told whether he had judged correctly or not and, if not, what the note was, in order that he might know the direction and magnitude of his error. Then c′ was presented again for 5 sec. to offset the tendency to form habitual errors. There were usually 20 of these test series given at one sitting, with auditory distraction between them. Free introspections as to the basis of judgment were called for, often after single judgments, and sometimes at the end of an hour.

After a month, other experiments were begun, in which the notes used were much closer together than those of our musical scale. Nine notes, including the c′ already being learned (264 d. v.), separated by equal increments of 8 vibrations, were selected between 232 and 296 d. v. inclusive, and were presented in test experiments of the same nature as the previous ones, except that this time the series did not end with the giving of a "yes" judgment: all 9 notes were presented, in haphazard order, in every series. The *O*s were told, however, that c′ might be presented more than once in a series, or that it might not be presented at all. This method has the advantage of presenting all the notes an equal number of times, and thus facilitating computation of the results.

Experiments of these two sorts were made alternately every two weeks. Toward the end of the year, tests of the first kind were made with three different organ stops (diapason, string, and flute) to study transfer of learning.

Amount of Learning. If, for purposes of comparison, we select from the pre-training records of all the *O*s only those cases where c′ was presented and judged, we find that judgments were correct only 40.4% of the time, and that in 59.6% of the cases c′ was mistaken for a note not c′. The average error com- [illegible] cents = 1 semitone). The untrained *O* was correct with respect to c′ less than half the time, and made on the average an error between a musical second and a musical third. The final records

show that judgments were correct 82% of the time, that is, that in only 18% of the cases was c′ mistaken for a note not c′; the average error was reduced to 33 cents. These averages are representative of the individual records (see Table 1).

There was, then, decided improvement; but can we say that, at the end of their training, the *O*s had acquired absolute ear for middle c? Baird's[50] figures show an average error of 24.5% (as against our 18%) with the most favorable stimulus, piano notes; and of 37.9% with organ notes. The magnitude of individual errors of 4 of his 9 *O*s was rarely greater than 3 semitones. The same thing may be said of 5 of our 6 *O*s, as can be seen from Table 1. It thus appears that our *O*s acquired absolute ear for middle c to the degree that Baird's *O*s had it.

Importance of Marginal Stimulus. It will be recalled that to three of the *O*s of this group, B-C, H, and Mi, c′ was presented intermittently and only under attention; and that to the other three, A, F, and R, it was presented continuously, but part of the time under distraction. The duration of attention was alike for both groups. A scrutiny of the individual records reveals no striking superiority of the second over the first group. The records of the first are, if anything, superior; individual differences, however, seem to be the potent factor rather than the difference in methods of training.

Rate of Learning. By far the greatest amount of learning occurs in the very beginning, during the period of intensive training preceding the first test experiments. The latter show an average of 70% correct judgments, with an error of 84 cents, as against the 40% and 283 cents of the pre-training experiments. Table 2 shows that there is on the average a steady improvement, although as can be seen from Table 1 there are irregular fluctuations in the individual records.

Transfer. Table 2 shows that there was a high degree of transfer of learning to notes of the diapason, string, and flute organ stops. As contrasted with the 40.4% correct judgments of the pre-training records, diapason notes show an average of 76.3%; string notes, of 77.1%; and flute notes, of 77.7%. From Baird's experiments we have these corresponding figures; diapason, 59.2%; string, 52.4%; flute, 49.7%. The order appears to be opposite in the two cases; but when we remember that the flute errors amounted to 42 cents in the average, and the diapason and string errors to 25 and 24 cents, respectively, a modification of the order indicated by the percentages above seems advisable, especially since it is not representative of the individual cases. Two of the *O*s made the highest percentages of correct judgments upon diapason notes, two upon string, and two upon flute notes. A slightly more representative order is

[50]Baird, *op. cit.*, 57, 59, 61.

TABLE 2

	Av. percent. of correct "c" or "not c" judgments	Av. error (cents)
Before training	.404	283
Test 1	.700	84
" 2	.743	61
" 3	.694	58
" 4	.714	48
Final tests	.820	33
String	.771	24
Diapason	.763	25
Flute	.777	42

given by the average errors, where we find that the flute errors are greatest in three cases, string errors in one case, and diapason errors in one case. It does not seem reasonable to attach much importance to the difference of our percentage order from Baird's, which also, like ours, is not completely representative of the individual cases.

There is the possibility that by the time these new stops were used, the *Os*' general method of judging had become perfected; so that, after the new c′ was once heard, they knew how to recognize it subsequently. This factor may indeed account partially for the results; for, as we have already noted, learning is fairly rapid; but transfer is the only explanation of the fact that, of 24 initial presentations of new c′s, 16 were judged correcly, or in other words that 16 c′s out of 24 were judged correctly the very first time they were heard.

Magnitude and Direction of Errors. Baird states that the errors made in judgments of absolute pitch seldom exceeded three semitones except in the case of 4 of his 9 *Os*. Table 1 shows that this is true of our *Os* also, with the exceptions B-C and R.

The direction of our errors is chiefly that of overestimation of c′ both for the tonometer notes and for the organ stops; but with respect to the notes presented, the terms in which Baird speaks, the errors are underestimations. The average error calculated from the notes below c′ that were judged as c′ is 20 cents for tonometer notes, and 11 cents for organ notes; whereas

[51]The average errors in cents are calculated on a basis of the number of judgments made in any test, instead of upon the number of times the notes were presented, as is done with the percentages in Table 1. It is difficult to decide which figures to use when comparing our results with Baird's. [illegible] the two methods would have yielded identical results, since every note that was presented was judged [illegible] "no c" judgments of c′ introduce errors of omission taken into account by the percentages but not by the errors in cents. The latter, however, [illegible] single estimate of [illegible]

the error calculated from the notes above c′ mistaken for c′ is 61 cents for tonometer notes and 16 cents for organ notes. Baird[52] also finds that with organ notes underestimation is relatively more frequent.

Results of Experiments with Notes of 8 d. v. Differences. The results of these experiments in all essential respects confirm those already discussed, as Tables 3 and 4 will show, except for the fact that in this case we have no record of the *Os'* ability previous to training; when these experiments were begun, c′ was already well established.

From an average of 43% of c′ presentations correctly judged, with an average error of 68 cents in test 1, the *Os* attained to 57% correct judgments in the final performance, with an average error of 29 cents. There is average improvement from test to test in respect of the diminution in size and number of errors. In respect of the percentage of presentations correctly judged, there is one inversion.

The individual records naturally show greater irregularity than the average. It is interesting, however, to note that here, where the task is harder and the percentage of correct judgments is less than in the experiments with notes of the diatonic scale, the individual errors are of less magnitude, that is, they are grouped more closely around c′.

From the total number of errors made upon each of the various vibration rates, an approximately normal curve could be drawn, though it would be somewhat skewed in the direction of pitches above c′, indicating that, as in the case of the diatonic notes, the pitch of c′ is usually overestimated. The average error of overestimation is 31 cents, whereas that of underestimation is 15 cents.

TABLE 4

	Average percent. of correct judgments of 8 d. v. differences	Average error
Test 1	.43	68
" 2	.47	53
" 3	.64	39
" 4	.57	29

Results: Group 2

Procedure. The results of the first group of experiments—particularly the immediateness of learning, the irregularity in individual records, and the unimportance of the marginal stimulus—suggest that an important factor in the acquisition of absolute ear may be simply the giving of attention to the task

[52]Baird, *op. cit.*, 61.

in hand, that is to say, interesting oneself in the note for its own sake rather than for its melodic or harmonic significance. Accordingly, in a second group of experiments, we made attention the variable factor, by putting 4 new *O*s, B, G, McG, and Z, through two series previous to any training, of which the first was just like the pre-training experiments of Group 1, where the various notes presented were to be judged by their own names; and the second consisted in running through the same experiments once more with the *O*s asked to judge merely whether the note presented was "b" or "not b" (the b below middle c, 248 d. v.). We had it in mind to discover whether the heightened attention which a narrowed *Aufgabe* insures would influence the results.

After these two sets of pre-training experiments were completed, the *O*s were trained intensively to b by being asked to listen attentively to it for 5 sec. once every minute for 10 min. Thereafter tests were given at the end of 3 min., 1 hour, 1 day, 1 week, and 1 month, to determine the persistence of learning. These test experiments consisted of the presentation of the diatonic notes from f to e′ inclusive (176-330 d. v.), and in addition two accidentals in a non-critical region (a′♭ and b′♭) to offset the possibility of melodic cues. The series, in each of which all the notes were presented (as in the experiments of Group 1 with a difference of 8 d. v.), were constructed so as to insure their being as melodically nonsensical as possible. Reaction times were taken to discover whether any relation existed between speed and accuracy of judgment.

Pre-training Experiments: (a) Degree of Precision (Set 1). The purposes of this group of experiments were to discover the influence of heightened attention upon absolute pitch, the amount of forgetting, and the relationship between speed and accuracy of judgment. The pre-training experiments, moreover, proved interesting for comparison with Baird's results in certain other respects.

For the group as a whole, the order of correctness for the various notes, as shown by their percentages of presentations correctly judged, is as follows:

g	c′	a	f′	b[53]	d′	e′
.43	.38	.38	.21	.19	.18	.10

Chance would give .11, since the 7 diatonic notes *plus* 2 accidentals were used. We must realize, however, that if all the

[53]This figure is higher than that shown in Table 6 because in this case neither the octave-errors nor the "b" judgments of b′♭ were counted as errors (*cf.* Baird, *op. cit.*, 49).

judgments made had been, for instance, "c" judgments, c′ would have been judged correctly 100% of the number of times it was presented, and obviously this figure would misrepresent the actual state of affairs. Our *O*s did not use such a method, that is, they did not respond invariably with the judgment "c", or "d", or any other one note, but as a matter of fact a comparatively large proportion of their judgments were "c" judgments. The order of frequency of judgment in terms of the various notes, regardless of octave, is:

c	a	f,g	e	b	d	
83	59	57	43	34	31	judgments

The positive correlation between the two orders is .81. If we now look for an order of correctness by getting the percentage of correct judgments for the various notes, on the basis of the number of judgments made in terms of each note, we obtain the following:

b	c	f	d	g,a	e
.47	.36	.30	.25	.19	.09

(The figure of b is high chiefly because of one individual's score.) The notes c and f, then, are both frequently judged and frequently judged correctly, though on the whole the positive correlation between frequency and correctness is insignificant (.009).

No matter which order is considered, it is apparent that, even before training, the *O*s give correct judgments in a greater percentage of cases than would be due to chance. It is also noteworthy that judgments in terms of accidentals were almost never given, even though the *O*s were told that some accidentals would be presented. The *O*s thought in terms of naturals, and in terms of some naturals more than others.

For Baird's *O*s the case is similar: c, f, and g are the notes best judged. He claims, however, that "c-ness", "f-ness", and "g-ness" are peculiarly easy to detect, whereas it is possible that these notes had merely received more attention and emphasis in the course of the *O*'s previous casual experience.

(*b*) *Errors*. Another very interesting phenomenon as shown by this first set of pre-training experiments is the occurrence of octave-errors. Baird considers these errors peculiar to the absolute ear; but, as we have seen (p. 473), they are sometimes thought to be due simply to unfamiliarity with musical instruments. Altogether, there were 14 octave-errors in the present experiments, and 13 cases of errors of more than one octave.

Table 3

Percent. of number of times presented that the various vibration rates were judged as c′ at different stages of learning

D. v.		232	240	248	256	264	272	280	288	296	Av. error	in cents
Cents		58.70	56.76	54.45	52.43	50.94	50.19	48.77	47.44		below c′	above c′
Observer A												
Test	1	.10	.30	.20	.20	.40	.50	.40			49	32
"	2			.10		.60	.70	.50	.20	.10	5	58
"	3			.05	.30	.55	.50	.25	.15		12	41
"	4				.15	.35	.50	.15	.15		6	47
Observer B-C												
Test	1		.40	.10	.30	.20	.50	.40	.20	.20	40	59
"	2			.20	.40	.00	.50	.10			36	30
"	3			.05	.05	.65	.10	.20			8	24
"	4			.15	.10	.40	.35	.15			19	29
Observer F												
Test	1	.10		.30	.30	.30	.30				54	12
"	2		.10	.10	.10	.40	.20	.30			27	34
"	3			.10	.15	.55	.20	.20	.05		15	31
"	4					.60	.05				0	4
Observer H												
Test	1				.40	.60	.80	.40			10	37
"	2			.10	.30	.70	.30	.10			18	17
"	3		.05	.10	.25	.60	.20	.25	.05		21	29
"	4				.05	.85	.40	.05			27	19
Observer Mi												
Test	1			.20	.20	.40	.40	.10	.10		23	33
"	2					.50		.30			0	38
"	3					.80	.20				0	15
"	4					.60					0	31
Observer R												
Test	1			.05		.65	.30	.55	.10		3	53
"	2	.10			.05	.60	.30	.35	.05		17	[illegible]
"	[illegible]					[illegible]	[illegible]	[illegible]	[illegible]			
[illegible]	[illegible]					[illegible]	.05	.25			[illegible]	[illegible]
Average		.012	.035	.075	.200	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]	[illegible]

Table 3a

Numerators show the actual number of times the note was judged as c′ at different stages of learning; denominators show the actual number of times the note was presented. (The fractions are reduced to decimals for convenience of inspection in Table 3.)

Observer A

D. v.	232	240	248	256	264	272	280	288	296
Cents	58.70	56.76	54.45	52.43	50.94	50.19	48.77	47.44	
Test 1	1/10	3/10	2/10	2/10	4/10	5/10	4/10		
" 2			1/10		6/10	7/10	5/10	2/10	1/10
" 3			1/20	6/20	11/20	10/20	5/20	3/20	
" 4				3/20	7/20	10/20	3/20	3/20	

Observer B-C

D. v.	232	240	248	256	264	272	280	288	296
Test 1		4/10	1/10	3/10	2/10	5/10	4/10	2/10	2/10
" 2			2/10	4/10	0/10	5/10	1/10		
" 3			1/20	1/20	13/20	2/20	4/20		
" 4			3/20	2/20	8/20	7/20	3/20		

Observer F

D. v.	232	240	248	256	264	272	280	288	296
Test 1	1/10		3/10	3/10	3/10	3/10			
" 2		1/10	1/10	1/10	4/10	2/10	3/10		
" 3			2/20	3/20	11/20	4/20	4/20	1/20	
" 4					12/20	2/10			

Observer H

D. v.	232	240	248	256	264	272	280	288	296
Test 1				4/10	6/10	8/10	4/10		
" 2			1/10	3/10	7/10	3/10	1/20		
" 3		1/20	2/20	5/20	12/20	4/20	5/20	1/20	
" 4				1/20	17/20	8/20	1/20		

Observer Mi

D. v.	232	240	248	256	264	272	280	288	296
Test 1			2/20	2/10	4/10	4/10	1/10	1/10	
" 2					5/10		3/10		
" 3					8/10	2/10			
" 4					6/10				

Observer R

D. v.	232	240	248	256	264	272	280	288	296
Test 1			1/20		13/20	6/20	11/20	2/20	
" 2				1/20	12/20	6/20	7/20	1/20	
" 3					14/20	3/20	6/20	1/20	1/20
" 4					12/20	1/20	5/20		

Baird gives the percentages of octave-errors to correct-note judgments, for 4 of his 9 *O*s, as .31, .22, .10, and .03. Three of our 4 *O*s show corresponding percentages: .27, .21 and .05. Octave-errors are not, apparently, peculiar to the absolute ear.

If, in order to parallel Baird's results as nearly as possible, we consider simply, as he did, the *actual* number of confusions of the different intervals, our order from greatest to least is: seconds, fourths, minor thirds, semitones, fifths, thirds, tritones and sixths, minor sixths, and sevenths. This is not exactly Baird's order, since for him sixths come before fifths; but neither is it an order of community of overtones, and so it parallels Baird's in this respect. Our orders, then, confirm Baird's conclusion that community of overtones cannot be the basis of confusion in judgments of absolute pitch.

The order of frequency for confusion of intervals for our *O*s is not easily determined. If we count, for the moment, octave-errors as *errors*, we have this order:

correct judgments	88
errors of 1 semitone	32
" " " second	55
" " " minor third	37
" " " major third	29
" " " fourth	51
" " " tritone	13
" " " fifth	31
" " " minor sixth	10
" " " major sixth	13
" " " minor seventh	11
" " " major seventh	4
" " " octave	14
" " " " *plus* 1 semitone	6
" " " " " " second	2
" " " " " " minor third	3

When we consider, however, that the *O*s judged in terms of naturals only, and that the number of different intervals possible among the naturals is not uniform, some revision of the above figures seems necessary. On the basis of one diatonic octave there are possible 7 different cases of correct-note judgment, 2 different cases of semitone errors, 4 of seconds, 4 of minor thirds, 5 of thirds, 6 of fourths, 2 of tritones, 6 of fifths, 3 of minor sixths, 4 of sixths, 5 of minor sevenths, 2 of sevenths, 7 of octaves, *etc.* If we divide the number of errors of each interval made by the number of times it is possible for that interval to occur on the basis of the diatonic notes of one octave, the frequency of the errors will be found generally to fall off directly with the size of the interval. See p. 486.

Pre-training Experiments: Influence of [illegible]. Table 6 shows that under a narrowed [illegible] the *O*s were considerably more adequate, even before training. For, if we select from the first set of pre-training experiments those cases

Proportional number of:

correct judgments		12
errors of 1	semitone	16
" " "	second	13
" " "	minor third	9
" " "	third	9
" " "	fourth	8
" " "	tritone	6
" " "	fifth	5
" " "	minor sixth	3
" " "	sixth	3
" " "	minor seventh	2
" " "	seventh	2
" " "	octave	2
" " "	" *plus* 1 semitone	3
" " "	" " " second	5
" " "	" " " minor third	75

Baird did not have so difficult a problem in determining which intervals were most frequently confused, since his *Ss* did not restrict themselves to judging in terms of naturals, but inspection of his results (62) shows that they rendered more natural than accidental judgments.

where b was presented and judged, we find that it was judged correctly only 5% of the time. The average error, calculated from the notes wrongly judged as "b", is 259 cents. When judgments merely in terms of "b" or "not b" were asked for, they were correct in 36% of the cases, and the error was reduced to 222 cents.

In the case of B, we do not find any improvement under the narrowed *Aufgabe.* Of course if the individual had absolutely no notion of the pitch-region of a note, a heightened degree of attention would be of no avail; and it is true of B that he had no practical experience with musical instruments.

Other casual experiments not recorded here were made by the writer during the summer of 1924 on still other persons, and without exception there was a decided improvement under heightened attention brought about by such narrowing of the task.

Rate of Forgetting. Table 6 shows that this learning persisted for a month, and that it was even slightly augmented by the practice obtained from the test experiments.

Reaction Time. The average reaction time for correct judgments is 5.9 sec.; for incorrect judgments, it is 6 sec. There is even in the individual cases a slight difference in the direction of shorter time for correct judgment, but the difference is never striking.

Direction of Error. As in Group 1, underestimation of the notes presented is commoner than overestimation. (See Table 5.)

TABLE 6

	Average percent. of correct b judgments	Average error (cents)
Before training:		
Set 1.	.05	260
" 2.	.362	223
3 min. after training	.425	94
1 hr. " "	.487	99
1 day " "	.522	94
1 week " "	.450	92
1 month " "	.560	54

Results: Group 3

Procedure. Mu and P undertook to learn all the notes of the chromatic scale from g (196 d. v.) to f# (369 d. v.) inclusive. With P the tonometer was used. Pre-training ability was determined in the usual fashion; intensive training was given by sounding each of the 12 notes 3 times, under attention; and subsequently tests in which each note appeared twice in irregular order were given 3 times a week for 4 months. In these tests auditory distraction was always introduced between notes to prevent the formation of judgments on the basis of interval. After the learning was well under way, P was asked at alternate sessions to make her judgments immediately; in all cases where time was allowed, introspections were called for.

In the summer of 1924, Mu trained herself to the same pitches. No pre-training record could be made in this case because of Mu's experience as *E* in training the *O*s of Group 1. Common brass organ reeds, and the method of random selection at widely separated intervals of time, were found convenient for self-experimentation. Since the reeds were almost uniform in size, tactual clues were inconsiderable in comparison with the primary auditory ones. In this case, all judgments were made *ad libitum*, and except in those cases where the pitch was named immediately, introspections were recorded after each judgment.

Amount of Learning. With respect to the 12 notes to which they were trained, the degree of adequacy attained by both of the *O*s of this group places them easily among those who have absolute pitch. In the case of Mu, the final 96 experiments were 75% correct, and the average error was 27 cents; for P, the final 96 experiments were 62% correct, with an average error of 65 cents. Table 7 shows that again the learning was largely immediate, and in this case the fact is brought out more strikingly than before, since more notes are learned with less intensive training than was the case with either of the other groups. We also find in this group the usual variation in the records from day to day.

Errors. The results of the pre-training experiments of P show most of the characteristics of those of Group 2. Only 5 out of 192 judgments were made in terms of accidentals although accidentals were being presented; "c" judgments were more frequent than those of any other note (38); next in order come the "e" (33) and "f" (31) judgments; "a" stands next with 17. As a group, "c" ,"e", and "f" judgments embrace 9 out of the total number of 13 correct judgments. Errors are chiefly in the direction of underestimation of the notes presented. P, who has had considerable practical musical experience, did not make any octave-errors; her errors never exceeded the interval of the minor seventh.

TABLE 7

	Correct judgments (percent.)	Error (cents) Underestimation	Overestimation
Observer Mu			
Final tests (96 exp's)	.75	15	12
Observer P			
Before training (192 exp's)	.07	333	79
First test after training (24 exp's)	.38	96	54
Final tests (96 exp's)	.62	35	30
Immediate judgments (216 exp's)	.55	53	31
Natural judgments (216 exp's)	.62	25	21

After training, errors are usually less than a minor third. In 455 experiments, Mu never made an error of more than one tone, and in 500 experiments P made only 34. Underestimation was more frequent than overestimation for both *O*s.

Both *O*s made a proportionately larger number of natural than of accidental judgments; but though Mu was slightly more adequate to the naturals, P found the accidentals easier to judge. The individual notes which were best judged included both naturals and accidentals in both cases; for Mu the two easiest notes were g and b, and for P, g and e′.

Immediate vs. Mediate Judgments. Table 7 shows that P's accuracy was slightly diminished when she was asked to judge notes immediately upon their presentation. Baird, on the contrary, found his *O*s somewhat more adequate under these conditions. His *O*s, however, were in this case responding in accordance with their habits, whereas P was accustomed to take her own time in forming her judgments ("natural judgments",

see Table 7); with only a few notes had learning progressed to the stage where immediate response was natural. In the experiments in which she was allowed to take her own time, the average reaction time for correct judgments is slightly less than that for incorrect judgments.

Introspective Results

The objective data show that our *O*s acquired absolute ear for the notes to which they were trained. We have still to consider their introspective reports to discover, if possible, the basis of absolute judgments. First we shall inquire whether there is enough uniformity in the introspection to indicate that there is some single basis upon which absolute judgments rest. The reports do not warrant such a conclusion, for we find a great variety of clues not only among the various *O*s but in the introspections of each individual as well. There are clues from pitch, timbre[54], kinaesthesis, synaesthesia, association, and logical processes, acting sometimes alone, and sometimes in various combinations.

There are, however, a few general statements that can be made. In the first place, all *O*s have recourse to some kinaesthetic placement of the notes, like implicit singing or whistling. In the early experiments these cues frequently served as the principal basis for judgment. More and more, however, kinaesthesis tends to drop out, though it is usually summoned in difficult cases. Concomitantly, there is developed a finer analysis of the timbre of the note, and upon it, then, the greater burden falls. Especially for the 8 d. v. differences do the kinaesthetic cues seem inadequate. This fact is explicitly stated by 4 of the 6 *O*s of Group 1, and is implied in the reports of the other two. A stated that singing did not give a sufficiently exact placement, and that timbre seemed a finer criterion. B-C stated that in these experiments he gave practically no attention to pitch. M said that she determined c′ mostly by timbre, but discarded extreme notes immediately by pitch. (This is the most explicit statement of what is very generally implied in many of the introspective reports. Taken with the facts that extreme notes were in general discarded immediately, it means that pitch is a rough, immediate criterion, which serves to give a rough immediate placement of the note.) R asserted that pitch seemed a rough criterion, and often his reports upon individual cases show that, though pitch seemed satisfactory enough for c′, the timbre was not right for it, and that on the latter basis the note

[54] By "timbre" is meant gross qualitative differences between notes that are not fundamentally differences of pitches. The physical nature of the qualitative differences is unknown.

was rejected. It is also apparent from the reports of F that he usually relied upon a feeling of familiarity based upon pitch and timbre. H definitely passed from the stage where kinaesthesis predominated to that where he says he found relaxation better, with attention to timbre. The synaesthetic, associative, and logical cues are comparatively few, and were used by only 3 *Os*, chiefly in the early experiments. They include such experiences as colors, visual placements, sounds of boat whistles and sawmills, and such reasoning as that a note which came early in one series would come late in the next.

In general, also, the *Os* pass from a rich experience in the early experiments to a poorer one, which is sometimes so immediate that it cannot be analyzed at all. Sometimes the basis of judgment can be described as a feeling of familiarity, and sometimes as an outstanding feature of the total *Gestalt* of the note.

With respect to the correlation between speed and accuracy of judgment, we have already noted that in Group 2 there is too slight a correlation to be statistically valid, but in this group training did not proceed very far. With P, forced immediate judgments were found to be somewhat less accurate than the slower natural ones; but where the judgments were made *ad libitum*, reaction time for right cases was found to be slightly less than for wrong ones. It seems probable that with further training the degree to which our *Os* would make immediate judgments would approach that shown by Baird's.

Quantitative treatment of the introspection of P and Mu shows that the majority of their judgments were made chiefly upon the auditory clues offered by the note. It is impossible to say to what extent obscure kinaesthesis may have been present and determinative. Nevertheless, auditory features, especially of timbre, furnished the outstanding clues. Rarely did it happen, however, that pitch was so much overlooked that a very different pitch of similar timbre was mistaken for the correct note: the two criteria of pitch and timbre were not so divorced. Rather does it seem that, after a very rough initial placement of the note as high, low, or intermediate, some feature in its relation to the total tonal configuration furnished the basis for judgment. In difficult cases, kinaesthesis was consciously introduced, more often successfully than otherwise. Sometimes decision was arrived at by a process of the mental elimination of neighboring notes.

To the *Os* of all three groups, outside disturbances were very distracting, and requests for repetition of the presentation of notes were frequently made.

Summary of Results

(1) Average persons frequently possess absolute ear in some degree inasmuch as they can judge notes correctly, previous to training, in a greater number of cases than would be due to chance.

(2) Ability to judge notes correctly can be greatly improved by training, and in the region of 264 d. v. can be extended at least to notes only 8 d. v. (about 50 cents) apart.

(3) The improvement is largely immediate, and is relatively lasting in its effects.

(4) In making judgments of absolute pitch a high degree of attention to the notes is more effective than a lower degree; and a note which is not attended to has no noticeable effect.

(5) The majority of errors are small, and the frequency of error falls off directly as the size of the interval increases. Previous to training, some octave-errors occur.

(6) Errors of underestimation predominate for both men and women in the region between g (196 d. v.) and f'# (369 d. v.)

(7) A high degree of transfer of learning occurs when various organ-stops are used.

(8) There is a very slight correlation between speed and accuracy of judgment, except where the *O* is forced, prematurely, to make all his judgments immediately; then accuracy is slightly diminished.

(9) Ability to acquire absolute pitch correlates better with practical musical ability than with ability in the Seashore tests.

(10) From the introspective reports it appears that, in the course of learning, experience becomes less rich. Kinaesthesis, especially of the throat and mouth, has an important part in the beginning, but tends to give way to auditory features of the note. When judgment is difficult, kinaesthesis is summoned as an aid, and it is possible that kinaesthetic factors are always obscurely, if not clearly, functioning.

In the later stages of learning there is, typically, a rough immediate placement of the note presented for judgment; and, secondly, a finer placement made upon a clue furnished by some out-standing feature of the total tonal configuration. Sometimes judgment is based simply on a feeling of familiarity, and sometimes it is too immediate for analysis.

(11) There is no objective evidence which necessitates the postulating of an attribute of quality, or tonality, which recurs in successive octaves, and upon which all judgments of absolute pitch rest; nor do the introspective reports disclose the existence of any such attribute.

In connection with these results upon the specific question of the acquisition and nature of absolute pitch, there have been some which have a wider bearing; namely, those which show the influence of *Aufgabe*. It was found that when, previous to any training, *O*s were asked to name every note presented by its own name, they were less adequate to any single note than they were when asked to judge all the notes simply by reference to the name of one particular note. It was also found that, where the task was harder, as in the experiments with 8 d. v. differences, the frequency of correct judgments was less, but the number of large errors was also less, than in the easier experiments where only diatonic notes were presented.

Conclusions

(1) *The average person can acquire absolute pitch.* This statement is based on the fact that there is a similarity of performance between Baird's "gifted" *O*s and our group of average persons after intensive training. Similarity between the two groups is shown in respect of degree of accuracy, of nature of errors (size, direction, and individual differences), and to some extent in respect of immediateness of judgment, inasmuch as judgments became more immediate as learning progressed and, even where immediate judgments were forced, a high degree of adequacy was maintained. Indeed, we may go further and say that it seems true that most persons who are familiar with music have absolute ear in some degree even previous to training.

(2) *The possession of absolute ear seems to rest simply upon the giving of attention to notes as phenomena*: that is to say, upon an interest in the notes themselves rather than in their melodic or harmonic relationships, which, because of their much greater musical importance, usually monopolize attention. The importance of attention is indicated (*a*) by the immediateness and permanence of the learning; (*b*) by the individual variability from day to day; (*c*) by the increased accuracy under a narrowed *Aufgabe*; (*d*) by the fact that a stimulus which is not attended to has no noticeable effect; and (*e*) by the fact that a good hearing of the note is necessary before judgment can be made.

There still remains the question why certain individuals, without having been trained, possess absolute ear so much more completely than others. An answer may lie in the suggestion that such persons, probably when they were children, found notes interesting in themselves, perhaps because of an unusual degree of ability to deal with notes in this way. Vivid associa-

tions which the notes suggested,[55] establishing them thus in their own right and then in time fading out, may have been the counterpart of the experiences which our *O*s underwent in learning. In support of this suggestion it may be said that many persons who have absolute ear discovered it when they were very young; and that, in certain schools of music which claim to teach absolute pitch, frequent successes are said to occur with the children, who are taught according to the system of "fixed 'do' ", in which each note has only one name,—"c" = "do", "d" = "re", etc.,—acquiring thereby more individuality than in the system of "movable 'do' " commonly taught in the public schools.

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[55]R. Rolland, *Jean Christophe,* I; F. Galton, *Inquiries into Human Faculty,* 1883, 99, 182, 183.

THE PSYCHOLOGY OF *GESTALT*[1]

By Harry Helson, Cornell University

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(10) *General Considerations*

In a previous discussion of the theoretical aspects of configurationism I endeavored to show how the members of the school found their psychology anew upon a set of assumptions which shall guarantee the integrity of phenomenal and behavior patterns. The configurationists not only deny that the formal properties of perception and action can be derived from a knowledge of elementary sensational units or part responses like reflexes, but they also assert, in establishing the constructive part of their programme, that the form or structure of a complex, integrated whole exercises a constitutive effect upon the elements within the structure, and changes them in a manner predictable only from a knowledge of configurational laws.

It will be remembered that the main object of the configurational psychologists is to account for a given phenomenon without destroying it by a direct, frontal analysis. In place, then, of the older introspective or behavioristic analyses of perception and behavior, the configurationists substitute functional situations in which a change in conditions will result in a change in the fact studied. Functional variations are the chief means of formulating configurational laws. This method of procedure is in line with the demand that descriptions and explanations of psychological data must apply to the *phenomenal reality*, whatever it may be. Instead, therefore, of asking *what a given fact is*, the configurationists ask "*What is the function which determines the phenomenon?*" It is this point of view which has

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[1]This article is the second of a series on the psychology of *Gestalt*, the first of which appeared in this Journal, 36, 1925, 342-370.

led them to test old assumptions and to formulate new hypotheses in an endeavor to bring larger and larger classes of facts within the sphere of experimental psychology.

One can easily see that the configurationists have little difficulty with the stimulus-error or the terms to be used in *Beschreibung*. Since the observer is not called upon to analyze his experiences, but rather to report the object of his perception, no special psychological attitude or training is necessary. Hence we find untrained psychologists, men specializing in other subjects, and experts, taking part in the same experiments and agreeing very well in their reports. If the conditions of the experiment have been set and the observer is careful and of normal intelligence, even though he is psychologically näive, the problem of description is no different in psychology from what it is in the other sciences,—since the same (configurational) principles obtain in all the objective disciplines. To be sure, many facts require practice and training in observation, as the configurationists themselves admit; but this requirement is no peculiarity of psychology alone, and may well be expected in a science dealing with complex integrations like perceptual and behavior patterns.

Most of the facts studied by the configurationists require a totalizing or synthetic attitude favorable to the perception of, and the reaction to, wholes. Many observers seem to fall naturally into what has been called the analytic attitude, and often fail to report on configurational phenomena or do so only after practice and with difficulty. We shall have occasion to note the differences between those who do and those who do not react readily to configurational situations.

In this paper I shall trace the development of configurational theories by a consideration of the experimental findings upon which they are based.

(11) *The Figure-Ground Configurations*

The phenomenological study of figure and ground made by Rubin[2] appears to fit in so well with many of the conclusions reached independently by the configurationists that Koffka and Köhler have quoted this writer's work in detail as confirmation of their own main contentions, *viz.*, that there are no isolated sensations in experience, that analysis destroys phenomenal properties of perception, and that the facts of memory, recognition and behavior require the configurational point of view if they are to be made amenable to any general theory which shall embrace them all. In place, therefore, of descriptions of experience in terms of elements, Rubin and the configurationists would describe it as made up of definite configurations in which a figure protrudes from a more or less undifferentiated background. Figure and ground represent two fields, the

[2]E. Rubin, *Visuell wahrgenommene Figuren*, I, 1921, VII-244.

one of which predominates while the other sets it off. Qualities emerge from a general level, and hence must be considered with reference to their background if their properties are to be fully accounted for in perception. The presence of figure and ground in perception is necessary for the emergence of any qualities whatever; otherwise experience would present us with undifferentiated fields to which reactions would be impossible. Moreover, the fact that one is always conscious of, or reacting to, at least two fields in mutual dependence precludes one from describing experience by means of unit-elements or constant, independent factors like sensations, reflexes, or fixed associational mechanisms.

The distinction between figure and ground is avowedly phenomenological. It we examine our perceptions as we find them, we observe that certain parts of the perception have a clear, well-defined pattern while the rest is more or less homogeneous and in the background. One field may be called the figure, or positive field, while the other is the ground, or the negative field. It is often possible, with ambiguous figures like the reversible staircase, to change figure into ground and conversely. The usual explanation of this phenomenon, as Koffka points out,[3] is that attention turns toward different parts of the figure and makes them successively more vivid than they otherwise would be. The difference between figure and ground would then be merely a matter of difference in clearness. But, for Rubin and the configurationists, figure and ground possess different phenomenal properties, and are therefore two wholly different objects when perceived now as figure and now as ground. The object-character of any field depends upon its being the figure, having a definite form and structure. A thing is stuff *plus* form, ground become figure. The ground, however, must not be regarded as wholly unformed. The ground possesses certain vague, positive determinations; for one can say of it that it is a broad, extended surface, or a small rim around the figure.

The contention of the configurationists that phenomenal differences represent real differences in configurations is based upon the fact that a field when seen as figure possesses a higher limen for color induction than when seen as ground.[4] Katz' distinction between surface and film colors holds for figure and ground.[5]

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[3]K. Koffka, Perception; An Introduction to the *Gestalt-Theorie, Psychol. Bull.*, 19, 1922, 531-581.

[4]A. Gelb and R. Granit, Die Bedeutung von "Figur und Grund" für die Farbenschwelle, *Z. f. Psych.*, 93, 1923, 83-118.

[5]D. Katz, *Die Erscheinungsweisen der Farben, etc.*, *Z. f. Psych.*, Ergänz. Bd. 7, 1911. *Cf.* M. F. Martin, Film, Surface and Bulky Colors and their Intermediates, this JOURNAL, 33, 1922, 451ff.

The film colors are not localized definitely, lack form, and are soft and yielding, belonging to the ground; the surface colors, which are more stable, seem to belong to the figure.

Experiments in which ambiguous[6] fields were presented to *O*s showed that, once a field has been perceived as figure, "an after-effect exists which expresses itself in a tendency to experience in the succeeding trials the same field as figure which was seen as figure the first time."[7] This is called the figural after-effect, and according to Rubin cannot be explained by association. Association implies, he says, a connection between at least two elements, whereas in his experiments the same figure was recognized in succeeding trials without the correlative associative factor; only one figure was seen each time. There is a process at work analogous to association, we are told, but association does not adequately cover the fact that everything related to the figure is remembered better and centralizes better than what is related to the ground. The figure is more impressive than its ground, and may be connected with other figures more easily than connections can be formed between one ground and another.

The contour runs between figure and ground and is the common boundary of the two fields. Witasek[8] regards the contour as in itself a perceived configuration; but Rubin asserts that only the surface forms a configuration. The contour affects the figure more than the ground in view of the fact that it determines very directly the figure's form. If the contour were not present to differentiate the two fields, no figure would be present, and no meaning or significance would attach to the presentation. Contours thus affect the whole visible world as it appears to us.

For a thoroughgoing phenomenological point of view one should read Rubin's account of such perceptual complexes as the surface-figure, contours, lines, pointed properties, angular relations, breadth and distance. In this discussion he emphasizes differences which experience presents to everyday perception. I give two examples as illustrative of a method which substitutes phenomenological descriptions in the place of analytical descriptions. An angle, we are told, is the result of the relations between two contours, not the property of either one of them. The surface included by the contours is a property of the totally experienced figure and may be called a prong (*Zacke*). Prongs may be pointed, or blunt, or neither. For most people a prong does not seem pointed if it has an angle of 90°. Or consider the phenomenological differences between a surface and a contour: a surface has no parts, being all of a piece, while the contour which has parts constitutes a unity. If an *O* is asked to describe a square piece of paper, he will picture it as a formed surface without parts, while the contour will be described as a unity arising from parts. Phenomenological differences of this sort may be of great importance in determining the actual constitution of one's perceptual world and the reactions one makes to it.

Koffka has applied some of Rubin's phenomenological descriptions to the facts of child psychology, and derives the concept of the configuration from the more general fact of figure and ground as follows. "The first phenomena are *qualities upon a ground*. They are the simplest mental configurations. The phenomenal appearance in consciousness divides it-

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[6]Not all figure-ground configurations can be reversed. Some fields can not be changed from figure to ground and conversely. Really, Rubin deals only with what Werner has called "poly-figurations" or configurations capable of a number of phenomenological phases. "Mono-figurations", in Werner's terminology, cannot be reversed. H. Werner, Rhythmik, eine mehrwertige Gestaltverkettung, *Z. f. Psych.*, 82, 1919, 189-218.

[7]Rubin, *op. cit.*, 13.

[8]S. Witasek, *Psychologie der Raumwahrnehmung des Auges*, 1910, 348ff.

self into a given quality and a ground on which the quality appears... It is, however, a part of the nature of a quality that it should lie upon a ground....Such a coexistence of phenomena in which every member 'carries every other,' and in which every member possesses its peculiarity only by virtue of, and in connection with, all others, we shall henceforth call a *configuration*. According to this view the most primitive phenomena are figural."[9] Order, according to the configurationists, is therefore present from the very beginning, since the perceptual and reaction consciousnesses arise only when an emergent from a general level greets the new-born individual.

The concept of figure and ground provides the configurationists with a means of investigating functional differences within a given situation. Its phenomenological value is evident, since it can apply to any sense modality and to behavioristic as well as to phenomenal data. This may not seem possible at first glance; but a sufficiently broad use of words enables one to make the application. Thus we may say that sounds arise from a background of other sounds or stillness; tactile experiences may emerge from diffuse surface or organic pressures, pains or the like; in short, any experience appears to the percipient as either a continuation of some previous one, on the same level, or as a rise or fall from a level, or as a change in figure within a given level. From the point of view of the behaviorist, the first reactions are not to "simple" stimuli, but rather to situations which present "step-wise" or differential aspects fundamentally. related to figure and ground. Figure and ground may thus be said to be the simplest configurations which are observable in perception and to which a discriminatory response can be made.

(12) *The Perception of Movement*

It is within the field of the perception of movement that the configurational theories took their rise and have received their most systematic application and development. It will therefore be necessary to examine in some detail the experimental work bearing on the problems of apparent movement, if we are to understand why the configurationists have advanced their theories, on what evidence they base them, and in what direction configurational methods are tending. Our survey of Rubin's work on figure and ground has shown to what lengths phenomenological descriptions may go, but it has not brought out what the configurational work on movement will show, *viz.*, the effect of varying specific conditions within a total complex in such wise that laws can be stated in terms of the variables responsible for the phenomenal pattern; in other words, how functional dependencies can be traced in configurational phenomena.

The reason that the field of apparent movement has been so productive of results is, perhaps, that it offers some of the most

[9] K. Koffka, *The Growth of the Mind*, 1925, 131.

difficult paradoxes to be met with in experience, and hence requires a theory which shall embrace a large number of facts which at first sight do not seem to harmonize into any single scheme. The most noteworthy attempt of the configurationists has been to explain the facts of real movement, apparent movement, and phase movement, whether within a single sense modality or across sense modalities (movement from touch to vision!), by means of one set of assumptions and principles.

Let us first note the various possibilities under which movement may be perceived. There is, of course, the perception of a real, single object in motion. The problem of inferred motion, like that of the movement of the hour-hand of a watch, does not here come into consideration. If two stationary objects are exposed in quick succession, as in the stroboscope, they may be perceived as a *single* object in motion. Even a single stationary object may, under the right conditions, be seen as moving. A single moving object may be seen as at rest in different places at different moments. Finally, a number of cross-sectional stationary phases of moving objects, as in the moving picture, may result in the appearance of movement. Any theory of movement must account for real movement, for the apparent movement of stationary objects, for the failure to perceive real movement when it is present, and for the perception of movement when successive phases of a moving object are seen. From the phenomenological point of view apparent movement may possess all the descriptive properties of real movement; hence to explain away apparent movement by designating it as illusory does not account for the fact that real and apparent movement appear so much alike they often cannot be differentiated.[10]

From our discussion of the divers ways in which movement may be perceived or may fail to be perceived it is clear that the phrase "the perception of movement" is highly ambiguous. It becomes even more ambiguous when we inquire into the meaning of the word "perception"; for as soon as we do this we become involved in the problem of the psychologically real. If we consider perception as the end-result of a stimulus, then stroboscopic movement is not real, since nothing real has moved; and we have to explain the 'illusion' of movement. But in view of the fact that *O*s are unable to distinguish between real and apparent movement on the basis of their immediate experience, apparent and real movement seem to possess the same phenomenal properties: so far as psychology is concerned, we have no reason for calling the one illusory and the other real. It must be granted that without perception there would be no move-

[10]F. L. Dimmick and G. H. Scahill find descriptive differences between real and phenomenal movement. *Cf.* Visual Perception of Movement, this JOURNAL, 36, 1925, 412ff.

ment of any kind, since movement depends upon the apprehension of a stimulus during at least two instants of time and through two points of space. Hence, as Kehr has pointed out,[11] the perception of movement is a special case of perception in general, and may throw light upon the whole problem of perception.

A considerable amount of experimentation preceded Wertheimer's investigation of apparent movement, and out of this experimentation a number of theories had been advanced to explain the observed facts.[12] All the theories were built upon the assumption that the perception of movement could be analyzed into simpler constituents, or at least might be correlated with physiological processes and the sensations which accompany them. Wertheimer's paper of 1912 contains the results of a series of experiments designed to test the prevailing theories and the assumptions underlying them. Some idea of the confusion in the field may be gained from the different views of movement held by various writers as a result of their analyses: according to Marbe apparent movement is due to a fusion of after-images; Wundt also held a peripheral explanation in terms of eye-movement; Exner, Stern and others maintained that movement requires a special kind of sensory process, thus adding to the growing list of sensory elements; still another theory, to mention only one more of those in the field, implied that the perception of movement required a founding process, analogous to the founding process of Ehrenfels by which the form-quality is produced over and above the founded sensory elements. In all these theories the assumption is made that perception can be broken up into something simpler which will correlate directly with the stimuli. None is concerned with the necessary and sufficient conditions of all kinds of perceived movement and with the endeavor by their means to connect it with other phenomena which are admittedly simpler and more fundamental in origin. The upshot of the experiments conducted by Wertheimer may now be briefly stated: the perception of movement is a perception *per se*, not a perception involving new elements, but rather a perception differing from other perceptions in certain describable attributes which are lost in the process of direct analysis. Having found adequate descriptive terms for the phenomenon, Wertheimer proceeded to correlate changes in the objective conditions with changes in the *experience of movement*,

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[11]Th. Kehr, Allgemeines zur Theorie der Perzeption der Bewegung, *Arch. f. d. ges. Psych.*, 34, 1915, 106ff.

[12]M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Z. f. Psych.*, 61, 1912, 161-278. A bibliography of the literature and theories mentioned below is given in this article. The argument and the facts from here onward follow this article very closely.

making a functional or indirect analysis of the facts he was studying.

Wertheimer's method of procedure was, in general, as follows. Two black lines were exposed in succession on a white background, the one vertical, the other horizontal, in such fashion that if both were seen simultaneously they would form a right angle. The colors of the lines and of the background were varied. Figures of various forms and in various arrangements were introduced and observed. With slow rotation of the stroboscope first the one line appears at rest and then the other. Quicker rotation causes both lines to appear stationary at the same time. If the speed is now adjusted between these two speeds, the one line appears to move over to the other, and often returns to its original position. This is Wertheimer's optimal movement or "phi phenomenon." Other kinds of movement may also be seen under conditions which are now known: each line may move by itself, independently of the other, and this Wertheimer calls successive movement or dual part movement; the stage in which both lines are seen at rest is called the simultaneous stage.

A number of questions arise concerning the part which the time interval between the stimuli plays in determining the kind of movement seen and the influence of other variables, notably the order of presentation of the stimuli, the position, mutual distance, form and color of the objects seen in movement. Subjective factors like fixation and attention were studied and controlled. Later a third object was introduced into the field of vision, to complicate the situation, and its effect upon the movement was noted. The influence of after-images and other after-effects was also taken into account.

We may summarize Wertheimer's results briefly as follows. Optimal movement occurred with most *O*s when there was a pause between the stimuli of 60 σ. Stationary simultaneity appeared at 30 σ, while stationary succession of each stimulus occurred at about 200 σ. A more detailed discussion of the influence on movement of the factors mentioned above will be given in the consideration of later work. Let us consider the facts advanced by Wertheimer against the prevailing theories of movement.

The evidence against the eye-movement theories seems conclusive, since the time required for movement (60-100σ) was less than the minimum required for eye-movements (130 σ). Furthermore, fixation was so controlled as to reduce or rule out eye-movements; and when after-images were projected upon a fixed spot they remained stationary while the lines moved. Finally, several movements could be seen simultaneously in different directions, a fact which precludes the possibility of the eye-movement explanation for these cases.

Theories involving a union of the lines stroboscopically exposed, whether by means of founding central processes or of peripheral fusion processes, are also rejected by Wertheimer. So, too, the central completion theories are discarded, because the optimal conditions show a single, identical thing moving, which may change in color or length while in motion. The fact [illegible] single moving object is seen is not to be confused with a conviction [illegible] that one object moves, it is not a matter of seeing one line and then [illegible] [illegible] cannot distinguish between this apparent movement and that of a single moving object. Of course, various kinds of mov[illegible] [illegible] be seen under [illegible] the [illegible] *O*; for example, continuous, jerky, single, and part or dual movement were all reported.

Some further observations have interesting theoretical consequences. The introduction of a third object into the field of movement, either like the other two or differently colored and formed, may change the original movement. With proper fixation the third object appears at rest while the other two move; or the third object may move independently of the original two. Two parallel lines, *a* and *b*, may be exposed with two lines *c* and *d*, forming an angle, *a* opposite *c* and *b* opposite *d*. Exposures favorable to movement of the lines forming the angle, *c* to *d*, were not favorable to movement of *a* to *b*. But if one of the parallels, *a*, is exposed with the oblique line *c*, then good movement appears of *a* to *b* as well as movement from *c* to *d*. And this movement would take place when the exposure of *a* and *b* alone gave almost no movement. Wertheimer compares this result to a dead interval which is quickened into life by a live one in its vicinity. It may be thought that since *a* was connected with *c*, and *b* with *d*, therefore good movement took place from *a* to *b*; but since good movement took place from *a* to *b* when *c* and *d* were unnoticed, this explanation does not hold. Nor do the lines *c* and *d* partially absorb the attention, because a lack of attention is unfavorable to the perception of movement. Experiments with distraction from *a* and *b* did not give a better movement. The effects of attention receive detailed consideration in the paper we are discussing and in the papers on movement which are to follow; but it may be stated in general that the configurationists do not believe that the explanation of apparent movement is to be found in processes going on at the periphery of attention; in fact, the best movements occur at the center of attention or are enhanced by "concentration of attention."

The unitary, unanalyzable nature of the phi phenomenon, which the configurationists have stressed, centers largely upon the following facts: (1) for the most part nothing was seen of the lines; "something moves" is all that can be said; (2) only one of the lines may appear; (3) parts or pieces may appear, but such parts or pieces may not be seen until after the completion of the movement. Some *Os* even reported that they saw movement but no object which moved; this pure movement Wertheimer called "pure phi"; it is incapable of further analysis or of any description other than that 'movement was seen'. Wertheimer concludes that movement is not built on stationary elements and cannot be analyzed into static elements; phenomenally, the perception of phi possesses all the descriptive attributes of real movement. To call it an illusion of judgment would be false, because full knowledge does not alter the seen movement. Nor does the central-completion theory suffice, because the movement may occur with no semblance of an exciting stimulus. With red lines on a black ground, the one line moved over to the other but no red appeared, while an absence of color was noted when white lines on a black field were exposed. Pure phi contains no perception of line, color, content or founding process. Wertheimer's characterization of pure phi phenomena is as follows: "Im Gegensatze zu anderen psychischen Gegebenheiten sind sie nicht statischer sondern dynamischer Natur; in dem spezifisch charakterisierten 'Hinüber' usw. haben sie ihr psychologisches Fleisch und Blut, nicht zusammensetzbar aus dem der üblichen optischen Inhalte."[13]

Whether or not such a thing as pure phi exists is after all a secondary matter compared with the main result of Wertheimer's experiments. The facts which he discovered led Wertheimer to the conclusion that perception cannot be analyzed into simple, sensational units and explained by their means. An explanation of apparent movement, or of any other perception, must spring from an entirely different source,

[13] *Ibid.*, 227.

namely, from a functional variation of conditions which are responsible for, favor, or hinder it. Only in this way can laws of perception be formulated and the facts of one domain be related to those of another. Any theory of the perception of movement must therefore embody the facts of movement *qua* movement and must embrace the physiological antecedents of the psychological event. The configurational theory proposed by Wertheimer was designed to fulfill these conditions and to stimulate further investigation. The theory must explain optimal, dual and successive movements; it must explain pure phi,[14] colored movement, and the simultaneous stage; in short, any theory must account for all the facts of movement as distinguished from all the other varieties of perception we know.

The configurational principle advanced to explain the perception of movement has not only been extended to include all the facts of perception but it has also been erected into an universal postulate on which the configurational psychology has been reared. The psychological and physiological processes involved in the perception of movement must be regarded as total, structured processes, unified in time. The perception of movement is a configurational phenomenon because it represents an unified experience which cannot be analyzed into simpler elements without forfeiting the peculiar *quale* by which we recognize the experience; furthermore, in the experience of movement there are super-summative properties not derivable from the properties of the parts as such, And the configurationists maintain that these statements hold true of all perception, memory, thinking, acting,—of experience in general. The investigation of configurational phenomena is not limited to psychology. Psychology, which depends upon physics, chemistry, biology and related sciences, requires concepts which receive confirmation in these other fields. In this way natural-scientific laws can be formulated in psychology, enabling psychologists to explain, control, and predict the data which they observe. Let us see how configurational principles have been elaborated along these lines.

Since the phenomena described by Wertheimer are in some sense 'illusory', arising from the successive exposure of two stationary stimuli, the

[14]Although Koffka and Köhler are inclined to accept the fact of a pure phi, objections to it have been raised. Benussi repeated Wertheimer's experiments both in touch and vision with 27 *Os*, and found that "something moved" even though the *Os* could not tell what moved. Benussi [illegible] in the sense of a movement without an object." V. Benussi, Versuche [illegible] *etc.*, *Arch. f. d. ges. Psych.*, 36, 1917, [illegible] *Os* reported that the movement could be analysed into a "gray flash in time." [illegible] and the Phi Phenomenon, this JOURNAL, 31, 1920, 317-332, and the later [illegible]

question arises concerning the effect of figures of the Müller-Lyer type upon the kind of movement perceived. In other words, if two figures are exposed in succession, the one of which *seems* larger or smaller than the other, will there be a difference in the kind of movement seen as compared with that produced by two stimuli which appear to be equal in size? Will the figures seem to grow or shrink in size as the second one is perceived? This is equivalent to asking whether the phenomenal configuration can have any effect upon the kind of movement perceived, whether the figural factor in a situation is in any sense a really causal factor. Other problems also call for solution: the effect of changing the distance and the time between the stimuli; analytic *versus* synthetic attitudes on the part of the *O*; the direction of attention and the rôle of fixation; the effect of the second stimulus upon the first. These and a number of other questions were answered by Kenkel, who used the Schumann tachistoscope in which two figures were exposed in quick succession and were observed either in one exposure or in a number of serial exposures.

Three kinds of movement are described by Kenkel, according as they arise from figures of the Müller-Lyer (or illusory) type, denote the real differences in the size of the stimuli, or concern only one stimulus. The three kinds of movement are differently labelled. *Alpha* movement depends upon illusions of magnitude. In *alpha* movement the parts of figures exposed successively seem to change in size, growing larger or smaller, as they move; *e. g.*, the central line of the Müller-Lyer figures. *Beta* movement depends upon actual differences in the size of the stimuli and involves a change in the magnitude of the figures as the first moves into or grows into the second. *Gamma* movement concerns the movement within a single figure as it expands and contracts when it is exposed and as it is fading from view. Descriptively, the *alpha* and *beta* movements are qualitatively like each other; so that we must admit that the *apparent* magnitude of a figure exercises the same effect upon the perception of movement as do *real* differences in magnitude.

"The *alpha* movement," we are told, "can rest only on one fact: the configurational process released by the second figure is so modified by the preceding configurational process that between the two processes phi-*alpha* arises alongside of the *beta* movement conditioned by the different spatial positions of the two stimuli."[15] In other words, both stimuli must be regarded as forming a single configurational whole. Memory, past experience and the like cannot be used to explain the facts, although practice was often necessary in order to see the phenomena at their best. Often it was possible to see all three movements within the compass of a single exposure.

From the configurational point of view the experiments of Wertheimer and Kenkel are important because they show that the various factors in the stimulus-situation must be taken as a whole, as well as the perception itself, if phenomenal movement is to be seen. Moreover, the figure or form of the stimuli, their arrangement with regard to each other, and their actual spatial patterns, are all determinants of the perception. The term configuration (*Gestalt*) includes not only the spatial configuration but also any and all factors which may change the experience, such as the time between the stimuli, the spatial distance, the intensity, and whatever else affects the movement. In fact, we may say that a configurational system is a complex

[15] F. Kenkel, Untersuchungen über den Zusammenhang zwischen Erscheinungsgrösse, *etc.*, in *Beiträge zur Psychologie der Gestalt*, edited by K. Koffka, 1919, 49.

in which a change of any one part results in a change in the whole. If the total complex is intricate and involved, then it takes a longer time to comprehend all the parts within a unitary figure, and longer exposures are necessary if good movement is to take place. An analytic attitude destroys movement, as practice helps it to appear. Benussi and Wertheimer had shown that exercise in a given kind of movement makes its reappearance easier, so that even in configurational phenomena the effects of past experience come into play.

But other factors may determine the perception of movement, factors which, unless we call the total complex of subjective and objective conditions a configuration, would seem to lie outside the configuration proper. Attention, if it is not constant, may upset the whole phenomenon, changing the apparent movement both quantitatively and qualitatively. Both Benussi and Schumann had noted this fact earlier.[16] Well-divided attention over the whole field favors the appearance of the various kinds of movement. Moreover, the "constitution" of the *O*, his attitude, his previous practice and past experience, may play a part in determining the kind of movement seen. All of which goes to show that subjective factors apart from the objective configuration may be of tremendous importance in determining what will be perceived and in making against the constancy of configurational phenomena. To take care of all the variables within the organism, as well as of those without, in a single causally-coherent configurational system, Kenkel gives expression in the following way to what may be called the total configurational structure in which phenomenal movement is enmeshed: a configuration appears in experience in a certain form and size determined by the physical stimulus, the retinal image, and the complex K. The physical stimulus is constant (or may be kept so), while the complex K in the organism is not constant, depending upon the direction, division and strength of attention, the configurational dispositions, attitude, practice, and a number of other variables at present unknown. It would therefore appear that the configurationists can hardly dispense with the older concepts of attention, attitude, practice and the like, to the extent to which their initial programme committed them. So many variables must be included within the configuration that, in its broadest sense, it means no more—and no less—than the totality of conditions both outside and inside the organism.

[16] V. Benussi, *op. cit.*, [illegible]; [illegible] Schumann, *Z. f. Psych.*, 30, 1902, 289. Benussi asserts that attention is a necessary condition of the appearance of illusions, just as it is of many other phenomena of perception, but it is not a sufficient condition. *Cf.* also an article by Benussi, Stroboskopische Scheinbewegungen und geometrisch-optische Gestalttäuschungen, *Arch. f. d. ges. Psych.*, 1912, 24, 31ff.

Whether the factors within the organism can be reduced to processes amenable to the laws of physics and chemistry or included under the rubric of sensational experiences has been a matter of controversy between the new configurationists and the Graz configurationists. Benussi had investigated the types of movement described by Kenkel, and treats them as illusions subject to the laws of illusion or as "supra-sensuously" conditioned phenomena.[17] According to Benussi's theory, if an illusion is to make itself perceived, a synthetizing attitude on the part of the *O* is necessary. Exercise in the synthetic attitude is accompanied by fatigue in the analytic attitude and conversely (law of double exercise). There is thus an exercise-fatigue equivalence. Illusions arise from an "inadequacy process" conditioned by the inadequacy carriers in illusory figures: in the Müller-Lyer figures the inadequacy carriers or factors favoring the illusion are the arrow-heads and feathers, in the Zöllner figure they are the transverse lines. So any factor favoring the illusion will aid the *alpha* movement. Benussi found that with the extreme analytic attitude the *alpha* movement could be made to disappear entirely. Kenkel does not mention this fact.[18]

The difference between Koffka-Kenkel and Benussi is regarded by the latter as merely a difference in terminology; but it is more than that, since Benussi's *Produktionsvorgang* (the special psychical act responsible for the inadequacy process) determines his whole theory of perception, and results in a bipartite psychology of sensation and perception, of subjective and objective processes.[19] The new configurationists insist that all the facts must be explained by means of a single principle accounting for discrepancies between sensational and configurational experiences and the stimuli from which they arise. There can be no doubt that Benussi has emphasized an important point with regard to configurational experiences, namely, their variability as opposed to the apparent constancy of the sensational or qualitative aspects of perception. Just as the configurationists have stressed the lack of agreement between sensation and stimulus, so Benussi has insisted that configurational phenomena are subject to distortion as compared with the objective situation. The fact that configurational perceptions are no more constant than sensational processes, and the resulting discovery of the analytic and synthetic attitudes as important determinants of configurational perceptions, have opened up a wide field for investigation concerning the perception of wholes and complexes.

Further investigation of the effect of the objective configuration and subjective factors upon the *gamma* movement was made by Lindemann, who exposed a single object tachistoscopically under various conditions.[20] Lindemann studied the effect on *gamma* movement of varying the exposure time, the intensity and size of the objective configuration, and subjective factors like attention, fixation and fatigue. The results were confirmatory of Kenkel's findings; but *gamma* movement is not a result of attention alone. Rather, Lindemann's general conclusion is stated in terms of con-

[17] V. Benussi, Gesetze der inadäquaten Gestaltauffaussung, *Arch. f. d. ges. Psych.*, 32, 1914, 396-419.

[18] It will be remembered that Schumann, in his four articles on the illusions, emphasized the fact that the method of "total impression" favored the appearance of the illusions. More will be said later concerning the analytic and synthetic types of observation and their effect upon configurational phenomena.

[19] Benussi, *opp. citt.*, and Experimentelles über Vorstellungsinadäquatheit, *Z. f. Psych.*, 45, 1907, 217ff. Benussi has modified his theories in later papers. A summary of his views will be found in Gesetze der inadäquaten Gestaltauffassung, *Arch. f. d. ges. Psych.*, 32, 1914, 396-419.

[20] E. Lindemann, Experimentelle Untersuchungen über das Enstehen und Vergehen von Gestalten, *Psych. Forsch.*, 2, 1922, 5-60.

figurational tendency, which may be said to express the most general law of configurational change: all phenomena of movement are strongly determined by the tendency of configurations to become as "good" as possible. While no precise definition of "goodness" is given, it seems to imply that configurations tend to become simpler, more stabilized, and to realize an optimal form.

A good configuration may be distorted by the introduction of a point near its border. Thus if a point is introduced into the periphery of a circle, the *gamma* movement is weaker at that point and the figure may be deformed; the circle becomes rectangular in shape. On the other hand, if the point is exposed alone, it appears in a different place from that which it would occupy if exposed in or near a figure.

The effects of fixation and attention are opposite: fixation destroys the movement, while attention favors movement in the part attended to. If the end of an ellipse is attended to, the direction of the movement is toward that end, "and seems to draw the rest of the figure after it." If the diameter of a circle is attended to, then the movement seems to be along that line. But when a part is *fixated*, the movement in that part is *poorer*. Fixation inhibits, while attention facilitates, the perception of movement.

According to Lindemann, it is evident that configuration influences the perceived movement, its direction, amount, and quality. The movement of a circle is stronger in its horizontal than in its vertical direction, although movement is present throughout the circle as a whole. But if we contrast the square with the circle we find that in the former parallel sides move outward and back, the vertical lines move more than the upper horizontal, and the lower line moves least of all. If however, the square is tilted on one corner, then all four angles "shoot energetically outwards and back as if it were a wholly different configuration." From this it appears that two directions are preferred, the horizontal and the upward, a fact which may have some bearing upon the anisotropy of visual space. Another fact of interest in these experiments came out when pointed figures were used: the movement in such figures was stronger toward the pointed parts than toward the rounded parts, a fact which is suggestive of the tendency of electricity to flow in the direction of a point. Figures drawn in perspective showed movement in the third dimension.

Several general properties of configurational phenomena emerge from Lindemann's descriptions. "Good" configurations tend to resist change and to preserve their form. If it is impossible for a figure to change in the direction of a simple configuration, a new configuration tends to appear. If the way to two configurations is equally short and easy, then the direction of the movement is uncertain and unclear, lacking univocal determination. Complicated configurations tend to change into more simple. The apparent size of a configuration is a function of the time of exposure: a certain minimal time is necessary for a configuration to reach its full size.

Many of the facts described by Lindemann received consideration and elaboration in Hartmann's work on the fusion of two figures exposed successively in a Schumann tachistoscope.[21] It is possible to change the time of exposure of each stimulus, the time between stimuli, the spatial interval between the figures, the intensity of light, the magnitude and form of the configurations. Fixation and other subjective factors were also taken into account. Although, as Hartmann points out, the phenomena of optical fusion had been studied at length, the earlier results led to no univocal and clear conception of the processes concerned in fusion phenomena. Nor [illegible] relation with other facts, especially with those of movement and configurational [illegible] Koffka had already pointed out the relation between fusion and movement,

[21] L. Hartmann, Neue Verschmelzungsprobleme, *Psychol. Forsch.*, [illegible], 1923, 319-397.

and had proposed a theoretical explanation of both which we shall consider later. Hartmann's research was therefore concerned not only with fusion phenomena but also with the way in which configurations arise, disappear, and change. His object was to ascertain the limen of fusion of two figures, either alike or different in form, and to note the effects of the spatial-temporal variables mentioned above.

It was possible to compare the fusion-points of figures like the triangle, circle, square and ellipse by determining the speed of rotation which would result in their fusion. The optimal relations of the variables for the fusion of various figures are best expressed in Korte's and Cermak's laws, which I shall next discuss. Here it suffices to point out that configurations possessing the simplest and firmest structures tend to fuse the quickest. On the other hand, if different figures are offered for fusion successively, each tends to preserve its own form. The law of constancy here prevails over that of fusion. Different figures of the same surface area and intensity possess different limens of fusion: thus the point at which a circle fused and was steady would give flicker when triangles of the same area were inserted. Different figures required different speeds of rotation in order to fuse. It thus appears, again, that the configuration as such is constitutive, exerting a real effect upon certain measurable phenomena.

Lindemann had shown that the configuration and *gamma* movement are closely linked together. Hartmann showed that the configuration is of importance upon the limen of fusion. Phenomenal configurations, even though illusory figures, exerted a real effect upon fusion, just as was the case in Kenkel's experiments on movement. Thus, if a figure-ground configuration was exposed, the configurations possessed different limens of fusion according as the one field or the other was taken as figure. When a field is ground it tends to fuse quicker than when it is figure, a fact which was made the basis of a special investigation by Gelb and Granit, and will be discussed later on.

The results of Hartmann's experiments may be stated thus. The effect of a configurational structure is more than summative, since the effects of mere geometrical or arithmetical increase in the size of the stimuli, the intensity of the light coming from them, and all other factors of importance for the phenomena studied, are to be explained only in the light of configurational changes and their influence upon the configuration as such. We may therefore say that simple and firm configurations tend to fuse quicker. On the other hand, configurations tend to maintain their form as opposed to radical change. The strength, direction and distribution of movement *within* figures depend upon a number of factors, chief of which is the configuration. The effect of any variable, indeed, depends upon the configuration: thus the factor of intensity has less effect upon "good" configurations than upon poor. In short, the limen of fusion depends upon the following factors as they influence the configuration: the intensity of the stimuli, the temporal relations of the stimuli, and the form of exposition. The detailed results bearing on the effects of surface *versus* outline configurations, and the breadth and position in space of the stimuli, can not be recounted here, nor the minor differences between Hartmann and his predecessors regarding the effects of fixation and the like. Hartmann's final conclusion regarding the fact that *alpha* and *beta* movement and the various stages of fusion are amenable to the same laws carries us back to the laws of Korte and Cermak upon which it is based.

We have already found that variations in the time of exposure, the spatial interval between successive stimuli, and the intensity of the illumination, all have an effect upon the kind of move-

ment perceived. Korte[22] undertook to vary some of the objective conditions, while the rest remained constant, in order to formulate the laws of dependence of movement upon the various factors which can be controlled in the stimulus-situation, on the assumption that the subjective conditions were for a time kept fairly constant. Korte hoped in this way to find an objective explanation for many of the facts which Kenkel had assigned to subjective causes. On the basis of the Wertheimer physiological theory, Korte was led to expect that it might be possible to compensate for the priority in time of the first stimulus by increasing the intensity of the second, and thus to determine the direction of movement. The four variables which Korte controlled were the absolute intensity or size of the stimuli, the difference in brightness of the stimuli, the time of exposure of and between stimuli, and the spatial distance between the stimuli. The effects of these variables were noted on the *beta* movement and on a fourth kind of movement which Korte called the *delta* movement, described as follows: if two stimuli (of which the one is more impressive than the other, owing to its greater magnitude, intensity, or variant form) are exposed in a Schumann tachistoscope, the stronger stimulus will move to the other stimulus; whereupon both may unite and return to the place of the first. The main characteristic of the *delta* movement consists in the return of the first stimulus to its original position, whether or not the second accompanies it.

The Korte laws applying to movement may be summarized as follows. "If the spatial interval is too great for optimal movement, then optimal *beta* and *delta* movement can be obtained by increasing the impressiveness of the stimuli (really their intensity); or by lengthening the pause between them. If the light intensity is too great, then optimal *beta* and *delta* movements appear with increase of the distance between the stimuli, or decrease of the time between their exposure. If the time is too great for optimal movement, one must either increase the interval between the stimuli or decrease the impressiveness. Optimal *delta* movement usually requires that the difference in impressiveness between the stimuli be increased."[23]

If we let s denote the distance between the stimuli, i the intensity of the stimuli, t the time interval between the stimuli, I the total impressiveness ($i.$ $t.$), *opt.* optimal condition, and $\sim$ the rough proportional relation which exists between these various factors, we may state the laws of Korte for optimal movement as follows:

1. $s_{opt.} \sim i$	$i_{opt.} \sim s$
2. $i_{opt.} \sim \mathrm{I}/t$	$t_{opt.} \sim \mathrm{I}/i$
3. $t_{opt.} \sim s$	$s_{opt.} \sim t$

[22]A. Korte, Kinematoskopische Untersuchungen, *Beiträge z. Psychol. d. Gestalt*, 1919.
[23]*Ibid.*, 177.

In the formulation of these laws subjective factors were assumed to be constant.

It appears, Cermak tells us,[24] that the laws of Korte have a more general significance than a statement about apparent movement implies; for certain parallel phenomena, which were thought to be very different, are at bottom amenable to the same laws and can be referred to the same processes. Korte's experiments were limited to stationary objects and to two phases separated by a fairly large spatial interval. The question now arises whether the laws of Korte are valid when the interval between the successive phases of the stimuli becomes smaller or when more than two phases succeed one another. The assumption is made that, if the distance between the phases is reduced to zero, then the phenomena of apparent movement and light-mixture are identical, since light-mixture is a limiting case of successive phases of an object in which the distance between them is zero; *i. e.*, the phases come from a single source, and hence there is a temporal, but no spatial interval between them.

Cermak's initial assumptions have been critized by Hillebrand, who believes that the perception of light-mixture consists in an immediate awareness of several excitations which have been fused into one, and must be regarded as the perception of a single, simple datum which is not the correlate of the stimulus-components. The difficulty for Hillebrand lies in the fact that the stimuli under Cermak's conditions have the same spatial position, whereas in stroboscopic movement the stimuli are separated. For Hillebrand the two facts are different, both in origin and with regard to what is phenomenally perceived. But since Cermak's whole problem consists in discovering whether or not strobosocopic movement and the phenomena of fusion *fall under the same conditions*, it does not make any difference what is perceived so long as the same laws are responsible for both experiences. Hillebrand's objections do not touch Cermak's position.[25]

Let us return to Cermak's actual experimentation. A black belt 148 cm. long and 2 cm. wide revolved upon two cylinders. On it were pasted two rectangles cut out of white paper, 6 by 20 sq. mm. and 2 by 20 sq. mm. respectively. These figures, for purposes of comparison, were so placed that they moved at right angles to their length. The rotating belt and figures were illuminated from above by an electric lamp. The rays from the light-source were interrupted by means of a rotating disc. Sectors had been cut out of this disc, leaving openings of different sizes through which any amount of light could be transmitted at any frequency. The speed of rotation of the belt could also be varied either with, or independently of, the light-frequency. The stimulus thus consisted of two white figures on a black belt moving at different rates (at different times) and illuminated by flashes of light whose frequency varied. It will be seen that here, in contrast with Wertheimer's work, it is not a question of two stationary stimuli, separated by a spatial-temporal interval, giving rise

[24]P. Cermak and K. Koffka, Untersuchungen über Bewegungs- und Verschmelzungsphänomene, *Psych. Forsch.*, 1, 1921, 66-129.

[25]F. Hillebrand, Zur Theorie der stroboskopischen Bewegungen, 1, *Z. f. Psych.*, 89, 1922, 209-272, and 2, *ibid.*, 90, 1923, 1-66. Wertheimer has answered Hillebrand on his criticism of the "short-circuit" theory in *Psych. Forsch.*, 3, 1923, 106-123. See also Hartmann's treatment of the Hillebrand criticisms in *Psych. Forsch.*, 3, 1923, 370ff.

to the perception of different kinds of movement. In Cermak's experimentation, by change of the variables (frequency of light flashes, speed of rotation of the belt, *etc.*), the *O* sees: (*a*) a single moving object, (*b*) an object broken up into several objects which are moving simultaneously, or (*c*) several objects in stationary succession. The assumption is made that, when the belt is not rotating, *i. e.*, when the speed is reduced to zero, and the distance between the stimuli is zero, we have the limiting case of apparent movement, since the decrease in the distance between the two stimuli has reached the lower limit of spatial-temporal separation.

We may briefly summarize Cermak's results as follows. If we have a speed suitable for optimal movement, an increase of speed destroys it, and gives the perception of several objects, similarly to the simultaneous stage of apparent movement.[26] This phenomenon may be called flicker. This kind of flicker, however, must be distinguished from the flicker arising when one object appears stationary in successive positions. If the speed of the belt is decreased, one object appears at rest in different places, corresponding, in some ways, to Wertheimer's successive stage.

Korte's laws were found to hold, except the third in the case of large light-frequencies. The rates of speed of the rotating belt during the time that optimal movement is still seen are, however, larger than they should be according to Korte's law. According to Korte, when optimal movement appears with any frequency v and any speed of exposure c, a lessening of c should result in succession; in Cermak's experiments, the moving object should appear in various positions without movement. It should do this for any frequency, therefore for every value of v. Cermak found that this is true only for low values of v, and only when the illumination-intervals are long does one see discontinuous succession. If the frequency v is large, then, in spite of the decrease in the value of c, the movement remains optimal. One must therefore decrease the light-frequency a great deal in order to get succession. This result seems to show that succession is due to long intervals between the light-flashes and not to the small spatial distance between the stimuli, a fact which is demanded by the Wertheimer and Korte theories and laws.

To correct this defect, Cermak introduces the "zone" law to take care of all possible cases like the above: when v is large, the range of values of c grows in such a manner that the upper limit remains almost unchanged; v is then the value of the critical fusion-frequency. Further increase of the frequency increases the values of c positively. In this way the transition is made from optimal movement (which gave the most trouble) to light-mixture.

Cermak formulates eight laws, which are known as the parallel laws, applying to light-mixture and apparent movement. They express the dependence of the variables governing phenomena of fusion and apparent movement, and may be stated as follows: (1) in order for fusion and simultaneity to appear the light-intensity must vary inversely with the time of exposure; (2) other things equal, lessening the time of exposure favors the fusion and simultaneous stages; (3) changing the time of dark exposure influences fusion and movement more

[26] In other words, if the movement of an object is too rapid it loses the appearance of movement and a streak of light is seen motionless. The [illegible] multiplication of objects in the moving picture when too rapid rotation results in duplication of the seen object, but all are stationary images. [illegible]

than changing the period of illumination; (4) the greater the time of exposure, just so much the greater must the pause be between the phases if any kind of movement or any degree of fusion is to be seen; (5) making the object smaller favors the simultaneous and fusion stages; (6) central vision favors the appearance of simultaneous movement and fusion; (7) increasing adaptation to dark vision favors the simultaneous movement and fusion, except for very weak intensities; and (8), up to a certain limit, lengthening the period of illumination hinders the fusion and simultaneous phenomena.

These parallel laws, Cermak believes, force us to assume a common set of underlying causes for the phenomena of fusion and of apparent movement, and a theory must be constructed embracing both. Since Lindemann and Hartmann, particularly, have shown that the phenomenal configuration influences both the kind of movement perceived and the limen of fusion, it becomes clear by implication that the facts of movement and of fusion at the same time express certain facts of configurational change. Keeping this conclusion in mind, we may say that the configurational processes underlying movement and fusion are similar, if not identical. In order to justify this inference, Cermak takes over Müller's psychophysical axiom which asserts that identity, similarity, or difference in any phenomenal process corresponds to an identity, similarity, or difference in the psychophysical process.[27] Hence, if we find two phenomenal processes that are alike or obey the same laws, the brain-processes underlying them must be correspondingly alike. The phenomena of light-fusion and movement, we may conclude, arise from a common set of psychophysical processes. And it would seem to be a necessary corollary from the development in theory which we have been tracing that, if fusion and movement are alike and simultaneity and fused quality are similar, then it is highly probable that quality and spatial form may have the same underlying causes if they prove amenable to the same laws. A fact which seems to point in this direction appeared in Hartmann's experiments, where it was found that changing the color of the figures exposed for fusion had practically the same effect as changing the form of the configurations: "verschiedene Farben verhalten sich also bei gleicher geometrischer Reizkonfiguration hinsichtlich des Flimmerns anscheinend ähnlich wie verschiedene optische Raumgestalten."[28] If it is found that the facts of quality can be brought under the laws governing spatial-temporal configurations, thus correlating quality with time and space, then one of the most

[27]G. E. Müller, Zur Psychophysik der Gesichtsempfindungen, *Z. f. Psych.*, 10, 1896, 1-2.

[28]Hartmann, *op. cit.*, 377.

difficult problems of experimental psychology seems to be well on the way toward a solution.

Another investigation into the effect of time upon spatial extents and of both space and time upon the *beta* movement shows a further development of the configurational concepts to include within a given configurational phenomenon several additional variables which may affect the experience. We find in these experiments, conducted by Scholz,[29] that the same laws govern these phenomena in three different sense modalities, vision, hearing and touch. Benussi and Gelb had already found that two spatial extents defined by three lights flashed successively may be made to appear longer or shorter by varying the time interval between the stimuli.[30] The spatial interval between two light flashes seems to shorten when the time between them is decreased. The question arises, what interval between the successive flashes will give the shortest spatial extent. The whole problem is complicated by the fact that phenomenal movement appeared during the optimal time for the maximal shortening. We find thus that movement, extent and time are related, not only in vision, but in hearing and touch as well.

The method used by Scholz was different from that used in the Korte experiments; yet Korte's laws on the whole were verified. Standard intervals were fixed by three lights constantly illuminated. Above the standard lights were the intermittent lights whose flashes defined the spatial extents to be compared with the standard distances. The experiments in hearing consisted in giving two sounds whose sources were separated by varying distances. The *O* was asked to indicate how far apart the two sounds were by showing their positions on a meter stick parallel to the sources. In these experiments the *Os* reported an auditory phi phenomenon. The tactile experiments were made by stimulating the *O* with successive touches at different times and at different places. The *O* pointed to the spot last stimulated; and here also the shorter temporal intervals gave the impression of a smaller spatial extent on the skin. Movement and extent were also closely related in this sense modality.

In all three sense domains it was found that the phenomenal magnitude of a spatial extent depends upon the temporal interval between the successive stimuli. When the real distance between the stimuli is sufficiently large there are temporal intervals in which the magnitude of the phenomenal extents becomes considerably smaller than the normal. The normal extent is defined as the distance between any two stimuli when they are presented simultaneously. Moreover, the optimal temporal interval for the maximum of shortening coincides with the time in which apparent movement appears. It would therefore be safe to say that movement and time and space are mutually related, at least within certain limits; outside these limits we must note certain other effects.

With small separations between the stimuli, shortening of the apparent extents increases with the smaller temporal intervals below the zone in which movement takes place. Below the limen for movement, fusion of the stimuli is reported and only one stationary sound or touch appears, a fact which may be related to the Cermak parallel laws. If the distances are very small, the apparent spatial extents seem greater. In vision this result coincides with the appearance of movement, just as the greater real distances give apparent movement with the maximum of shortening. In

[29] W. Scholz, Experimentelle Untersuchungen über die phänomenale [illegible] Sukzessiv-Darbietung zweier Reize begrenzt werden, *Psych. Forsch.*, 5, [illegible]

[30] V. Benussi, *Psychologie der Zeitauffassung*, 1913, 285 ff., [illegible] den VI. Kongress für experimentelle Psychologie, 19[illegible], 36 ff., 159 ff.

hearing and touch the zone in which the phenomenal extents increase with longer temporal intervals was found to extend into the successive stages of movement. All three stages of Wertheimer's phi phenomenon appeared in these experiments. In the experiments on hearing and touch, the phenomenal spatial extent decreased with the decreased temporal interval to the point where *equal* objective extents appeared phenomenally greater or smaller, if the temporal interval between the stimuli was increased or decreased. Movement accompanied both increases and decreases of extent.

Localization of tactile stimuli is very labile and uncertain, even when the temporal intervals between the stimuli leave no uncertainty as to their position. The order or direction of the stimulations is of importance here because there is a tendency to make errors in the proximal direction. If the order is proximal to distal, the distal spot is displaced more; for owing to the tendency to localize proximally, the distal is affected more than the proximal when the order is proximal to distal, since the spots summate. When the order is distal to proximal, each works against the other and compensation lessens the amount of error.

We have thus traced the development of configurational theory in the field of movement and have found that phenomenal movement can be related to other classes of configurational events and is subject to the same laws. The tendency of configurational experimentation seems to be toward extending configurational principles to the various sense modalities, unifying as many different fields as possible, and reducing apparently diverse phenomena, like movement and fusion, spatial extent and movement, form and quality, to more simple underlying configurational processes.

(13) *Configurational Physiological Theories of Movement*

"The main thesis of the configurational theory may be said to be that physical configurations of the nervous system possess properties parallel to their phenomenal correlates."[31] This quotation from Hartmann shows beyond all peradventure that the configurationists regard as the most important part of their theory that part which forms a bridge between psychology on the one hand and physiology and physics on the other. The considerations leading to this position can be stated briefly. By means of physical and physiological hypotheses the configurationists have been able in many cases to control and predict the phenomena which they are studying. It is almost a truism that we have no laws in psychology that can be called psychological laws pure and simple. The so-called psychological laws, *e. g.*, association, color mixture, apply really to stimuli. There are few theories, either, of the physics or of the physiology of the nervous system which are adequate to account for more than very restricted kinds of psychological process, *e. g.*, the color theories of Young-Helmholtz, Hering, and Ladd-Franklin. Most extant psychological theories, if they possess any value in

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[31]L. Hartmann, *op. cit.*, 391.

explaining the phenomenal processes, furnish no hints as to the processes which accompany them in the nervous system, just as the physiological theories throw no light on the phenomenal pattern.[32]

The aim of the configurationists, in formulating their physiological theories, has been to account for phenomenal patterns by means of configurational processes in the nervous system without any break in theory. If the wholes, patterns, formal aspects of experience cannot be built up out of summative elements, then it becomes necessary to postulate configurational processes within the nervous system to furnish a physiological correlate for the phenomenal data. Moreover, if configurational principles can be applied to the physical processes at the basis of phenomenal and stimulus patterns, the same laws may be formulated for both. Then, by controlling the objective situation according to laws of configurations, we may be in a fair way to control the experiential processes. It is possible to control the objective configuration, by changing spatial-temporal and qualitative dimensions of the stimuli. What goes on in the nervous system must be a matter of conjecture, largely, especially as regards the actual brain-processes involved in perception, thinking and acting. Configurational theories of neural action have therefore been avowedly heuristic.[33] They have been advanced to stimulate further research by furnishing hints for functional variations in conditions. Wherever possible, the configurationists have tried to substitute configurational laws, in terms of objective principles applying to the nervous system, for the subjective factors usually found in psychological theory.

It is necessary to begin with the first configurational hypotheses if we are to understand the rôle that physiological speculation has played in the configurational movement. It must be borne in mind that, in view of the later experimentation, a large number of new facts embracing real movement, apparent movement, colored movement, fusion and the like must all be amenable to whatever theory is finally accepted.

Wertheimer's original theory was designed to explain the facts of apparent movement without resorting to associational-

[32]It will be remembered that the configurationists recognize the problem of the gap between mind and body and endeavor to bridge it by means of the configurational principles, building psychology upon the foundations of physics.

[33]To those who believe that explanation of mental process is to be found [illegible] the nervous system, this part of the configurational programme should strongly appeal. [illegible] the nervous system is a reaction against an extreme phenomenology which has crept into German experimental psychology in the last few years. For [illegible] P. Linke, *Grundfragen der Wahrnehmungslehre*, 1919. *Cf.* Note 39 below.

atomistic physiological assumptions.[34] The brain process corresponding to the movement must be a total, structured configurational event, areal in character. Such processes, Wertheimer assumes, exist, and may be described in the following manner. If a place *a* is excited, a circle of excitation arises spreading from *a*. If two places, *a* and *b*, are excited, the circular spread occurs from both. If *a* is excited before *b*, a short circuit takes place: the spread of excitation crosses from *a* to *b*. The direction of the current is determined by the part first stimulated. The nearer *a* and *b* are to each other, the more favorable are the conditions for the phi phenomenon.

The successive optimal and simultaneous stages of movement are explained as follows. (*a*) Successive movements of two lines presented stroboscopically occur when the time between the exposure of the one, *a*, and the other, *b*, is so great that the circular activity of *a* is extinguished before *b* is stimulated, and hence *a* moves and stops before *b* appears and moves. (This explanation does not account for dual part movement, when both stimuli move at once alone.) (*b*) Optimal movement (*a* moves over to *b*) occurs when the time between *a* and *b* is just long enough for the excitation from *b* to meet the excitation from *a* at its height. (*c*) The simultaneous stage (both *a* and *b* appear simultaneously at rest) occurs when the time is so short between *a* and *b* that the circular excitations take place immediately and do not reach a sufficient height to make possible a short-circuit between the two.

It is also assumed that no part of the excitations in the brain reaches consciousness before the total does. The single stimuli must give rise to excitations which "spread" before becoming conscious,—a point which Köhler later tries to explain. But it is not clear whether *a* and *b* act as isolated stimuli in the successive stage, where each moves alone. As a concrete illustration will show, the influence of the single stimuli is left in doubt.[35] Let us take the perception of a triangle whose sides are *a*, *b* and *c*. According to the theory outlined above, since all the parts are symmetrical, they must influence each other in equal amounts. In the case of a patient who was unable to see a triangle and was obliged to judge its form by passing his fingers around the contour, time was needed to travel from one side to the other, as in Wertheimer's successive case. If *A* represents the circle of excitation in the brain of side *a*, *B* of *b*, and *C* of *c*, then *A* must influence *B*, *B* must influence *C*, and *A* must influence *C* equally,—*i. e.*, *AB* must be equal to *BC*, *CB* must be equal to *CA* and so on. In other words, the relations between *A* and *C* must have the same effect in the total picture as *A* and *B*, and *B* and *C*. But this result is precluded by the temporal inequalities; by the time the patient's hand has reached side *c*, the excitation initiated by *a* has less influence on it than *b*. The Wertheimer theory implies that symmetrical spatial configurations are perceived when their parts have proportional excitational spreads. In the case cited, the time interval made no difference, and the relations of the sides to one another were not mutually reciprocal; yet a symmetrical figure was perceived. The theory encounters difficulties also in assuming that discrete stimuli may give rise to unitary structures. Nor can one assert that the neural configuration must be completed before the phenomenal configuration appears, because the parts may be perceived as

[34] M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Z. f. Psych.*, 61, 1912.

[35] Taken from K. Gneisse, Die Enstehung der Gestaltvorstellungen, *etc.*, *Arch. f. d. ges. Psych.*, 41, 1921, 295-334.

such and yet be recognized as belonging to a single configuration. On the other hand, if the parts become conscious before the brain configuration is completed, the theory falls; for it demands that the consciousness of the whole is possible only when the various single excitations meet in a unitary structure.

Further elaboration of the Wertheimer theory has been undertaken by Koffka. The relations among the central processes responsible for the various kinds of movement are not given by Korte's laws. Koffka asks where the two excitations from *a* to *b* meet, when they meet, and how strong each is at the place and time of meeting.[36] By geometrizing the central neural processes arising as a result of the stimulus excitations, Koffka arrives at a mathematical formulation of the laws of Korte based on the Wertheimer theory. Here again the case of successive movements of each stimulus offers difficulties; for here the one stimulus may take effect without the other, and discrete processes are the correlates of the movements, whereas in optimal movement it was demanded that the effect of the stimuli be regarded as "total, unitary processes." On the other hand, the fact that the *second* stimulus may be of influence upon the first requires a more elaborate theory than the simple hypothesis advanced by Wertheimer.

In order to explain how discrete stimuli can influence each other and how they can give rise to the various phenomena of movement and fusion Hartmann and Köhler have amplified the original Wertheimer theories.[37] Since Köhler's views are a restatement of the Hartmann theory, let us consider Köhler's theory.

Wertheimer found that, even though the stimuli *a* and *b* were colored, it was possible to get movement in which no trace of color was visible. But he also found that his *O*s obtained colored movement. Köhler asks: "How do two stimuli, *a* and *b*, give one movement, and how can two stimuli give colored movement at one time and at other times movement which lacks all traces of color (pure phi)?" Recognizing that discrete stimuli are a source of difficulty, and that the short-circuit theory needs revision, Köhler assumes that the optic sector reacts as a whole to the whole constellation of physical stimuli. The incoming excitation currents are functions of the total situation and can be described in physiological terms as follows: if *A* is the central nervous process corresponding to stimulus *a*, and *B* that corresponding to *b*, when *a* and *b* are presented either simultaneously or near one another, *A* and *B* are different from what they would be if each occurred alone. Therefore *A* is determined not only by *a* but also by *B* within the central nervous system. In this way illusions can be explained which are due to *Nebenreize.*

The way in which *A* and *B* influence each other finds a different explanation from that which Wertheimer had employed. Köhler asserts that the stimuli *a* and *b* set up currents in the [illegible] *A* and *B* fuse into one

[36] K. Koffka, Zur Theorie einfachster gesehener Bewegungen, in *Beiträge z. Psychol. d. Gestalt*, 1919, 283-318.

[37] L. Hartmann, *op. cit.*, 391 ff.; [illegible] Z. [illegible] Bewegung, *Psych. Forsch.*, 3, 1923, 397-406.

beneath the psychophysical level, so that the conscious process arises as a unitary structure. In this way Köhler meets the objections raised above against the Wertheimer theory. Both currents tend toward an equilibrium. Korte's results confirm this theory better than the Wertheimer theory; for he found it possible for a later stimulus to be perceived as earlier in time, if it possessed a greater intensity than the first.

We may call this a theory of "sub-psychophysical absorption" to account for the various stages of movement and fusion. The part, successive and simultaneous stages are easily accounted for in the following manner: if *A* and *B* do not fuse, then part movement of each is seen; if *A* and *B* appear too closely together, both are seen at rest.

To account for the presence and absence of color in movement Köhler has recourse to his earlier theory of color which found expression in *Physische Gestalten.* In order for color to appear in consciousness, it is necessary for unequal amounts of ionic concentrations to pile up in various parts of the optic sector. If this damming process does not occur, color will not appear. Hence time is necessary for the perception of color. If the displacement of ionic concentrations is not too fast, color and movement will both be perceived. If the concentrations do not reach a sufficient strength, no color will be seen and a pure movement will be the result.

The theory, thinks Köhler, explains the physiological bases of movement in the various sense domains. If two pressure sensations are felt together, they seem to be nearer than if separated by a greater interval of time. If a strong and a weak stimulus are presented, the stronger will tend to absorb the weaker. Some of Benussi's results seem to harmonize with this theory; for it was found that tactile *beta* movement seemed to grow smaller when the stimuli were moved together in time until only one sensation was felt. In the domain of vision it was found that, the shorter the time of exposure between any two lights, the nearer they seemed to be in space. The same thing holds for sound.[38,39]

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[38]Benussi found the phi phenomenon on the skin, getting movement not only on one arm but from one arm to the other through the air. Lack of space forbids a discussion of his results. His work on tactile phi is contained in several articles, a summary of which will be found in Versuche zur Analyse taktil erweckter Scheinbewegungen, *etc.*, *Arch. f. d. ges. Psych.*, 36, 1917, 59-136. *Cf.* Scholz' work, discussed in the preceding Section.

[39]Linke, *op. cit.*, has criticized the configurational theories of movement on the ground that they do not explain the perception of movement by reference to purely psychological principles. He asserts that we must find an explanation of movement within the phenomenal facts themselves, since an appeal to the nervous system throws no light on the experience of movement. Linke's theory of movement does not take account, therefore, of the stimuli, the nervous system, or the origin and changes of movement due to physical or neurological factors. His position is purely phenomenological, and shows the psychological influence of Husserl at its most extreme. Linke regards movement as a function of "assimilative perception," which is not to be confused with Wundt's concept of assimilation. When

(14) *Formal Spatial Effects Due to Configurational Properties*

We have already seen that different phenomenal configurations possess different phenomenal properties by which they may be distinguished, *e. g.*, such general differences in clearness, persistence, "thingliness", and impressiveness as are denoted by the concept of figure-ground. While it is difficult to draw a sharp line between descriptive and functional concepts, we may regard the figure-ground properties as descriptive, while the effects of the configuration upon the perception of movement, the limen of fusion, and the like, are functional. But the configuration may exert a functional effect upon phenomena which is difficult to characterize as either static or dynamic *per se*. Among these borderline phenomena may be reckoned stereoscopic vision, after-images, the perception of depth and of convex and concave objects, and many of the facts we shall discuss in following Sections. In this Section we shall be limited to the effects of configurations as they appear in the perception of space and in allied experiences.

(*a*) *Stereoscopic Vision*

Lau, who has attacked the problem of stereoscopic vision and depth from the configurational point of view, is concerned to prove that stereoscopic vision depends upon more than a simple functional interplay of retinal elements.[40] Lau showed his *Os* 6 parallel lines in the one field of the stereoscope and the Zöllner figure in the other. Lau asserts that, if the peripheral, atomistic explanations of stereoscopic vision are correct, we should expect the parallel lines to lie in the same plane with the Zöllner figure as when offered alone; for nothing has changed, according to the element-theories, in the parallel-line image on the retina. But if the images from the two eyes form a single configuration, and if, as we should expect, each field is a different configuration, then the parallel lines must appear in a more complicated spatial arrangement when offered with the Zöllner figure. Observation proves that the parallel lines appear to be in a different plane from that in which the criss-cross lines are seen.

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only part of an experience is given in perception, and this part has the compelling power to re-instate or complete it, then assimilative perception is present. Pre-perceived elements enter into the given perception and complete the sensory data. Movement consists of a succession of phase-images or a plurality of place-determinations. To be the phases of one movement means to be parts of one and the same object. What moves is the thing which is identified in the various phases. Linke regards this explanation as a true descriptive explanation, without an appeal to the stimuli or the nervous system. But a single instance is sufficient to show the weakness of the theory: Gelb and Goldstein found a patient who was able to see a moving object only in successive places, each phase being stationary. This patient knew that the phases belonged to a single object, and identified them as the same, yet he saw no movement. How can this observation be explained on the basis of Linke's phenomenological theory?

[illegible] *Fälle auf Grund von Untersuchungen Hirnverletzter*, 1920, 96.

[40] E. Lau, Versuche über das stereoskopische Sehen, *Psych. Forsch.*, 2, [illegible] Ueber das stereoskopische Sehen, *ibid.*, 6, 192[illegible], 121-126.

(*b*) *Depth, Rivalry and Related Phenomena*

Experiments in which the Zöllner lines crossed the parallels in both fields, except that the lines extended in opposite directions for each set of parallels, showed that rivalry set in: the parallel lines appeared in the same plane while the Zöllner lines appeared in different planes, depending upon which image dominated. The illusion was also increased. Köhler, in preliminary experiments with the Müller-Lyer figure, reported negative results, and Lau found that this figure was destroyed when he tried the experiments described above. In a later paper,[41] Lau has elaborated a configurational theory of depth perception on the basis of further experimentation. This theory embraces the facts just cited. Lau declares that it is not merely the disparate images which are responsible for depth; depth, he believes, arises as a result of the deviations between configurations; retinal points do not act in isolated fashion, but the configurations are effective as total structures. Qualitative considerations also are involved in the perception of depth; for a white line on a black ground, given as one field, does not fuse with a white surface, given as the other field, even though identical points are stimulated. In stereoscopic vision there is an "equalization" (*Vergleichung*) of structures, and this process may act reflexively upon the structure of each retinal image. Thus monocular structures can be destroyed when the configurations offered to each eye are too unlike: the different parts of each image come into rivalry, with the result that similars fuse and not a trace of the original remains; or parts of monocular structures may be unified into a single configuration, and each configuration may be put in a different plane, as was shown above.

The phenomena of rivalry in connection with the theory of identical and corresponding points have been further investigated by others whose results support Lau's configurational theories. Let us take first Lewin and Sakuma's experiments on movement and depth.[42] These *E*s presented two cards with figures on each, one to each eye, in the Hering haploscope, and moved either all or parts of the figures. The figures were such that both eyes might have images in common (binocular images) or one figure might contain parts not present in the other (monocular image). With movement that is too fast, double images result; with the right speed (moving only one image, usually the left), the mono-image hurries on in advance of the rest of the configuration which both eyes have in common. The monocular and binocular images give different quantitative results when measured with respect to movement, rate of movement, and the like.

It is interesting to note that real movement of part of a configuration presented to one eye could cause phenomenal movement or distortion of parts of the objectively stationary binocular structures. Thus if two vertical lines, composed of three black dots separated by a small interval, were presented to both eyes, and if the top dot of the left field was moved to the right, the result was that (instead of a vertical line with only the top dot displaced to the right) the middle dot moved half as far as the top dot (only phenomenally, of course), and the resulting figure was an oblique line whose direction was to the right, the bottom dot forming the base of the oblique line without having stirred from its position.

More important still is the fact that disparate images may give single structures, while images on corresponding or identical points may give double structures. The less deformable a figure is, the easier may it double and give rise to double images.[43] If two triangles having a common base

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[41]Lau, *op. cit.*

[42]K. Lewin and K. Sakuma, Die Sehrichtung monokularer und binokularer Objekte, *etc.*, *Psych. Forsch.*, 6, 1924, 298ff.

[43]H. Hildebrandt has reported on the changes in configurational structures in astigmatic vision. See Experimentelle Untersuchungen über das Sehen bei nicht-optimaler Akkommodation, *Psych. Forsch.*, 6, 1924, 112ff.

and apexes pointing in different directions are presented, they are on identical points of both retinas and yet are seen as two. *Retinal points can thus be regarded rather as retinal zones which will suffer a certain amount of displacement before double images appear.* It was found that, to prevent accommodation to abnormal convergence, a displacement of retinal zones did occur. When one of the images or a part of it is moved, "simple" vision is impossible unless there is some distortion of retinal ordinates; since we are dealing with labile systems, not with absolute points, it is possible to preserve a unitary phenomenal structure when non-corresponding points of the retina are stimulated.

The question now arises concerning depth: if identical points on the retina are fixed, why does not depth arise when the left arm of the haploscope is moved? Again, since the binocular images are not on identical points (for we must assume in the cases cited above that there are disparate images on the retinas), why does the *O* not get depth relatively to the monocular points? There is some depth effect, but it is minimal. Lewin and Sakuma advance a provisional theory of monocular depth, according to which depth arises when the retinal zones in one and the same eye undergo different and opposite strains (*Durchbiegungen*). Or, in other words, a relative depth effect appears when, in the transition from a stable motor system, the strains or tensions do not disappear. It is admitted that other factors like intensity and eye-movements are of importance; but the essential point of this configurational theory lies in its insistence that the changes within a single eye depend upon the deviations between the retinal images of both eyes! Each retinal image is affected by the image on the other retina.

Closely related to the work of Lewin and Sakuma is Kuroda's investigation of binocular phenomena.[44] When the eyes are stimulated by different light-rays any one of three things may happen: one image may dominate, rivalry may set in, or there is a fusion of both fields. The quantitative and qualitative differences in the stimuli determine what shall happen in the retina. So far as quality is concerned two factors are of importance: the intensity of the stimuli, and the amount of similarity existing between them. Thus contours possess greater dominating powers than their fields; but, if they are broadened, they lose their significance and also their dominating qualities. Different forms possess different grades of domination. Different colors possess different degrees of dominating power; yellow more than blue, white more than black, red more than blue, and so on. The greater the difference between the two stimuli, the greater the domination of the stronger.

If figures are alike, simple vision is assured. On the borderline stand figures which are unlike but similar in quality. Mixture may take place between white and black, for example, when the brightness difference is reduced. Domination and mixture are inversely related. Other variables which may affect binocular vision are brightness, saturation and the size of the single fields. In general, various amounts of these factors affect mixture, as other investigators have already sufficiently demonstrated.

Kuroda's conclusions are that domination and rivalry appear only when there is a sufficient amount of difference between the stimuli. But rivalry appears when the difference between the stimuli is not too great; rivalry is a half-way stage between mixture and domination. In reality, rivalry is *alternate* domination, and is related to domination in that it requires a low degree of difference. Rivalry is also related to mixture in requiring [illegible]

[44] C. Kuroda, Zur Grenzbestimmung der binokularen Phänomene, *Psych. Forsch.*, 6, 1924, 282-297.

(c) *The Perception of Two Colors, the One Behind the Other*

Kuroda does not mention a point, which might well have come up in his experiments, concerning the fact of the simultaneous appearance of two colors, the one behind the other. This has been a moot question since Helmholtz gave an affirmative answer and Hering denied that it is possible to perceive two colors simultaneously, the one behind the other. Stated more precisely, the point at issue is whether identical retinal spots in monocular vision can mediate the perception of two different colors at the same time and result in the perception of depth. The problem has been investigated by Fuchs, who found that different colors may appear simultaneously in different planes behind each other. He says that "the conditions for the appearance of this phenomenon are the presence of suitable configurations in configurational perceptions. The main condition is that both objects, the transparent as well as the object seen behind it, must be perceived as two different, complete configurations."[45]

Fuchs used a piece of glass tilted at a suitable angle with the one color in front and the other behind, so that the *O* saw the color in front reflected from the side of the glass toward him, while the other color was perceived through the glass. Either color may vary in size. While it is difficult to perceive one color behind the other, Fuchs was able to find enough positive *Os* to get very interesting results. It is more difficult in monocular than in binocular vision. If one color is regarded for itself, there will be no transparency, no depth; for either a single color will appear which is a mixture of the two colors, or else only the color fixated. If single points on either colored surface are fixated, either one color or a mixture is seen. The same thing is true if the contours are fixated. If the figures are regarded as wholes, one can attend to single points or contours and still get depth, but in general point-fixation destroys depth.[46]

It is not a question, Fuchs tells us, of noticing now one color and now another; both are seen simultaneously, in different planes. Fuchs concludes that the perception of depth is an immediate experience, requiring no more mediation than the perception of right, left, up and down. In agreement with Lewin and Sakuma's later work, quoted above, it is possible in monocular vision to get depth and also two colors in different planes. It is thus not even necessary to stimulate different retinal spots in a single eye in order to perceive depth monocularly.

If one observes with one eye and then uncovers the other, the farther color shows a displacement backwards. Eye movements and head movements increase the transparency of the color seen in front and the clearness of the color behind. The same effect can be achieved by moving the colored objects. The explanation advanced for the effects of movement is that movement helps to bring out the form of each configuration and thus accentuates the color of each. It is possible to invert the colors, so that what was seen before may now be made to appear behind the other. The *Os* reported that the depth factor is increased when the stimuli are regarded as objects and the sensations of color are neglected. Colored gelatine or smoked glass as the reflecting medium destroys the perception, because the surfaces cannot be surveyed and regarded as structures *per se*.

With yellow and blue circles, the yellow in front of the blue, if the yellow is subjectively localized in the same plane as the blue, it will appear

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[45]W. Fuchs, Experimentelle Untersuchungen über das simultane Hintereinandersehen auf derselben Sehrichtung, *Z. f. Psych.*, 91, 1922, 146ff. Many of the facts described by Fuchs from the configurational point of view have received attention from members of other schools. Katz must be mentioned in this connection, as his book on color vision has been a source of information to Fuchs. See Note 6 for a reference to Katz' work.

[46]Fuchs, *ibid.*, 161. I omit discussion of the experiments with *Spiegelbilder*.

whiter or grayer. If it is localized behind the blue, it looks more yellowish than when seen in the same plane, but less yellow than when localized in front of the blue. When the yellow is objectively behind the blue, if it is localized in front of it, it appears more compact, a better yellow and more "thingly," than when seen where it really is. Fuchs concludes that yellow cannot compete with blue as a color because it lacks chroma, compactness and "thingliness" as compared with blue. Yet, as we saw above, Kuroda found, at least in the case of one observer, that yellow dominated in the stereoscope when presented with blue.[47]

Experiments with different stimulation of identical retinal spots also resulted in the appearance of one object lying behind another. The double image method was here used: a colored surface and then an object were presented to one eye, and it seemed as if one were looking at the surface behind the color of the object.

The influence of the configuration upon the colors may be summed up, according to Fuchs, in this way: the form of the contour is important in determining whether the colors will be seen as wholes, or on a background, or in parts; the configuration of the surface may be so conditioned by the contour that it is less unified than it might otherwise appear. Parts of the surface may disappear altogether or appear as parts of another configuration; the configuration may be such that a spot will raise itself above the rest of the surface, appearing as a whole in its own right, while the rest of the configuration forms the background or surroundings for this independent bit.

Shadows played some part in the experiments; they deserve mention, although Katz has described similar phenomena in his work on colors. The colors in Fuchs' experiments often looked shaded. But differences in the shading were noted; there are opaque and transparent shadows. When the shadow is opaque, it is in front of (or on) the colored surface, but does not form part of the configuration. When the shadow is transparent, the color of the surface can, of course, still be seen behind it. The transparent shadow seems to be the limiting case of double color perception,—the distinction between lighting and that which is lighted (*von Beleuchtung und Beleuchtetem*) seems to be the last stage in separating colors.

All of Fuchs' *O*s with the exception of one (and at times another) agree that the phenomena described consist of an immediate experience of two colors behind one another and in the same direction. They deny that the perception is a matter of meaning, since it is constituted by an immediate sensational content *sui generis*. It is not a question—*contra* the views of the members of the school of the form-quality—of something new which results in a new content added to an already familiar experience. The essential conditions of these phenomena, Fuchs believes, reside in the configurational attitude or perception.

It is difficult to draw any general conclusions or to trace any special development in configurational theory from the work on binocular vision and the perception of depth. There does seem to have been an attempt made to determine what occurs inside the configuration under the varying conditions. In the work of Lewin and Sakuma a theory is advanced to account for the

[47] There are also differences of a more important kind among *O*s, as Fuchs [illegible] *O*s were unable to see two colors simultaneously, the one behind the other. [illegible] as the "analytical" *O*s, since a configurational or totalizing attitude was necessary to get the phenomenon. Similar results with analytic *O*s, where configurational phenomena are in question, [illegible] Eberhardt, Benussi and others.

perception of depth in monocular vision. Fuchs had left his facts in the air, after ascribing them to an "immediate awareness". The later theories of the configurationists represent an attempt to discover the mechanisms, physical and physiological, which are responsible for the phenomenological data they have described. It may help us to understand these attempts better if we summarize briefly the configurationists' general position with regard to space perception.

(15) *Configurational Theories of Space Perception*

The configurationists reject both the nativistic and the genetic accounts of the perception of depth, localization, orientation and direction in space. Phenomenologically, space-perception is an immediate experience of certain properties possessed by objects in the external world. Genetic theories, which attempt to derive space from elementary sensations which either do not possess spatial characters at all or are endowed with "local signs", err in assuming that mind builds space out of non-spatial elements or that "experience" can form spatial configurations by summative processes. The nativistic theories are attacked because they neglect the objective conditions under which the perception of space is made possible; for even though configurations are immediate data of experience, they arise only when conditions favor structuration. Phenomenal space copies real space, often in spite of the anisotropy of the retina and optic sector; the deviations between phenomenal and real structures must be explained, in so far as we are able, in terms of the physics and chemistry of the nervous system. We must also take into consideration whatever subjective factors we find of influence in any given situation. Thus Lau rejects explanations of depth which are based upon the converging and diverging functions of the eyes, because recourse to them is explaining one function only by means of another.

The first thing of importance in the configurational theories of spatial perception is that there are no elementary experiences lacking spatial properties; rather, objects emerge from a general level, much as figure and ground behave. The general spatial level is a firm structure which resists change.[48] The structure contains "anchorage points" which are the points of reference of our spatial structures. This general spatial level, or frame of reference, remains fixed in spite of eye movements, head movements and even movements of the body. We have seen how eye movements and hand movements take place in order to sharpen or modify existing spatial configurations in the di-

[48]What follows is a general configurational theory of space perception, largely on the basis of Wertheimer's theory as laid down in his 1912 paper, 253ff.

rection of better configurations, and how they may change old configurations into new configurations. But normally objects maintain their positions quite independently of the changes which the *O* makes. It is possible, with sufficiently powerful stimuli or by unusual conditions, to loosen these points of reference. Wertheimer gives an illustration which shows how a frame of reference may be destroyed.

If an after-image of the phi-phenomenon is projected on a table or chair, two possibilities are open: (1) the after-image appears to move upwards or downwards and the object on which it is projected remains stationary, in which case the ordinary frame of reference has not yet been disturbed; (2) the object on which the after-image is projected and the whole field about it seem to join in the movement of the after-image; there is movement either to the right or left, or up or down, but no stationary objects remain as fixed points of reference. The whole spatial frame of reference has now become instable and spatial perception is altered accordingly.

The configurational account thus far given does not imply that spatial perception is entirely relative, or subjective, or optional. Even though the anchorage points are loosened and ordinary configurations are so deformed or changed that new spatial structures arise, there still remain definite objective and subjective (in the sense of within-the-organism) conditions which determine the new spatial changes.

When points of anchorage are gone, says Wertheimer, localization, direction and distance judgments become erroneous. The perceptions of depth, position, apparent size and a multitude of other spatial phenomena are bound up with configurational structures. Spatial changes concern changes within the general level in which we normally find ourselves. Changes may take place within the level, which then remains firm, or they may concern the level itself. The experiments of Lewin and Sakuma show that, if a part of a configuration is moved, it may modify the configuration to which it belongs. This would necessitate a change within the level; but if the movement is carried too far it will result in double images, the possibility of accommodating the existing configuration to the change having gone, and two separate structures arise.

In close agreement with the Wertheimer theory of space are the results of von Hornbostel's experiments on the optical inversion of objects.[49] If a cube constructed out of wire is held before a mirror so that the mirror-image can be seen through the wire model, it is possible, with practice, to invert either the mirror cube or the felt cube. The result is that, if the optical cube is inverted and the tactile cube is not, the *O* finds that the cube looks like one object and feels like another! Which is the more real, the invisible but felt object, or the impalpable but seen object? Von Hornbostel's answer is that it will be the object which is made or kept convex, *i. e.*, the object which protrudes from a background. Rubin had ascribed more "thing" character to the figure than to the ground from which it protrudes,[50] so that we should expect the figure or protruding object to be the more real. The importance of these facts for the configurational theory [illegible] the object on this

[49]E. M. von Hornbostel, Ueber optische Inversion, *Psych. Forsch.*, [illegible], 1921, 130-150.

[50]E. Rubin, *op. cit.*, and Section 11, above.

case the visual object) which protrudes from a background (convexly) is an object in a definite space, whereas the felt object is not yet localized in tactile space. In merely holding it (since we are already visually-spatially minded), our tactile space-structure is not sufficiently impressive to offset the visual configuration. The tactile space becomes structured only when one moves in it, with the result that one part lifts itself from the rest,—in other words, when one establishes in touch the conditions for a figure-ground effect. If the *O* moves his fingers about the cube, it may become more real and "thingly" than the inverted mirror cube, which, in comparison with the tactile structure, has now lost its firmness. It is a matter, then, of which configuration will predominate. Often there may be rivalry between the tactile and visual spatial structures.

The inversions which he has described cannot, von Hornbostel says, be explained as illusions of perception or as the results of ideational activity on the part of the *O*. They are experienced under certain conditions which are explicable on psychological grounds, *viz.*, by changing the right-left, up-down and forward-backward anchorage or reference points of our ordinary spatial structures. Such factors as association, eye-movements and the assumption of a special psychical activity, do not explain the facts. The explanation must be in terms of the conditions which govern spatial configurations.[51]

Later configurational experimentation has borne out the original Wertheimer spatial theory. Space perception requires frames of reference in which our experiences are ordered. These frames or levels may be distorted, altered, or destroyed. Since configurations tend to preserve their form, we should expect resistance to alterations and distortions, and the appearance of new spatial structures only under special conditions. The effects of these special conditions on spatial configurations have yet to be worked out in greater detail and to be formulated in terms of general laws of space perception.

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[51]V. Benussi, writing in 1911, advanced some configurational principles in explanation of reversible figures; he cannot, however, be said to have anticipated the configurationists, because in his theories associative connections still play a large part, whereas a true configurational account makes no use of associational concepts. *Cf.* Ueber die Motive der Scheinkörperlichkeit bei umkehrbaren Zeichnungen, *Arch f. d. ges. Psych.*, 20, 1911, 363-496.

THE RÔLE OF EMOTION IN A SYNAESTHETIC SUBJECT

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The work of Wheeler and Cutsforth on synaesthesia has apparently tapped so many problems, not only in synaesthesia, but in general psychology as well, that it seems expedient to present certain details of another case. This case has been selected especially for the light it throws upon the content of emotion and the relationship between the so-called feeling and cognitive processes.

It will be remembered that synaesthesia as described in earlier articles[1] is unquestionably a process of perceiving. If this is true, it seemed to the writer that a study of synaesthetic processes in the field of emotion would prove enlightening; for it was suspected that the conventional distinction between feeling and cognition failed to hold in synaesthetic *S*s. Neither the view that feelings evaluate while perception furnishes knowledge, nor the view that feeling and cognition are two aspects of the same consciousness, seemed valid in face of our preliminary investigations. Moreover, our results of more recent date brought us in touch with the problem of the stimulus error, and unquestionably threw this problem into line with our discovery of the "parent process." Meanwhile, we were still concerned with the problem of meaning.

We shall here attempt to present the facts of a particular case. It will be observed, no doubt, that our use of words presenting the reports of our *S* does not harmonize in many respects with their ordinary psychological usage. The reasons for this departure should become apparent as we proceed.

The *S* for this investigation was Miss *E*, a graduate student in the University of Oregon, a 'major' in education, but well trained in psychological procedure. Our methods in this experimentation were so varied that they will be presented in connection with results throughout the paper.

When about 9 years of age *E* became aware of her synaesthetic processes, but of course did not recognize them as such. Recognition came about

[1] R. H. Wheeler, Visual phenomena in the dreams of a blind subject, *Psychol. Rev.*, 27, 1920, 313-322; The synaesthesia of a blind subject, *Univ. of Ore. Publs.*, 1, No. 5, 1920, 61pp.; Wheeler and Cutsforth, The synaes[illegible] blind subject, *ibid.*, 1, No. 10, 1922, 104pp.; The rôle of [illegible] learning, *Jour. Exper. Psychol.*, 4, 1921, 448-468; Synaesthesia and meaning, this Journal, 33, 1922, 361-384; Synaesthesia, a form of perception, *Psychol. Rev.*, 29, 1922, 212-220; Cutsforth, The rôle of [illegible] reasoning, this Journal 35, 1924, 88-97.

through a chance discussion with a group of older girls concerning the nature of piano tones. Soon she discovered that her thinking differed markedly from that of other girls. Until she reached her senior year in college and began to act as *S* in the psychological laboratory she suffered from the misapprehension that her mental life was abnormal. Throughout this long period she made repeated efforts to avoid the use of colors in her sensory and emotional experiences; but the attempt to eradicate the visual processes was a failure, and the only result was a modification in the behavior of the imagery and a possible reduction in her mental efficiency. *E* has now been under observation for 18 months.

A typical childhood memory. Our procedure here was simple. *E* was asked to recall several of her earliest memories and then to give a complete introspective account of her mental processes. Space will not permit a publication of detailed introspective data.

In summary, *E*'s childhood experiences are recalled in terms of concrete visual and synaesthetic visual imagery. The concrete imagery appears in fragmentary form, visualized through a washed-out film of synaesthetic coloring which has been retained as the content of an emotional experience. For example, *E* recalls that when about 5 years of age she was in the middle of a fully-bloomed clover field. The red and green of the clover appear to her now in quite definite visual imagery; projected through and upon the concrete visual imagery is a gauzy film of bright rose. This, she reports, is the synaesthetic color of joy which she experienced at the time. The rose-color is not directly associated with the clover blossoms, for it is of different tint and has the full significance of an emotional experience. In fact, the rose-synaesthesia is the reduced or mechanized content of the emotion itself. In recalled, as in immediate emotions, details such as intensity, affective quality and interpreted meaning consist exclusively of synaesthetic coloration, from the standpoint of sensory content.

E's stereotyped image-forms. *E* makes constant use of schematized forms in computing numbers, in reckoning the days of the week and months of the year, also in recalling the alphabet. These forms are rich in emotional coloration, traceable to the affective color-reactions of early childhood. This coloration assumes in part the concrete hue, shape, position and behavior of early visual associations. In addition to the types of imagery just mentioned, *E* makes use of a third, which is functionally closely related to the others, but apparently derives its origin from an entirely different sense modality. Imagery of a synaesthetic nature possessing color and carrying a strong emotional meaning is described as having "perceptual tangibility" with an unmistakable reference to the body.

For example, *E*'s number form consists of a vertical row of figures from one on up indefinitely. The figures appear in pencil upon a manila-colored background. Her alphabet form con-

sists of letters visualized along a horizontal line extending from left to right. The first 7 letters are identified with the 7 notes of a tonal octave. The imagery employed in these two forms is of an entirely different concrete nature, which has been derived from a direct visual stimulus. If there was ever any emotional synaesthesis connected with these forms, it has disappeared. In view of the facts, first, that the majority of *E*'s experiences are emotionally colored in a literal sense of the term, and secondly, that she uses these two forms constantly, we have every reason to believe that the lack of emotional coloring in these two cases is evidence of a very high degree of attenuation or mechanization. It is doubtful whether further simplification will take place, for everything contained in these two forms seems necessary for their use.

On the other hand, the coloration for days of the week and months of the year includes the curious tactual factor.

Sunday is a bright golden yellow—pleasing. Monday is a dull red with an intermingling of brown. Tuesday is a bluish-grey, sky-color like diluted copper-sulphate solution. Wednesday is a "soft" shade of brown like that of high-grade chocolate. Thursday is colored similar to Tuesday, but is more highly saturated. Friday consists of shades of red, blue and yellow. The colors are opaque and pigmented. Saturday is brown, like allspice, with a strong tint of yellow.

Sunday and Friday stand out in the form as being more emotional than the other days of the week. They are more highly colored, both in degree of saturation and in brightness. In other words, they represent more emotional experience than do the other and less colorful days. Perhaps they owe their imaginal coloration to some childhood experience which determined the synaesthetic processes, or the emotional value may have been added to the original synaesthesia, subsequently. However, we are compelled to favor the former view, because of the large number of instances in which the emotion determined the nature of the synaesthetic sensation and imagery, regardless of the nature of the stimulus.

The month form is a disc which is projected in space at a convenient reading distance. The months lie about the rim of the disc at regularly spaced intervals, one color merging into its neighbors. Beginning with January the months run counter-clockwise, with the new year falling at the position of half-past four. Thus, each month occupies about 30° of the disc.

January is made up of a "tactually compressible" dusty brown splotch, [illegible] flecked with white. it is utterly intangible, like vapor, and very pleasant. [illegible] brown like January, but less saturated. Toward the end it becomes [illegible] [illegible]. The month possesses an internal whirling movement, also a slight degree of tangibility; mildly unpleasant. [illegible] blues and purples, with the clear quality of spectral colors; pleasant. May has a [illegible] background, upon which are splotched bluish purple, whitish-

pink, red, and a very light yellow. The opalescent intangibility of April has nearly vanished; pleasant. June is a continuation of May, but lighter in color, with an addition of red and blue. The month is tangible. Strong mixed emotions. July is similar to June, with the exception that it is covered with a film of grimy, dirty brown. This color as well as the concept of July possesses a reference to bodily discomfort; strongly unpleasant. August is a continuation of July, with a marked increase in the July coloration and an increase in the bodily discomfort; strongly unpleasant. September is a background of dirty brown, splashed over with yellow and green; slightly unpleasant. October is a dark grey background, partially filled with shades of orange, yellow and red. The month possesses neither tangibility nor etherial opalescence; nominally neutral. November is a background of yellow, modified by the colors of October, less saturated; pleasant. December is predominantly a dark grey with a little blue, more white, and a small amount of intense blue-black; is in a magazine picture of midnight; pleasant.

In general our *S*'s month form is similar to those possessed by other synaesthetic individuals. It functions as a concept of a year's time, when perceived as a whole, and as different seasons when perceived in divisions. The segmented parts of the disc mean individual months. The individuality of the months depends upon their characteristic color and upon their proper place on the year-dial. Two additional factors enter into this form. First, the process of perceiving a synaesthetically colored month as an emotion,—by which we mean that the emotional response is represented in the various qualities of the colored imagery. Secondly, the process of perceiving a tactual tangibility or compressibility in the synaesthetic imagery. This is not an experience of that empathic variety in which a visual image appears as if it contained cutaneous sensations. Rather, the visual imagery means a tactual experience. It is, for *E*, tactual sensation. On the other hand, opalescent and etherial colors lack this tactual increment, and the experience as a whole contains the interpretative process that tangibility is impossible. This interpretation is traceable to the attentional emphasis upon the opalescent and etherial nature of the imagery.

The tactual and emotional elements are very closely related in that one color quality is common to them both. That is, the same appearance of the color-image that means opaqueness (visual signification) and tangibility (tactual signification) also means unpleasantness. Conversely, that appearance of the color-image perceived as opalescence (visual signification) and intangibility also means or functions as pleasantness. For example, the unpleasant months of January, July and August are tactually tangible, while the pleasant months (colors) of February, April, May and December lack the positive tactual quality and appear as intangible voids filled with nothing but opalescent coloring.

It is noticeable that the colors of green and dirty brown are generally included in the images of unpleasantness. Pleasant-

ness is included in the ephemeral colors of blue, red, and yellow. The less opaque and pigmented colors tend toward a neutral state, and give evidence of the same mechanization as was mentioned in connection with the alphabet form.

Olfactory perception. In these experiments various odors were presented to the *S* by means of a small, improvised olfactometer. Perfumes, ammonia, benzoate, formalin, ether, and other stimuli were used. Immediately after the presentation of the stimulus *E* described her perception introspectively.

Her olfactory perceptions consist first of a peculiar substitution of visualized, synaesthetic and emotional responses for the usual type of synaesthesia. It is impossible for her to perceive odors in terms of their own sensory qualities or in terms of stereotyped synaesthetic equivalents. A given odor is first perceived as a visualized emotional state. Then, during the course of the emotional synaesthesis, there is a disintegration into fragmentary concrete, visual and visual synaesthetic imagery which definitely relates the experience to some similar or identical experience in the past. It is from the imagery of the latter stage that the recognition of the odor is derived. The completion of the percept varies markedly, depending upon the degree of mechanization which has taken place through the influence of frequent experiences of like odors.

Thus, the first meaning to develop in the perception of an odor is pleasantness, unpleasantness, or a mild emotion. This meaning is a context of colored imagery. The context is then elaborated and differentiated as in ordinary perceiving, although the contents remain exclusively visual.

Failure to recognize depends not only upon the absence of associations, but also upon the circumstance that concrete imagery of these experiences is associated with an emotional response. Further, a recognition may fail because of an evident inhibiting of concrete visual imagery by unusually vivid and prolonged synaesthesia of the emotional response. That is, emotional coloration preempts the entire visual field.

E's responses to olfactory stimulation passed through certain characteristic stages of mechanization. Genetically the first reaction was emotional. This consisted of a color whose behavior and attributes meant that the stimulus was an odor and that the odor was pleasant or unpleasant. It served, therefore, not only as an undifferentiated perception, but also as an affection. As familiarity with the stimulus developed, the original emotion-colors diminished in intensity and internal movement, while the response became elaborated to include concrete visual imagery having to do with the place, the time, and other circumstances under which the stimulus was presented. But this concrete imagery was not affectively indifferent until mech-

anization was complete, *i. e.*, until the original coloration was all but gone. Meanwhile these concrete associations included not only their own synaesthetic colors, but also additional coloration of their own emotional hue. As a consequence, the relatively homogeneous color of the original emotional response became a mottled, heterogeneous massing of subdued tints. Therefore, the final stage in the mechanization process, where recognition was almost immediate, was at once affective and cognitive.

Typical Introspective Data to Illustrate the Above

Stimulus: refined ammonia. As the experimenter handed me the olfactometer I cautiously put it to my nostrils, because I had been warned to go slowly. First I was aware of a showering of sparks of electric-blue and copper, localized in the upper nostrils, which seemed to emit from the membrane. This experience first meant unpleasantness, and then pain. Immediately there developed, in the surrounding regions, memory visual imagery, still consisting in part of vivid and intensive blue and copper in the form of sparks, together with concrete visual imagery of the place and of myself in the act of smelling a bottle of ammonia. In this instance the original appearance of the sparks did not identify the ammonia, but with the appearance of the concrete visual imagery the experience became meaningful in a concrete sense. I cannot get beneath the meaning; it seems to be inherent in the experience and yet there is nothing to describe as content beyond the imagery, other than an observable something attending the imagery. This something is actually present but partakes more of an inference. It is a left-over. I cannot tell what it is. It is like trying to recall something when you almost have it and cannot clear it up.

Stimulus: rose perfume. Immediately upon the presentation of the stimulus a wealth of synaesthetic imagery developed consisting of numerous shades of pink, lavender, and a suggestion of light blue. This was the content of a feeling response of intense pleasure. This imagery had barely appeared when it began to fade, give way, but seemingly to clear up again in part, in terms of concrete visual imagery of pink, white and apricot-colored roses; I saw a large bouquet of white roses localized on a table before me—the table at which I was working a short time ago—then a bush of large pink roses seen against the house. This imagery was followed by snatches of apricot-roses seen hazily in their setting as I observed them on the way to the laboratory. All of this visual imagery came in very quickly and attention then slumped. The perfume had been perceived. Again the parent process was present, especially in the first part of the experience, but I cannot get at it. I know that there was something else there aside from the emotional coloring, but at the same time it seems as if it were not there.

Perception of sound. E's perception of single tones usually consists of a synaesthetic visual image which carries not only an auditory significance but also a distinct tactual meaning with some form of bodily reference. The brightness of the image is invariably dependent upon the pitch of the tone. The hue and degree of saturation of the color depend in varying degrees upon the timbre, intensity and manner of appearance of the tone, and always involve affective components. As in olfaction, therefore, synaesthetic images receive their color from

two general sources. This fact stands out all the more prominently in *E*'s responses to musical selections.

For example, the sound of a 5th octave C tuning fork is perceived in terms of visual imagery likened to a molten stream of glucose with the brightness, transparency and tactual consistency of glucose. The image is tridimensional and moves through space as long as the stimulus lasts. The two forks of 5th octave C and 4th octave G sounded together produce a single stream in which the two images behave like different currents intermingling without losing their identity. The higher note continues to be perceived as distinctly brighter, while the lower note is darker. In objective vision there occurs no analogous phenomenon to the telescoping process which *E*'s synaesthetic imagery undergoes. Beats appear as vibrations in the imagery, something like the elastic quiver of jelly. Behind all this synaesthetic imagery lurks the parent process.

It is possible to demonstrate both the effect of intensity and the effect of unpleasantness upon *E*'s auditory sensory reactions. For example, when the stimulus is steadily increased in intensity, the synaesthetic imagery of the experience will remain relatively constant up to the point at which the intensity itself produces unpleasantness. Then the synaesthetic imagery immediately takes on a different color, which carries both the meaning of unpleasantness and the meaning of an increase in the degree of intensity. Speaking in terms of the hedonic scale, the point at which the curve of affection begins to descend marks the instant at which the imagery becomes another color.

The visual synaesthesis of a mildly blown Quincke tube is described as a soft glow of brightness steadily increasing in size, fanshape, until the intensity causes the experience to become unpleasant. Then the coloring changes to a longitudinal, striated image, of poorly saturated, dirty brown. The color itself is the unpleasantness of the situation as well as the intensity of the tone, the emphasis depending upon *E*'s mental set at the time.

It would be interesting to know if the added emotion due to a rise in intensity depends upon proprioceptive stimulation. It appears as if this were the case, for, up to a certain point, the experience is a typical auditory-sensory one, characterized by the same mechanized combination of affective and primary, sensory coloration as we found in the case of olfaction. Beyond this point, added emotional coloration brings changes in meaning, and hereafter the emotional colors function both as auditory intensity signification and as emotion.

E's perception of musical selections consists of an elaborate amplification and combination of those processes which take place in the perception of single tones. A musical selection of any degree of complexity tends to become colored more and more by the emotion than by auditory factors. The whole is a [illegible], flowing stream of color which never [illegible] movement as long as the music continues. The colors are projected out into space, where they are seen either upon or through the different external objects within *E*'s range of vision. It is diffi-

cult to describe this imagery of music, for it is changing from instant to instant, both in color and form. The change in form involves a peculiar variety of internal movement which is important in the problem of meaning and which gives mobility to the auditory consciousness.

Single instruments and solo voices produce much simpler synaesthetic patterns than do choruses and orchestral music. Below is given a series of abbreviated introspections which describe in a sketchy manner the imaginal content of short instrumental solos. The *S* was instructed to listen carefully to the music played to her on a phonograph, after which she dictated a complete introspection upon her experiences. Columbia demonstration records were used.

Violin: blues, yellows, rose shade of red; darker brown. All along molten, drawn-out mass with no break, dipping toward the lower tones and upward as the higher notes were played. Parent process.

Viola: reds, yellows, blues, more yellow and blue as the tones become higher in pitch, dipping the same as with the violin. Shades became darker as the lower notes were struck. Parent process.

Concert bass: so dark could hardly distinguish any colors. Looked like reds and greens in the twilight. Dipped slightly as in 1 and 2. In a few places, while the very lowest tones were played, the colors changed into midnight blue. Molten mass, only too thick to be stirred. Parent process.

Violin-pizzicato: fairies dressed in blue and yellow dancing on a green. The green, shadowy and somewhat colorless. The higher tones less color: these fairies had no feet or faces; were separated and distinct otherwise in form. Parent process.

String-quartet: so complicated and rapid in movement impossible to describe.

Harp: pastel shades of blue, yellow, red, and violet. They changed from one to another very rapidly, sometimes blending and sometimes contrasting; much of the time several colors were present at once; again several shades of the same color were present. The forms were rather intangible, like clouds; they changed so rapidly that there was no distinct form to any of the imagery; an ever-changing panorama of coloration. Parent process.

These introspections reveal a minimum amount of emotional coloring, for the imagery is greatly attenuated and the auditory origin predominates. The notes on violin-pizzicato illustrate a tendency on *E*'s part toward personification. Usually such a tendency consists of incomplete visual associations. In other cases, known to the experimenter, it has been observed that musical instruments produce reactions which in form and behavior appear like birds, animals and definite human individuals. Thus to one *S* the sound of a clarinet in the William Tell overture appears like an awkward wading bird of the stork-variety. Again, to another *S* the runs in Liszt's Second Hungarian Rhapsody are indefinitely personified as sober-faced, dignified old men running backward upstairs. At present we are making no attempt to explain the phenomenon or even to sug-

gest its function. However, we believe it to be an important factor in E's consciousness.

Our S's appreciation of music is much above the average, and is derived solely from the color-form and the behavior of her synaesthetic imagery. Three factors enter into the coloring of this imagery of musical selections. First, a mechanized color which is now dependent upon the sensory quality of the music itself; secondly, the color which is injected into the imagery by emotional states aroused by these synaesthetic experiences; and thirdly, the tactual reference which is incorporated both into the color and into the form of the visual imagery. It is difficult for E to analyze her appreciation of music sufficiently for us to ascertain the extent to which each one of these three factors determines the color and behavior of the imagery. In the greatly appreciated music the synaesthetic imagery never exists unmixed with emotional coloration. There occurs a point in the ascending scale of pleasantness at which the colored perception of the instruments as such changes over entirely into a synaesthetic reaction. It cannot be emphasized too strongly, however, that even here this new set of imagery functions as both perception of the music and as an emotional response to it. Usually there occurs some form of tactual reference in both the sensory and the emotional increment of the perception.

Introspective Data Illustrating the Three Different Factors

(1) Auditory factor. Stimulus: Hymn to the Sun; violin; Kreisler. The synaesthesia of the violin tones appeared as long, drawn-out, wavy festoons of red and yellow, moving over the visual field in a horizontal direction. The piano accompaniment appeared as drops of color tending toward a stone-blue. The latter were beneath the violin images in position. Small amount of emotional imagery present appearing in alterations, blendings, and obliterations of the separate piano and violin tones. (2) Emotional factor. Minuet in E; Beethoven. It was that delightfully pleasing music consisting of a full pouring-forth of color, tending toward etherial reds, yellows and greens. Occasionally these colors were richly saturated. The colors were projected out into space in the form of vapours through which I had to look in order to see objects in actual vision. This selection was perceived entirely in terms of emotional synaesthesia; the sensory factor, as such, did not appear; there was no immediate consciousness of tones as such. (3) Tactual factor. Notes from introspection taken immediately after hearing a piano selection by a friend. The piano music appeared in a synaesthetic blue with a decided tactual reference, producing an experience similar to that of running the hand over a series of velvet tubes partially inflated with air. The tactual elements represent, in meaning, the quality of music, while the blue color is the [illegible] quality of the player determining the hue of the tones.

Illustrating the same [illegible]

[illegible]: Jazz orchestra made up of saxophone, banjo and piano. "The experience was decidedly unpleasant; the colors were made up of turkey reds of [illegible] and metallic sparkles of blue and copper. This unpleasantness includes a reference to the body, perceived in terms of the scraping of rough metal across the forehead, sides of the head,

around to the back of the scalp and through to the surface of the brain. This bodily reference is not tactual as such, but an interpretation from the turkey-reds and greens. The experience contains a tactual signification—the only immediate tactual experience I can describe; its content is color."

Objective vision produces an emotional synaesthetic phenomenon which corresponds very closely to that appearing in the perception of music. *E* affirms that most objective visual situations produce an accompanying color which is visualized as projected in front of, upon, or through and beyond the actual objects. These colors carry a definite emotional value of pleasantness or unpleasantness, delight or disgust. The like or dislike for objects, landscapes, *etc.*, is carried in the accompanying colors. This is best illustrated by the following quotation.

Notes on the change of coloration of a saxophone. "The boys who live in the second house from us have a jazz orchestra which always includes a saxophone. Its coloration was always a turkey red, orange, and bright blue mingled with a disagreeable shading of dirty greens and brown. Subsequently I became acquainted with the saxophone player whom I learned to like very much; he turned out to be an affable fellow with considerable personal charm. The next fall the jazz orchestra was again in full operation and I was surprised to find that the turkey reds, dirty greens and browns of the saxophone had now been modified by pastel shades with attending pleasantness. Later some boys were serenading in the neighborhood; the music did not fully awake me, but in a sort of dazed state I was conscious of tones which had the colors of a violin; this waked me still more and I listened attentively for some time, enjoying the quality of the music. I was greatly surprised to learn that it was this same boy with his saxophone. Ordinarily I should never confuse a violin with a saxophone. The confusion at this time seemed all the more unreal since I was perfectly familiar with violin and saxophone music played under similar circumstances."

If *E* happens to be looking at objects before her at the same time that she is listening to music, there frequently takes place a peculiar conflict or blending in the resulting emotional color reactions. Sometimes *E* finds herself trying to evade one conflicting emotional reaction in favor of the other. This is best illustrated in situations such as an opera or a motion-picture performance. In case the color produced by pleasantness or unpleasantness of the visual scene harmonizes with that produced in the perception of the accompanying music, the one enhances and amplifies not only the appearance of the other, but also the enjoyment and intensity of the other. When the reverse occurs, one set of colors tends to neutralize the other, both in saturation and in brightness. The same general laws of emotional coloration operate in the processes of amplification and neutralization as were found operating in the color scheme of the year form.

In summary, pleasantness as an accompaniment to vision consists of reds, blues and yellows, and as an accompaniment to audition of rose-reds, blues and brighter yellows. Unpleasant-

ness, as a visual accompaniment, consists of dirty browns and dull greens; as an auditory accompaniment it consists of dirty browns and less saturated, dead greens.

Auditory meanings in the perception of color. The content of our *S*'s perception of color has been studied very carefully with the purpose of isolating those factors which supplement the visual. Our procedure was to place a large sheet of colored paper in front of *E* with the instructions to describe minutely the color of the paper and any associated processes. Yellow, green, red, blue and green-yellow were used. Since several factors enter into *E*'s perception of objective color, it is impossible to discuss any one properly out of its relation to the entire process, although that is what seems necessary in analysis.

Upon the sensation of objective color *E* superimposes a considerable amount of synaesthetic imagery which functions as the meaningful part of the experience. This additional and supplementary imagery involves two factors: the sensory quality of the stimulus produces an emotional response which appears partly in the stimulus-color itself and partly in some additional color of synaesthetic origin, which is projected upon the objective stimulus. Incorporated within this perception of the color is the second factor, which consists of a stereotyped "association" with music. The experiencing of music, of course, is of auditory origin, and here there is no such auditory definiteness in the visual perception as there is in the tactual associations with music. It is possible, however, for *E* to judge the approximate pitch of this imaginal music from the degree of brightness in the stimulus-colors. The nature of the music derived from the stimulus-color depends upon the tint and degree of saturation. When fully described, this derived music is more like a memory of the music than the actual experience of hearing it. It is a musical experience in the absence of the parent process. *E* describes it as appearing static without internal movement within the image. Vitality is lacking in the rhythm, although the rhythmic striations appear in the imagery.

This point is illustrated by the following introspections. The Minuet, by Beethoven, was being played to the *S*. As soon as the synaesthetic coloration in her auditory perceptions was fairly well under way, the blindfold was lifted and she was confronted with a large sheet of violet paper. "The music aroused a wealth of apricot color, numerous tints of pink and lavender. Upon opening my eyes these colors were projected through and beyond the violet paper and my objective vision of the paper changed to a perception of a film-haze of violet color, which harmonized with the auditory, colored background."

[illegible]

the lighter tint to a darker tint; the latter was almost an apricot, the color as a whole produced a symphony of which the bright part of the yellow was the melody and the melody fluctuated from this to another produced by the darker, apricot tint. Neither melody was recognized as any particular selection, although both were orchestrations without indi-

vidual instruments standing out. Both melodies were above the pitch of middle C, a judgment following from the brightness of the colors. This imagery is not auditory; it is synaesthetic visual imagery with auditory signification. The signification appears in the behavior of the visual forms. Their auditory meaning is present only when the experience is perceived as a whole; but it is very definite. On analysis it resolves itself into visual imagery.

Green: The green is made up of several shades which run to the lighter and darker tints. This color produces a musical meaning more unpleasant than the above; the key is below middle C; it was perceived as the voice of a Russian singer with a symphony playing off in the distance. The green quality meant Russian and the other shades made the accompaniment.

Again, in these musical experiences that accompany objective vision we find a tendency toward personification. An account of this phenomenon can best be made in terms of a reference to olfaction. Here it was found that the sensation itself, although synaesthetic, did not function as a recognition of the odor. It was necessary to continue the process until some definite imagery appeared with which the synaesthetic, emotional color could be associated. Then recognition appeared. Likewise in the perception of the violin-pizzicato, the fairies constituted a definitization of the perception process. It appears as a growth toward differentiation, *i. e.*, an elaboration of meaning. It turns out therefore, to be an emotional experience which is made objectively meaningful in the form of a perception. The musical experience derived from objective vision is an amplification of the visual perception. Objective color has in it visual qualities which are common to the visual synaesthesia of emotions experienced in auditory perceptions. This fact probably explains the close relationship between the two types of perceiving.

Cutaneous and kinaesthetic perceptions. Bodily sensations always appear in terms of visual synaesthetic imagery. The moderate and ordinary sensations are visualized upon the body only under a special *Aufgabe* to do so. When the experience reaches a certain degree of intensity it is recognized as pain, and the synaesthesia takes on an emotional color-process projected either upon the body or out in space. When the emotional color is present it serves either as emotion or as pain.

In one of our experiments the forearm of the *S* was anaesthetized by freezing with ether. Then there was applied to the anaesthetic area a stylus which was connected with an inductorium. Kinaesthetic sensations appeared as intense and streaked grey forms with a very little yellow in them. Further up on the arm the streaks fused; thus the individual muscles and tendons lost their localization. This imagery was visualized upon the forearm.

The anaesthetic area was permitted time to become sensitive and the intensity of the electric current was increased. Upon the second application of the stylus appeared a confusion of synaesthetic colors consisting of bright red, yellow and blue. These colors were mixed pain and emotion. Part of the time they were projected upon the forearm and part of the time out in space. These pain-emotions consisted of unusually bright and "glowing" colors.

Emotions proper. Emotional states as such exist as colors projected out in the visual field. The colors last as long as the emotional stimulus lasts, and the intensity and prevalence of the color are a measure of the intensity of the emotional reaction. It is possible for *E* to carry all day the color of an emotional state, while at other times the color will immediately fade. These hues range from dark shades to light tints superimposed upon objects in the environment, to colors so saturated and vivid as to obscure the visual field.

Illustrative introspections reduced to note form. Read of accidental death of a near neighbor; the newspaper suddenly turned green and took on a swirling motion; as I read the details the color faded somewhat but for several hours my imagery took on a greenish cast. Later in the day I picked up the American Magazine and discovered a reference to an old acquaintance. The green cast changed to colors characteristic of happier moods —rose, orchid and cream-yellow....Upon learning one day in class that we were to be quizzed on a certain assignment my imagery became flooded with a washed out green and dirty-color, with flashes of turkey-red, bright blue, bright green, and pale orange, representing a mixed emotion of fear and a feeling of helplessness.

"I was rather tired, my imagery inclined to neutral gray. The experimenter was playing Beethoven's Minuet. While the resulting imagery was normal for that selection, it was rather subdued in shade. The colorings were blues, roses, apricots, lavenders and pinks. Then the experimenter blindfolded me while the selection continued, only to find, on seeing again, a sheet of very bright magenta color before me. Instantly for me the phonograph stopped playing, *i. e.*, the magenta color obscured the colors of the tones, as a result of which they lost their emotional value but retained their meaning as mere tone in terms of indifferently colored, moving forms. Colors changed to shades of sulphur, dirty brown, *etc.* Imagery meaning great distaste. The minuet was no longer a delight; pleasant shades of color were modified by distasteful ones."

Summary and Discussion

(1) In so far as this investigation relates to the previous work of Wheeler and Cutsforth, the present results are in entire accord with those obtained heretofore. Principal among them are the following. (*a*) The process of synaesthesia is distinctly a process of perceiving or of cognizing, the associated imagery of which plays the same rôle in synaesthetic *Ss* as in asynaes- thetic *Ss*. The only [illegible] groups of individuals is the stereotyped and highly colored character of the secondary increment in synaesthetic individuals. (*b*) The associated colors provide the context necessary in the

development of meaning. Constituting a part of that context is a process which the *S* is unable to describe. Any attempt to define it results in a centering of attention upon some feature of the associated image. We have chosen to give the name "parent process" to this phenomenon. It seems to be that increment which, in asynaesthetic individuals, is referred to as an olfactory, auditory, cutaneous or kinaesthetic quality. Presumably it may be called sensation as such. Its presence and absence are noted in a total-perception of any given experience; but when this noting becomes at all specific the *S* finds that he is referring to internal movent or "vitality" of the experience. Beyond this, little can be said about it at present. (*c*) The absence of this parent process is one of the identifying features of synaesthetic, imaginal experiences as opposed to "objective" experiences. (*d*) In certain individuals at least, synaesthetic phenomena pervade the entire mental life. It so happens that this statement has been true of all cases studied intensively up to the present time.

(2) Our *S* is completely synaesthetic. Not only are the perceptions in all non-visual modalities characterized by the absence of non-modal qualities as such, but visual perceptions and visual images are also in part emotional in their interpretative stages. Synaesthesis runs not only into the use of visual imagery, but also into visual imagery whose significance is emotional and tactual. That is, visual perceptions contain color-imagery of a synaesthetic character.

(3) Within the limits of this investigation all of *E*'s mental processes, other than the highly mechanized, contain an emotional increment present in terms of color-imagery. This emotional increment is present in inverse ratio to the degree of mechanization of the mental process in question. Thus, mechanization and attenuation are characterized not only by the loss of form, position, internal movement and detail, but also by desaturation of the colors having emotional value. In other words, mechanization, in *E*'s case, involves the process of approaching indifference. This situation is analogous to the diminution of meaning which results, in asynaesthetic individuals, in an approach to the sensory level of experience.

(4) The same colors, under different interpretative mental sets, function as emotion, as tactual, or as auditory perceptions.

(5) The presence of emotional increments in *E*'s perceptual processes seems to hark back to emotional reactions of early childhood common to visual, auditory and tactual stimulations; and the common denominator, or medium of association between visual, auditory and tactual meanings now operative in *E*'s mental life, is color-imagery functioning as emotion.

(6) Particularly evident, as an example of this situation, is *E*'s tendency to associate melody with objective colors. These associations are not present in terms of auditory images but in terms of an ambiguous emotional synaesthesia. That is, the emotional coloration may refer to the objective color or to the auditory meaning, equally as well, depending upon mental set.

(7) This confusion, or what would seem to the asynaesthetic *S* to be a confusion, between feeling and intellectual processes shows how closely the two modes of response are related. Indeed, it seems evident that it is impossible to differentiate between the two except upon interpretative grounds. In *E*'s case generalized or undeveloped perceptions are always as emotional as they are cognitive. In case of a full-fledged perception which passes through observable stages of definitization or elaboration, the first stage of development is a feeling process whose imagery becomes modified and elaborated as the perception becomes concrete and the object is identified.

In *E*'s case, therefore, emotion and cognition are not to be described by differences in content but by differences in attitude. The two modes of response are not to be contrasted in terms of aspects. The difference is a derived phenomenon, representing two successive stages in the course of development of a perceptual process. Neither the emotional nor the so-called cognitive stage of a perception is a matter of immediate experience; *i. e.*, they are not existential but interpretative phenomena. We are inclined to suspect that these facts are equally true of all individuals.

(8) Our results seem to throw light upon the problem of the stimulus or meaning-error. Tactual, auditory and emotional factors in *E*'s mental life, existing alone or in mixtures, are distinctly meaning phenomena, unanalysable except in terms of meaning, whose only observable content is visual. Tactual, auditory or emotional significance appears only in a total and complex experience; it is a characteristic of the complex as such, not something which can be reduced, adequately, to single contents of a sensory character. We are not here promoting the theory of imageless meanings, but we are inclined to believe that meanings are in themselves contents, deriving their structural character from synthesis. This explains why, when perceived as a whole, a certain experience possesses a definite tactual or auditory meaning; but, when analysed into its [illegible] original character and becomes nothing but visual imagery. The difference between the [illegible] and the analysed state is to be explained in terms of mental set. In other words, the problem is a functional one. The logical conclusion from this, of course, is the view that every experience

contains an irreducible increment—the meaning quality prior to analysis; an that no experience, however simple, exists in the absence of this functional phenomenon. In other words, as defined by conventional psychology, there is a meaning-error in every mental process. This is no other than the interpretative factor characteristic of every perception.

(9) It would seem that in *E*'s case visual perceptions involve a parent process. The redness, yellowness, greenness, *etc.*, are in reality such parent processes camouflaged, as it were, by mechanized associations of long standing, and simplified to such an extent that the awareness of color seems to be an elemental experience. But on more careful analysis this elementary character of the experience turns out to be a meaning-phenomenon and therefore complex. The ordinary *S* finds hue, saturation, brightness in his visual reactions so immediately as to lose the associative processes by means of which these colors become a phenomenon of awareness. On the other hand *E* brings such apparently extraneous associations to bear upon her objective vision as musical meanings whose contents are visual, synaesthetic images. These associations are so definite as to make possible judgments of pitch. It is a curious fact that these auditory associations are not auditory in content, for auditory processes, both sensory and imaginal, are visually synaesthetic. Hence auditory perceptions, both of direct and indirect character (by way of auditory stimulation or round-about association), are meanings whose content is visual. So far as we have been able to determine, where such auditory or emotional associations are not present, *E*'s consciousness of color is vague and indescribable.

(10) We may ask the question, why does our *S* insist upon describing so many of her experiences in terms of emotional coloration? Why can they not be described simply in terms of brightness and color qualities? Obviously for the simple and ample reason that it is impossible; for without this emotional reference the experience ceases to exist. To inhibit the emotional significance means to alter the total experience in such fashion as to make an entirely different sort of thing out of it. The same thing is true for the tactual increment in *E*'s perceptual processes. *E* is utterly helpless in attempting to describe this tactual experience in any other terms than meaning. Opalescence and etherialness are as much attributes, for her, apparently, as granularness, arealness or what not are for the conventional laboratory *S*. Attributes are seemingly indeterminate in a last analysis.

(11) Foremost in *E*'s mental life is the process of interpreting. This is functionally elemental and depends upon the circumstance that the simplest possible experience is a complex.

Introspectively it is impossible to get beneath the percept with its interpretative factor. It seems to the writer that this fact has not been taken into account sufficiently in systematic discussions.

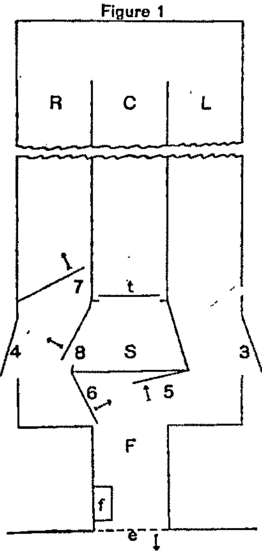
(12) The rôle of emotion in *E*'s mental life is twofold. First, it functions in undeveloped perceptions, and secondly, as the end-stage in elaborate recognition processes. Consequently, in a certain proportion of her memory processes, where mechanization has reached its limit, emotional coloration has all but disappeared. In the remainder of her recalls the emotional content, in terms of synaesthetic colors, remains in varying amounts up to complete predominance. In her case, of course, emotional experiences as such are recalled definitely in terms of visual images. We have come to the conclusion that, in a last analysis, feeling processes are undifferentiated cognitive processes, *i. e.*, perceptions in which either a labelling or identification process fails to mature, and that this fact holds for asynaesthetic individuals as well. It would seem that in ordinary laboratory investigations of feeling, *S*s have looked for contents whose characteristics and behavior show marked differences from the so-called perceptual reactions. In the light of our results it is evident that, before we can be certain of our psychology of feeling, a great deal more careful experimentation will be necessary, in which more regard is paid to the factor of interpretation in mental life.

THE BEHAVIOR OF SHEEP AND GOATS IN LEARNING A SIMPLE MAZE

By Howard S. Liddell, Physiological Field Station, Cornell University

In the course of an investigation concerning the effect of thyroidectomy on the higher activities of the nervous system a simple maze was constructed which can be learned by sheep and goats and by means of which it is possible to study habit formation in these animals following the extirpation of the thyroid glands. The ground plan of this maze is shown in Fig. 1. A detailed description is to be found in a previous article (*1*).

Figure 1



It consists of an enclosure 67 ft. in length and 10 ft. wide covered with cinders and surrounded by a board fence. One end of the maze joins the barn, in the second story of which is the observation window covered with fine wire mesh. There are wire netting partitions between the three parallel alleys. The sheep or goat to be tested is confined in the starting compartment S. When the door t falls the animal emerges and, impelled by the gregarious instinct and usually by desire for food as well, finds its way down the alley C. Either outer alley, L or R, may be made a cul-de-sac by closing gate 5 or 6. The animal must learn to avoid the blind alley and to go up the open alley to the feeding compartment F where it can see the flock in the barn through the screen door e. Learning is judged complete when it makes three successive direct trips through the maze.

A number of cretinoid sheep and goats have already succeeded in learning the labyrinth; but in this paper only the behavior of normal sheep and goats will be considered. Three groups of sheep have learned the simple maze: first, 11 lambs from two to three weeks of age tested during May and June 1921; second, 17 May lambs tested in July and August 1922 at about two and one half months of age; and third, a group of 18 May lambs of the same year and procured from the same flock, tested in December 1922 at about six months of age.

Analysis of the behavior of the lambs in learning the simple maze. The behavior of each animal exhibited certain characteristic features which determined, to a varying extent, its pro-

ficiency in learning the labyrinth. This was noticed in testing the first pair of twin lambs. One was tentative and vacillating in its exploration of the maze. Each few steps were followed by a pause or change of direction. Its movements became slower and the pauses longer during the first trial, although it continued bleating almost constantly. The other, however, exhibited rapid and hurried movements with few and brief pauses. Its whole behavior was vigorous. Frequently it went the length of alley C at a gallop and as a result it very shortly escaped from the labyrinth in the first trial.

Another trait which influenced learning was the tendency to explore the screening between alleys. This exploration was a frequent cause of the accumulation of errors in the early trials. Standing against the screening, butting it, and trying to nose under it or jump over it often consumed much of the time required by some lambs to escape from the maze in the first few trials, while other lambs never made any persistent effort to force themselves through the screening of the alleys.

Lambs trying to escape from the labyrinth when the position of the cul-de-sac has been reversed frequently make persistent efforts to jump either the wall at the end of the blind alley or the adjacent gate closing the entrance to F, while other lambs make few or no such attempts.

Nervousness, although a definite characteristic of the behavior of certain lambs and sheep, seems to bear no direct relation to proficiency in learning the maze. It has already been noted (*1*) that as a rule the animal in F will, after two or three trials, continue eating at the food box while its pedometer is being read. (The pedometer attached to the fore leg gives a record of the animal's activity in escaping from the labyrinth.) The excitability of some lambs seems to persist. One, for example, "did not overcome her nervousness in the presence of the observer, and would not eat while the pedometer was being read, as did her twin sister, but always stood close against door e, often scratching at it before the observer appeared." In spite of this she learned both the simple labyrinth and the simple labyrinth with the position of the cul-de-sac reversed in less time and with fewer errors than her more phlegmatic sister, although at the end of the ninth trial of the first problem her nervousness was so pronounced that, in fear that she might refuse to run the maze, the tests were discontinued for the day.

One extreme case was encountered of an adult ewe which could scarcely be trained, because of her hyperexcitability, to [illegible] succeeded in escaping from the labyrinth. So great was her nervousness that she stood for two hours at the entrance to S and never ventured farther than halfway down alley C. There is little doubt that her unwillingness

to explore the maze was due to nervousness, since she appeared terrified and charged down C past the observer when he approached to release her from the maze. The next day she would not leave S but remained almost motionless at the entrance for two and one-half hours. It should be mentioned that the only form of punishment employed in these experiments is the presence of the observer in the maze. On a number of occasions a sheep or goat has refused to explore the maze further. If the observer then enters the labyrinth and approaches the animal it becomes terrified and makes violent efforts to escape. In subsequent trials it will usually escape from the maze with no further loitering.

This nervousness is probably only an exaggerated form of the timidity exhibited by all sheep and lambs when isolated from the flock. In certain instances, where some unusual occurrence has frightened the animal in the maze, it will refuse to run and usually can never again be induced to attempt to escape from the labyrinth. Two cases may be mentioned.

Lamb No. 1 of the 1922 series learned the simple maze in 7 trials and with but 30 errors on December 7, 1922. On December 8, with the position of the cul-de-sac reversed, the lamb, after having explored the maze for one and one half hours, was startled by the firing of a gun twice about one hundred yards distant. At the end of two hours the lamb, which had not left alley C, was driven through the maze. In a second trial, a few minutes later, the animal did not leave S and was released from the maze in 10 min. The firing of the gun may not have been the sole cause of failure, since the lamb had already been for so long in the maze, although it was observed to react to the firing—On December 13 a strong wind was blowing dead leaves about the maze. Lamb No. 6 succeeded in escaping from the labyrinth in 369 sec. with a total of 17 errors. In the second trial the animal, which appeared startled by the moving leaves, would not leave alley C and was driven through the maze at the end of 40 min. It, too, appeared to be much frightened and the leaves blowing about seemed to be the cause. Two other lambs, however, succeeded in learning the simple labyrinth the same day during the interval between the testing of Lamb No. 6 and Lamb No. 1.

A disturbing factor which was not anticipated and which affected to no small degree the results of the earlier tests was the tendency of the lambs in the maze to interrupt their explorations at frequent intervals to nibble at the few small tufts of grass and weeds in the labyrinth. They would even stop to nibble in the midst of violent efforts to escape from the maze. Finally, the tests were interrupted and the ground was covered with ashes and cinders. Even this proved ineffective in repressing the nibbling, which was recorded five times in the trial immediately following. It must be concluded that this tendency is an integral part of the behavior of the sheep in running the maze. Even though great care was taken each day to weed the labyrinth thoroughly and to remove all small objects which might attract the animal's attention, such precautions were of

no avail. At certain intervals it would begin nibbling, even nosing into the cinders and ashes. Any object, such as a small wood shaving or bit of dead leaf, served its purpose.

There is well marked difference between the behavior of the sexes in learning the labyrinth. The males are more phlegmatic in their reaction to the new situation. In general, they seem much more self-possessed. They bleat frequently, as do the females, but not so excitedly or loudly. Even the more nervous males very soon become deliberate in their exploration of the maze. In this connection reference should be made to similar observations of Yerkes (*2*) on sex differences in the discrimination behavior of the ring dove.

As yet no evidence can be presented as to the exact age at which the lamb will run the maze while its mother is confined to the barn with the rest of the flock; in other words, the age at which the incentive to learn the maze is the same as that which impels the adult sheep to learn. A lamb as young as two months will eat at the food box f at the conclusion of the tests and will learn the labyrinth without the presence of its mother in F. However, when admitted to the barn it will seek her out and suckle. A lamb of three weeks will, however, bleat continually when separated from its mother even when confined with the rest of the flock. Observations of the sheep in pasture indicate that lambs as young as two months are impelled by the gregarious instinct rather than the more specific impulse to follow the mother. At this age the lambs graze together in small groups and are often found at a considerable distance from their mothers in the pasture. When the flock is being driven to the pen for the night the ewes will bleat and the lambs reply, but they do not necessarily run to their mothers.

Although, as previously mentioned, the effect of the ewe's bleating on the young lamb in the maze indicates that in all probability the desire of the lamb to be with its mother is the incentive activating its behavior, the bleating must be classed among the disturbing factors which diminish the reliability of the learning results. In the first place, the bleating of the ewe is inconstant. When confined in F she may bleat continually, especially during the earlier trials, but if kept in the barn with the rest of the flock she bleats usually only at irregular intervals. When the older lambs and sheep are running the maze bleating among the flock in the barn seldom occurs. The lamb or sheep in the maze exhibits considerable inconstancy in bleating. Most animals bleat almost continually during the first [illegible]

[illegible] later. The occurrence of bleating is as unpredictable as in the case of the sheep confined in the barn. In the second place, the response of the lamb in the maze to the bleating of [illegible]

or to some other sheep or lamb in the flock cannot be predicted. It would be anticipated that whenever the lamb heard its mother bleat it would reply and run toward the source of the sound. This the young lambs usually do, but not always. They sometimes reply without moving or may even run away from the sound.

Although the older lambs and sheep, as a rule, eagerly seek the food box f after escaping from the labyrinth, it has been shown that the desire for food is not the dominant incentive. First, the animals do not always eat at the conclusion of a test. For example, in learning the alternation problem, four successive trials per day were required and the position of the cul-de-sac was reversed at every trial. Considerable difficulty was experienced by the lambs in avoiding the blind alley. It frequently happened that they became excited as the trials progressed, and in many cases they would refuse to eat during the concluding trials of each day, remaining instead near the door e through which the flock in the barn could be seen. In such cases, the lambs, which were well trained, ran the maze as usual though the incentive provided by the food was no longer operative. The same thing has been observed in the case of nervous lambs who refuse to eat between tests but nevertheless succeed in learning the maze, in certain instances more rapidly than lambs which constantly feed at the end of each trial. Secondly, it was accidentally demonstrated that when the lamb in F cannot see the flock through the door e it will not eat. In the early experiments with the labyrinth the animal was taught to go from the barn to F, not through door e, but around the barn and through gate 4. The wooden storm door at e was on one occasion left closed; so that when the lamb was driven from the barn and, entering the maze through gate 4, reached F it could not see the flock through the screening at e as usual. On every previous occasion it had begun to eat at once, but this time it did not eat and was found nervously exploring F. When the storm door was opened and the flock was again visible through the screen door e, it immediately approached the food box f and ate as usual, paying no attention to the flock in the barn. This observation has been frequently confirmed.

An interesting situation developed when one of the ewes which had been running the maze for two years dropped twin lambs during the period of learning the alternation problem. Training was at once discontinued but was resumed a short time later. Although her lambs in the barn bleated continually the ewe gave no indication of attending to them but remained quietly in the starting compartment S and walked through the maze as deliberately as usual. At the food box f, with the lambs in the barn in plain sight, bleating excitedly, she continued eat-

ing without giving them the slightest notice. This indifferent attitude toward the lambs was exhibited during the tests of the succeeding days.

In spite of the continued potency of the flock instinct, other impulses influence the sheep's behavior in the labyrinth. This was soon realized in the first series of tests when ten errorless trials were required. In the early tests each lamb, apparently, made every effort to escape from the maze. Once the path had been discovered and traversed a number of times other impulses such as curiosity and the desire to graze, attention to chance stimuli such as noises outside the maze, etc., modified its behavior. Most of the errors which appeared in the later trials were due to these interfering factors. The time records also show irregularities during the later errorless trials which are due to the tendency shown by most of the animals to traverse the maze in a leisurely fashion once they are familiar with the path and are not startled during the trials. Since the sheep, and especially the lambs, are easily distracted and are irregular in their behavior once they have traversed the maze without error, it was decided, as mentioned above, to make three errorless trials the criterion of completed learning.

A number of observations of the sheep in the maze have shown the importance of kinaesthetic impressions in determining the animal's learning.

In one case the lamb at full gallop turned toward alley L as in previous trials but turned before reaching the end of the main alley C. It therefore galloped back the way it had come. This observation has been confirmed. The lamb on these occasions always seems surprised to find itself, not at the entrance to the feeding compartment, but back at S, and its actions give every sign of bewilderment. This type of behavior forcibly suggests that the animal is depending largely on kinaesthetic rather than visual cues in running the maze. In some cases the lamb in galloping down C will, when it turns before reaching the end of the alley, run directly into the screening between the alleys. Hunter's remarks (*3*) concerning the labyrinth habits of the pigeon are interesting in this connection. He notes that Rouse, working with a maze identical with one of his own, "calls attention to the attempts of his pigeons to butt through the partitions in the first part of the maze and later to turn before the end of a given alley was reached. No such behavior was noted in the present experiments with wooden partitions. Head-on collisions with the walls never occurred."

[illegible] movements by impressions from its proprioceptors is seen during the learning of the maze when the position of the cul-de-sac has been reversed. After having run the maze a number of

times it no longer enters the blind alley. Nevertheless, when it reaches the end of C, it turns toward the cul-de-sac so suddenly that it appears to be jerked in that direction. It then immediately turns and goes up the open alley. In the succeeding trials the turn toward the cul-de-sac, before entering the open alley, becomes less and less extensive; but it occurs just as suddenly and suggests just as strongly as at first that the sheep is being jerked or pulled in that direction. In one case the animal always made a sudden and complete turn, first toward the cul-de-sac, and then without pause continued the turn until it faced the open alley.

The distance receptors are, of course, not inactive during the running of the maze, but their influence is not dominant in effecting learning. Evidence has already been presented to show the influence of auditory stimuli such as bleating and loud noises. The following observation is interesting as regards the possible rôle of vision. A lamb galloping up alley L suddenly stopped and stared at the fence, strongly suggesting that the movement of its shadow had startled it. In one other case this behavior was observed. The complexity of the environment provided by the simple maze is indicated by another chance observation. The falling of the heavy trap door t eventually caused two of the boards to be broken. These were replaced by boards which were not painted gray as was the rest of the door. Two normal sheep, which had been running the maze for three years, each made a sudden pause at the entrance to the starting compartment S, after which each advanced slowly and nosed the new unpainted boards of the trap door. On succeeding trials this behavior was not repeated. However, tracks in the snow are apparently not noticed by the sheep in the maze even when freshly made in new fallen snow. For example, a sheep will leave a fresh trail leading up the open alley to the food box and will turn into the cul-de-sac where there are no tracks in the snow. The effect of change of illumination on the sheep's behavior in the maze has not been determined, although there is some evidence for believing it to be a disturbing factor.

Behavior of goats in the maze. Six normal goats and two cretins have succeeded in learning the simple maze described above. No essential differences have been observed between the sheep and goats in their method of seeking escape from the labyrinth. In Table I and II are to be found the records of the learning of the simple labyrinth and the learning of this labyrinth with the position of the cul-de-sac reversed by a young male goat and by a ram of about the same age.

TABLE I

Male goat, age 4 months

Problem: the simple labyrinth

Trial	*Errors*	*Steps*	*Time, in sec.*
1	0	1112	24
2	0	417	15
3	0	556	14

Problem: the simple labyrinth with position of cul-de-sac reversed

Trials	*Errors*	*Steps*	*Time, in sec.*
1	13	1391	989
2	33	2364	3025
3	0	417	92
4	0	278	35
5	0	417	26

TABLE II

Male sheep, age 3 months

Problem: the simple labyrinth

Trial	*Errors*	*Steps*	*Time, in sec.*
1	0	278	39
2	1	423	80
3	0	278	61
4	0	423	62
5	0	423	75

Problem: simple labyrinth with position of cul-de-sac reversed

Trial	*Errors*	*Steps*	*Time, in sec.*
1	12	1535	888
2	55	2503	3864
3	21	979	558
4	15	1113	674
5	2	423	53
6	0	423	44
7	0	423	59
8	0	556	44

Age as a factor in maze learning. Three females more than three years of age, two sheep and one goat, were tested in the simple maze and in every case refused to explore it. All were untrained in maze learning. One of these ewes was driven through the labyrinth, and in succeeding trials ran at once from S to the feeding compartment. When the position of the blind alley was then reversed, she remained at the end of the cul-de-sac until driven through the labyrinth. Again, in succeeding trials she quickly escaped by the new path with no errors. However, sheep from two and one half to three and one half years of age will readily explore the maze which they have learned as lambs after an interval as great as two years. Not only will they relearn the simple maze but they will also learn new and difficult problems such as the alternation problem

Summary

A simple maze with a single cul-de-sac has been constructed in which the position of the cul-de-sac can be reversed. This maze has been learned by both sheep and goats.

An analysis of their behavior in escaping from this labyrinth is presented above.

There seems to be no essential difference between the maze-learning behavior of the sheep and the goat.

The adult animal will not explore the labyrinth unless it has been trained in the maze when young.

References

(1) H. S. Liddell, *Quart. Journ. Exper. Physiol.*, 13, 1923, 191.

(2) R. M. Yerkes, assisted by A. M. Eisenberg, *Journ. Animal Behav.*, 6, 1915, 41.

(3) W. S. Hunter, *Journ. Animal Behav.*, 1, 1911, 283.

THE PATELLAR TENDON REFLEX AND AFFECTIVE TONE

By H. E. Burtt, Ohio State University, and
W. W. Tuttle, University of Illinois

A technique for automatically eliciting the patellar tendon reflex has been devised by one of the writers.[1] Some incidental results in connection with other problems that were studied by this technique hinted that the reflex might be somewhat responsive to emotional conditions. Before proceeding to the specific study of such conditions it seemed worth while to make some preliminary experiments with the simple affective processes. The results of these preliminary experiments are reported in the present article.

Stimulus words were used to produce the affective states. Inasmuch as such words have been shown to produce differences in the association reaction time presumably because of affective differences[2] it seemed well to combine the KJ experiment[3] with the association reaction experiment and to record both types of reaction to each stimulus word. This procedure made possible a comparison of KJ results with those of association as influenced by affective tone.

Material. The stimulus material consisted of 120 words. These comprised the 90 used by Tolman[4] and 30 others. There were equal numbers of supposedly pleasant, unpleasant and indifferent words. The pleasant or unpleasant words belonged roughly to the following categories: sense qualities, success, failure, love, family, food, death, disgust and misfortune. They were arranged in groups of 5 words of a given category. Indifferent (I), unpleasant (U) and pleasant (P) words rotated in that sequence throughout the experiment. The complete list follows in the order of actual presentation, viz., I-1, U-1, P-1, I-2, U-2, P-2, etc.

I-1	U-1	P-1
still	rough	smooth
round	dusty	fragrant
crooked	overcast	sunny
high	dull	sparkling
narrow	dingy	silvery

[1] W. W. Tuttle, An apparatus for eliciting and recording the patellar tendon reflex, *Amer. Journ. Physiol.*, 68, 1924, 338-344.

[2] E. C. Tolman and I. Johnson, A note on association-time and feeling, [illegible] 1918, [illegible]

[3] Throughout the article the symbol "KJ" will be used to denote knee jerk or patellar tendon reflex.

[4] Op. cit.

I-2	U-2	U-2
motion	defeat	victory
incident	failure	success
group	remorse	triumph
people	fool	genius
occupation	shame	career
I-3	U-3	P-3
business	grief	courtship
meeting	funeral	wedding
stamps	corpse	lover
student	coffin	kiss
writing	undertaker	bridesmaid
I-4	U-4	P-4
hill	snake	sister
water	sweat	mother
woods	worms	family
stones	sores	parents
road	swill	home
I-5	U-5	P-5
swinging	sour	sweet
tall	stuffy	airy
bent	dirty	clean
twisted	bitter	juicy
wide	chilly	soft
I-6	U-6	P-6
occasion	disgrace	reward
change	guilt	achievement
individual	blame	praise
action	stupidity	skill
feeling	scorn	applause
I-7	U-7	P-7
cover	poverty	banquet
soap	burglar	dinner
basket	bankrupt	peaches
gravel	blindness	candy
jacknife	illness	blossom
I-8	U-8	P-8
kettle	filthy	darling
squirrel	pimples	romance
ribbon	ugly	cupid
sentence	brutal	bridegroom
measure	snobbish	honeymoon

The words were grouped in this way in order to produce a brief cumulative effect for each category.

The remaining equipment consisted of the KJ apparatus above referred to, a Hipp chronoscope, a voice key of the Dunlap type, and an exposure apparatus of the falling shutter type, which exposed the typewritten stimulus words and made an electric contact the instant the word was completely visible.

Method. The KJ technique is described in the article referred to. In the present case it was set to elicit the reflex 6 times in the 1 min. A signal

magnet was added to the recording unit. On the disc that carried the contacts controlling the brake an additional contact was added which electromagnetically tripped the exposure apparatus. This contact was adjustable in its position on the disc, but in the present case was set to expose the stimulus 2 sec. before the hammer struck the knee. Preliminary experiments indicated that this was a suitable interval for the present purpose. An auxiliary platform was erected in front of the *S* and slightly above the arms of the chair. On this rested the exposure apparatus slightly at the *S*'s left, illuminated by a drop light, and the voice key as close as possible to the *S*'s mouth without obscuring the view of the exposure apparatus. The exposure apparatus closed the circuit through the magnets of the chronoscope and the voice key broke it. The *E* sat at a table at the *S*'s left. On the table were the chronoscope and various switches. From his seat the *E* could likewise reach the rack holding the stimulus cards. A master switch on the *E*'s table enabled him to throw in the circuit through the control contact so that just prior to the next blow on the knee the word would be exposed. The same switch set the chronoscope circuit for action.

The course of a trial was as follows. The screen was set so that the field of the exposure apparatus was covered. A small card containing the stimulus word was placed in the rack behind this screen. The *E* watched the *S* and, very shortly after one KJ, threw the master switch and started the tuning fork of the chronoscope. This latter served as a warning for the *S*. The word then appeared at the proper time followed by the reflex and the *S*'s response to the word. Meanwhile the *E* by means of a switch operating the signal magnet marked on the kymograph in a dot-dash code directly above the record of the crucial reflex the number of the trial. The master switch was then opened while the response and time were recorded and the card was changed. It will be remembered that the reflex was being elicited automatically 6 times in the 1 min. and at certain of these the stimulus word was given. The stimuli were given as rapidly as the results could be recorded, two or three per min.

Prior to the series the *S* was placed in the chair and the hammer was adjusted so as to give an average KJ, one that could be recorded conveniently on the kymograph. Untrained *S*s were run at least 5 min. prior to the stimulus words in order to familiarize them with the apparatus.

Six *S*s took part in this preliminary series. Three were graduate students and three were elementary students,—all men. Two of the former, E and F, were trained, *i. e.*, had served extensively as *S*s in other KJ experiments. The experiments were performed at Ohio State University in the Spring of 1924.

General Results

The general results of the experiment are summarized in Table I. The 6 rows give results for the 6 *S*s.

TABLE I

Average knee-jerk following Indifferent (I), Unpleasant (U) and Pleasant (P) stimulus words

S	Millimeters			Percent of I			Difference ÷ P. E.	
	I	U	P	I	U	P	I-U	I-P
A	19.6	21.7	24.8	100	111	126	0.9	2.3
B	12.2	6.5	10.9	100	53	89	4.0	0.8
C	12.0	9.4	10.6	100	78	88	3.9	2.3
D	9.6	6.3	6.5	100	66	68	2.7	2.7
E	35.2	36.1	35.2	100	103	100	0.3	0.03
F	33.1	30.3	31.8	100	91	96	1.7	0.8
Av.				100	84	94		

The first three columns of figures give the average height in mm. of all the KJs to stimulus words of a given sort, indifferent, unpleasant and pleasant. These mm.-figures, however, do not represent the actual height of the kick, because the amplitude was geared down in order to fit the kymograph. Some of the *S*s obviously have a much greater kick in these results than do others. The next three columns of the Table are designed to make the results of the different *S*s comparable. The crucial point is the extent to which affective tone increases or decreases the KJ relative to conditions with no affective tone. The figures for U and P for each *S* are divided by the figure for I. These ratios appear in the middle block of the Table. The differences between U and I and between P and I may then be noted. In order to determine whether such differences are significant, their probable errors are computed. The ratios of difference to the probable errors of difference appear in the last two columns of the Table. For instance, *S* A had an average kick of 19.6 mm. on indifferent stimuli, of 21.7 on unpleasant stimuli, and of 24.8 on pleasant. Reducing these figures to ratios, the results for U are 111% of those for I, and those for P are 126% of those for I. The difference between U and I is 0.9 the probable error of difference, and that between I and P 2.3 the probable error of difference. The records for all the *S*s do not include the entire 120 words. Some did not have time. to finish the entire series. The discrepancy is, of course, taken care of in the probable error.

The most striking thing in the Table is the apparent depression of the KJ by unpleasant stimulus words. The 6 *S*s on U average 84% as much as on I. Four of the *S*s show differences of this sort and two the reverse, but the latter differences are small, and only 0.3 and 0.9 times the probable error of difference. The other four differences are more significant, especially those for *S*s B, C and D. These, by the way, are untrained *S*s. It may be possible that the reflex of the trained *S*s is less susceptible to the affective state, or it may be that such words aroused less affective tone in these *S*s.

With the pleasant words the results are not as clear. There is some slight evidence of decrease in the reflex. Four *S*s show slight differences of this sort. One *S* shows a considerable difference in the other direction.

It was noted that the general level of the KJ as indicated by the intermediate reactions on which word stimuli were not given (and which are not included in the Table) varied somewhat during the experiment. It is possible that the low or high record for a particular group of words may be due to its position in the series rather than to its intrinsic quality. In order to check this possibility, curves are plotted (see Fig. 1) for the three

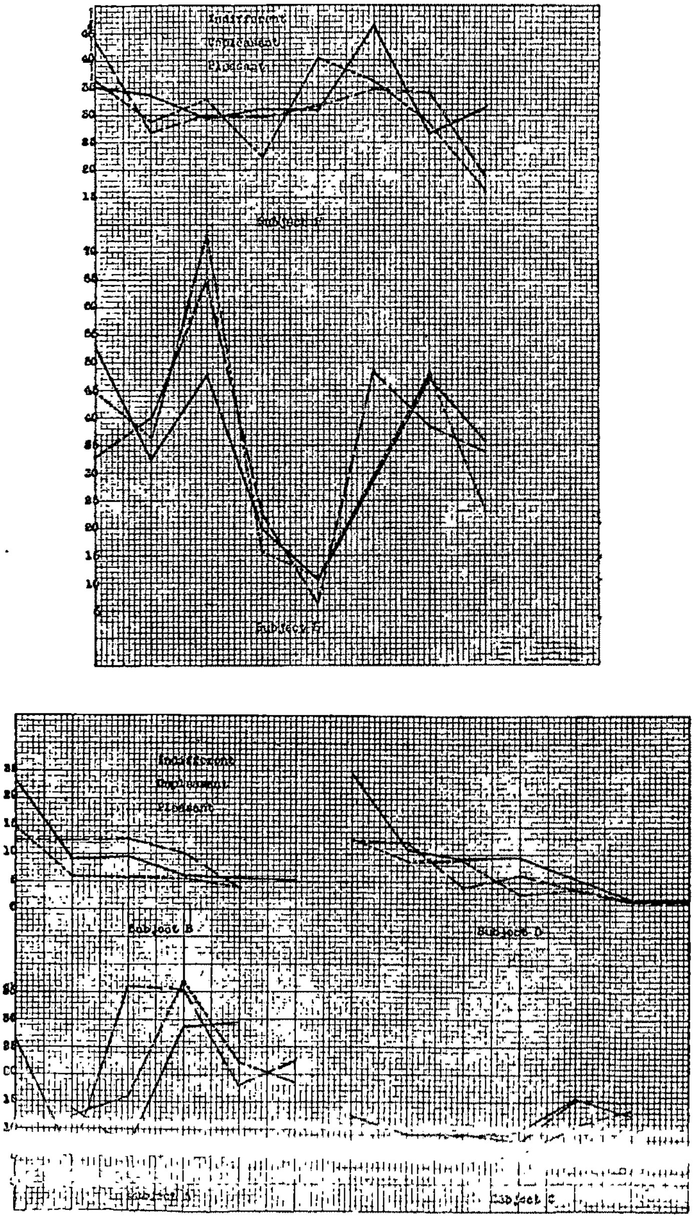


FIG. I

kinds of words, I, U and P, with the abscissa representing position in the series and the ordinate the average KJ for the 5 successive words. For the I curve that first ordinate is the average KJ for the 5 words denoted in the previous discussion of material as I-1, the second ordinate for this curve is the average of I-2, etc. Similarly, for the U curve, the first ordinate is the average KJ for the 5 words denoted U-1, the second ordinate the average of U-2, etc. The three successive groups I-1, U-1 and P-1 are plotted on the same ordinate; I-2, U-2 and P-2 are likewise plotted on the same ordinate. The three curves plotted in this way make possible the comparison of U or P at any point with the I that had very shortly preceded, and might be taken as an index of the normal KJ at that particular position in the series. In a few instances there is a break in some curve due to incomplete data. In these instances the available points on the curve are connected to bridge the gap. The solid line represents indifferent words, the broken line pleasant, and the dotted line unpleasant words.

The results of Fig. 1 tend to corroborate those of the preceding Table. With *S*s B, C and D the curve for U is rather consistently below that for I. F shows the same tendency to a lesser degree, E is equivocal, and A is rather consistently in the opposite direction. The curves for P are inconclusive. With D the P curve is rather consistently below the I curve, with A the reverse is true; but the others are rather equivocal. It would seem that the results shown in Table I are due to the actual effect of the stimulus words rather than to the position of the words in the series.

A further problem arises as to whether the differences shown are due to certain types of words more than others or merely represent the average effect. Table II is designed to bring out this point and the KJs are classified according to the nature of the stimulus words.

TABLE II

	Ratio of Unpleasant to Indifferent					Ratio of Pleasant to Indifferent				
S	Sense	Failure	Death	Disgust	Misfortune	Sense	Success	Love	Family	Food
A	76	90	236	130		62	35	501	124	
B	65		60	90		76	137	133	165	
C	67*	88*	111	56		82*	92*	98	81	
D	57*	90*	96	29	80	56*	100*	43	67	80
E	95*	108	153	72*	102	61*	119	96	113	82
F	116*	81*	111	61*	107	113*	78*	80*	95	128
Avg.	79	91	128	73	96	75	93	168	107	96

The ratio of each 5 unpleasant words of a given category is taken to the 5 indifferent words preceding it. This is equivalent to taking the two values from a given ordinate in the curves of Fig. 1 and finding the ratio of one to the other. In cases where

two groups of five words of the same category are available, the ratios for the two are averaged. Such cases are starred in the Table. Exactly the same procedure is followed with the pleasant stimulus words. For instance with *S* A, the ratio of the average KJ on the 5 unpleasant words dealing with sense qualities to the preceding 5 indifferent words is 76; the ratio of the average KJ for the 5 words dealing with failure to the preceding indifferent words is 90, etc.

With unpleasant words, the most pronounced decreases on the average are shown for sense qualities and disgust, with minor decreases for failure and misfortune and an increase for death. However, B shows his greatest decrease of all with death. Four other *S*s show their greatest decrease with disgust. We may conclude that in our results all the categories are in some instances effective in depressing the reflex, and that perhaps disgust is a trifle more effective in general than the other words. Whether this is due to the category or to the fact that these particular words provoked a more marked affective tone cannot be determined. The *S*s were questioned after the experiment as to the points that impressed them the most and whether they found some words more pleasant or unpleasant than others, but little of interest was forthcoming in their statements. None of the *S*s had had any training in introspection and instructions prior to the experiment to attend to affective tone were avoided because the effect of attention might introduce another variable and complicate the results.[5] The words with which the *S*s reacted indicated in most cases the normal association that might be expected. The stimulus words practically always called up something in the category intended by the *E*.

We may conclude, then, that unpleasant stimulus words in general have a slight but not universal tendency to decrease the patellar reflex.

With the pleasant words the case is not as clear. The most uniform category seems to be sense qualities, with 5 of the 6 *S*s showing decreases that average 25%. Love shows a considerable average increase, due, however, to the extraordinary results of one *S*. Two *S*s show their greatest decrease for success, two for sense qualities, one for love, and one for family. It appears that no one category is outstandingly potent in producing depression, but all are perhaps slightly effective in that way.

The association reaction time was recorded for each word except in a few instances where the chronoscope failed to work. The times for all the indifferent words for a given *S* were aver-

[5]W. W. Tuttle, The effect of attention or mental activity on the patellar tendon reflex, *Journ. Exper. Psychol.*, 7, 1924, 401-419.

aged, and likewise for unpleasant and pleasant. These averages are given in Table III.

The averages are in σ and the small number following each average is the number of cases. All the *S*s agree in a longer

TABLE III

	Average Association Reaction Time						Difference ÷ P. E.
S	I		U		P		I-U
A	659	28	824	25	733	29	3.3
B	558	25	576	21	548	16	0.9
C	877	28	1026	25	814	23	2.5
D	654	28	783	31	666	26	2.3
E	848	37	978	32	833	35	2.3
F	516	30	614	34	576	36	2.5
Avg.	685		800		695		

average reaction time for unpleasant stimuli. Three are quicker on the pleasant than on the indifferent, and three the reverse; and the averages of all *S*s for I and P are almost identical. The differences between I and U are evaluated from the standpoint of probable error of difference in the last column of the Table. While the ratio of difference to its probable error is small for most *S*s, the fact that all the *S*s are slower on U than on I deserves some consideration.

The finding corroborates the results of Tolman, who found that "the pleasant words as stimuli were not noticeably different from the indifferent words; but the unpleasant words showed a decided tendency to cause longer association times".[6]

We find, then, evidence of decrease in KJ and increase in association reaction time for unpleasant stimulus words. This correspondence suggests that the change in the KJ is due to the same factor as the change in the reaction time. To study this relation further the two variables were correlated, *i. e.*, a scatter plot was made for each *S*, plotting for each word the association time against the KJ. These correlations are given in Table IV.

TABLE IV

Correlation between KJ and Association Time

S	r	P. E.
A	—.11	.08
B	.08	.09
C	—.14	.07
D	—.29	.08
E	.08	.07
F	—.18	.07

[6]*Op. cit.*, 189.

The correlations are not large, but four of them are negative. The two positive ones are manifestly insignificant, and so is one of the negative. This is just a slight substantiation of the point that KJ and reaction changes are due to the same factor. Inasmuch as it has been elsewhere shown that the latter are due to affective tone of the stimulus words, it is probable that the KJ results are likewise due to this same affective factor. The conclusion is probably warranted that unpleasant stimulus words, those that appeal to simple sense qualities as well as those that deal with failure, disgust, death and misfortune, serve to produce a small amount of depression in the patellar tendon reflex.

Summary

A technique was available for eliciting the patellar tendon reflex periodically. Stimulus words—pleasant, unpleasant and indifferent—were presented visually just prior to certain elicitations of the reflex. The extent of the reflex and the association reaction time were noted.

There is a depression of the reflex on the average for unpleasant stimulus words amounting to 16%. There are slight and less consistent indications of a similar depression for pleasant stimulus words. All the unpleasant categories used were effective in some instances in producing depression.

The average association time for unpleasant stimuli is slower than for indifferent stimuli with all *S*s. The correlations between the extent of a reflex and the association reaction time for the stimulus word presented in connection with it are small, but all those that are of possible statistical significance are negative. This result suggests that the depression of the reflex and the slowing of the association time involve the same aspect of the stimuli, presumably the affective tone.

CONSTANCY OF ATTITUDE IN WEIGHT PERCEPTION

By Earl S. Rudisill, University of Pennsylvania

For some time the difference between process attitude and stimulus attitude in the judging of sensory qualities has been recognized. Titchener[1] made this distinction theoretically and others have subsequently taken cognizance of it.

The work of deLaski[2] and Gates[3] on dual impression on the skin indicated that such differences could be demonstrated experimentally.

Friedländer[4] made the assumption of two possible attitudes in weight discrimination, process attitude and stimulus attitude. Within the former he further differentiated experimentally pressure upon the fingers and kinaesthesis in the wrist. With these differences in mind a number of experiments were performed. But Friedländer's instructions were too indefinite and equivocal, and his statistical results were so equivocal as to be incomprehensible. The chief value of his work, so far as the present problem is concerned, lies in the fact that he has shown the possibility of adopting either process or stimulus attitude under direction of attention and of instruction.

Fernberger[5] carried out an investigation of the effects of process attitude and stimulus attitude in the judging of lifted weights. He had previously demonstrated the exceedingly complex character of such judgments.[6] "It has been found that pressure sensations on the tips of the fingers, kinaesthetic sensations localized in the hand, wrist and forearm might all be involved. In the early stages of practice the pressure criteria seem to be most used but, after considerable progressive practice, most *Os* tend to form their judgments on the basis of kinaesthetic criteria... Here, then, would be an excellent field in which to discover whether the judging under the stimulus attitude or the judging of some particular attributive process will have any effect upon the statistical limens."[7] Fernberger used three *Os* each of whom worked under each of three instructions. The first, instruction called for judgments with pressure on the finger-tips as the criterion; the second on the basis of kinaesthetic sensations in the wrist; and the third on the basis of the weights themselves (stimulus attitude). 1250 judgments were given by each *O* under each instruction. An introspection was taken for each 250 judgments. This study yielded quite as equivocal statistical results as that of Friedländer. The reports of the *Os* indicated that the instructions had been followed.

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[1]E. B. Titchener, *Experimental Psychology*, ii, 1905.

[2]E. de Laski, On Perceptive Forms below the Level of the Two-Point Limen, this Journal, 27, 1916, 569-571.

[3]E. J. Gates, The Determination of the Limens of Single and Dual Impressions by the Method of Constant Stimuli, *ibid.*, 26, 1915, 152-157.

[4]H. F. Friedländer, Die Wahrnehmung der Schwere, *Zts. f. Psych.*, 83, 1920, 129-210.

[5]S. W. Fernberger, An Experimental Study of the 'Stimulus Error,' *Jour. Exp. Psychol.*, 4, 1921, 63-76.

[6]S. W. Fernberger, An Introspective Analysis of the Process of Comparing, *Psych. Mon.*, 26, 1919, 161.

[7]S. W. Fernberger, An Experimental Study of the 'Stimulus Error', *Jour. Exp. Psych.*, 4, 1921, 64-65.

Reid[8], regarding the previous studies as inconclusive, carried out a lengthy investigation. He has reviewed the work of the two previous investigators and criticized both studies. Friedländer's work is first noticed. "We cannot consider Friedländer's work satisfactory as regards either his conditions, his statistical data, or his attempts at interpretation. He operates with different numbers of variables under the two *Einstellungen,* presents some variables less often than others, employs only one time order, alternates instructions from day to day, allows *O* to judge in terms of either weight, has but two *O*s, and finally, obtains only a small number of judgments (41) for every variable. Secondly, he publishes the data of only one *O*, and combines the judgments of different categories and for different variables. Finally, he offers the same explanation (the difficulty of the G-*Einstellung*) now for the result A>G and now for the result G>A,—even in the case of one and the same *O*." Reid then considers Fernberger's investigation. The latter's statement—"All of the earlier work in this field has been performed under the stimulus attitude — the *O*s have been instructed to judge the difference between the weights themselves"—is called in question, since Friedländer has used the process attitude. The validity of this criticism hinges upon the point at which one draws a line of demarcation between earlier and later work. Fernberger undoubtedly had in mind as earlier work that of Fechner and Urban, as well as his own earlier researches. The rate of lift in Fernberger's experiment is misstated as 72 strokes of the metronome per minute instead of 92. "Fernberger...works with only one time order; he does not keep the interval between double lifts constant; and he has but three *O*s. These, however, may be relatively unimportant matters; it is of far greater importance that Fernberger has not given any detailed discussion of the carriage of his instructions."[9] We may note in regard to the last criticism that while Fernberger does not print the actual introspections he does report the substance of them.

The assumption that instructions for the process attitude were actually fulfilled is questioned. "We remark...that in the case of so complex a perception as that of lifted weights the change from stimulus instruction to process instruction does not at all guarantee a change from common-sense attitude to psychological attitude on the part of the *O*. A highly trained and reliable *O* will, of course, do his best, with full awareness of what he is about, to effect the prescribed change of attitude; but the untrained *O* may very well shift his attention from weight to pressure pattern, as from one object to another, without any corresponding change of total attitude. It is therefore, altogether possible that the changes of instruction carried generically different meanings to Fernberger's trained *O* and to his other two *O*s."[10] It is further asserted that "verbal understanding of an instruction, on the part of the untrained *O*s, does not necessarily imply its assimilation and its realization in practice,—still less the steady maintenance of the correlated attitude."[11] This would undoubtedly be the case with an *O* who had undergone no psychological training; but no such *O* took part in the experiment. It is also partially true of an *O* who is unpractised in this particular kind of experimental work. But with progressive practice the condition is changed.

Reid[12], in his experimental investigation, used seven *O*s of different degrees of training, six of whom were graduate students and one an assis-

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[8] A. C. Reid, The Effect of Varied Instructions on the Perception of Lifted Weights, this JOURNAL, 35, 1924, 53-74.

[9] *Op. cit.*, 58.

[10] *Ibid.*, 58.

[11] *Ibid.*, 58.

[12] *Ibid.*, 59ff.

tant professor of psychology. He took 2500 observations under each instruction for each *O*, and 1000 as control under the initial instruction for the particular *O*. A characterization of the criteria of judgments was taken from each *O* at the close of each series. The space error was eliminated by means of a carrier bracket[13]. Titchener's modified form of Fechner's weights was employed. The standard weight was 700 gm., while the comparison weights ranged from 649 to 751 gm., in intervals of 25.5 gm. The *Os* lifted the weights with palm downward. The time of lifting was regulated by a metronome set to 60 beats per min., with the bell sounding on every fourth stroke. Thus the interval between double lifts was 4 sec. In order to render the performance automatic, a preliminary series of about 500 judgments was given. Reid employed three sets of instructions which definitely called for attitudes leading to judgments of the weights as material objects, of pressure patterns on the thumb and fingers, and of kinaesthetic patterns in the wrist.

Reid ranked the several series in descending order of excellence (on the basis of limens and precision) for each *O*. For convenience the relations are put in the form of mathematical formulas. To the left of this formula he indicates the expected practice effect according to the order of the series for each *O*, while to the right he presents a third formula expressing (in descending order) excellence in carriage of instructions, as indicated by the reports given by the *Os* upon the completion of each series.

As to progressive practice he discovered evidence for intra-serial effect in the case of one *O* in all three attitudes; for two other *Os* in the stimulus attitude series; and for one *O* in finger-tip pressure and stimulus attitude series. The series mentioned are only those in which the *Os* concerned worked with uncontaminated attitude. The assumption that contamination neutralizes or prevents practice effect is violated in the case of subject M in two uncontaminated series. Practice effect in homologous series is found for three *Os*. In the case of one *O* practice effect through heterogeneous series was found.

A study of tendency of judgment and time order shows that 6 out of 7 *Os* gave more correct judgments when the first time order was used, that is, when the standard stimulus was presented first. More 'greater' than 'lighter' judgments were given by each *O* for every series, except in the control series for one *O*. All *Os* but one gave more correct judgments when a heavier weight followed a lighter one.

Although much critical emphasis is placed on the disregarding of the doubtful judgments by earlier investigators, Reid was not able to find that anything correlated with them, except that they grouped themselves about the smallest differences.

The final statement of this author is to the effect that stimulus instruction yielded the most accurate results. Finger-tip pressure was next and the wrist patterns ranked third in precision.

Reid worked with two time orders and thus accomplished an approximate elimination of the time error, or at least a counter-balancing of the time errors in opposite directions. But while this questionable advantage was gained there was undoubtedly a tendency, due to the attendant opposition process, to break down or prevent the establishment of a stable image of the standard stimulus. There is, we believe, a distinct advantage in the direction of stabilization of judgments connected with the use of the first time order alone, that is, standard stimulus first. A single time order helps render automatic all the factors which are common to all three attitudes in lifting and judging. This condition furthers precision, for the *O* is thus more nearly free to concentrate attention on the attributive phase of process or upon stimulus as may be required. This contention is borne out by

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[13] E. B. Titchener, *Experimental Psychology*, ii, I, 1905, 116.

the fact that Reid secured more correct judgments under the first time order than under the second.

The reports which the *Os* were required to give upon the carriage of instructions were made upon the conclusion of a complete series under a given instruction. Thus they were reporting on fulfillment of instructions as long as a month after the actual experiment. Such reports can be regarded as little more than impressionistic reports on the *Einstellung* except for the work completed immediately before the dictation of these characterizations. It may be reasonable to suspect that what really happened was that, with the fading of memory as to the earlier fulfillment of instructions, the type of fulfillment which was experienced in the very last portion was ascribed to the entire series. While there is a fair correlation between fulfillment of instructions (according to the reports) and the effect of progressive practice, as well as with precision of judgment, for the reasons given above, the reports are not to be relied upon, it might seem, as truly indicative of contamination or purity of attitude.

In the light of these criticisms, it seemed worth while to repeat this experiment in order, by a more frequent instrospective check, better to determine constancy of attitude and also to check the equivocality of the statistical results of all the previous investigators.

The purpose of the present investigation, then, is further to study the possibility of adopting different attitudes in the judging of weight, to determine the statistical effects of varied instructions, to study introspectively the comparison consciousness as employed in differential weight perceptions, and primarily to investigate the possible constancy of attitude under such an experimental situation.

Procedure

We employed four *Os*[14]: M. K. MacDonald, M. A. (M), instructor in psychology; H. S. Oberly, Ph. D. (O), instructor in psychology; S. Sanderson, M. A. (S), instructor in psychology; and L. D. Stratton, Ph. D. (St), graduate student in psychology. O, S, and St were in about the same stage of practice, that described by Fernberger as the transition period between earlier and later practice.[15]. M was in a later stage of practice.

Hard rubber weights were used as stimuli.[16] The standard weight was 100 gm. It was presented with 7 comparison weights of 84, 88, 92, 96, 100, 104 and 108 gm. The greatest variation shown by any of these weights during the experimentation was but .2 gm, while the average variation was .16 gm. The deviation in the case of each weight was negative. The weights were arranged in a double series on a turning-top table. The order was such as to obviate any effect of sequence.[17] The *O* sat in a chair fitted with a padded arm. An upright screen was erected, with an aperture through which *O*'s right arm extended, palm downward. His hand was placed in position conveniently to lift the weights as the table was turned. The space error was eliminated in this way. The rate of lift was regulated

[14]I am greatly obliged to Professor Fernberger for the suggestion of this problem, as well as for his valued counsel and assistance in the preparation of this paper. Grateful acknowledgement is also due to the *Os* who made possible this study.

[15][illegible] *Jour. Exp. Psychol.*, 4, 1921, 74-75.

[16]S. W. Fernberger, A New Form of Stimuli for Lifted Weight Experiments, this JOURNAL, 31, 1920, 147-151.

[17]S. W. Fernberger, Interdependence of Judgments within the Series for the Method of Constant Stimuli, *Jour. Exp. Psychol.*, 3, 1920, 126-150.

by means of a metronome which was set at 92 strokes per min. The bell sounded at every fourth beat. A complete lift was accomplished in the time of 4 strokes. *E* sat at the table about 100° to the right of *O* and turned the table on every fourth beat of the metronome. In this manner the standard and the comparison weights were in turn placed directly under *O*'s palm. The process was continuous except that *O* was allowed a rest whenever he so indicated. Rests were taken from one to four times in a period of time required for 112 or 126 judgments. Judgments were taken in groups of 350, that is, 50 on each pair of weights. At the end of the 112th, 224th and 350th judgment introspections as to the criteria of judgment were written by *O*. We, therefore, have 18 introspective reports from each *O* under each instruction. The points at which introspections were taken divided each group of judgments into three parts, the end of any one of which was a convenient stopping place for any day's work. We shall hereinafter refer to 350 successive judgments as a *group*, the tripartite sections of a group as *divisions*, and all of the judgments under a specified instruction as a *series*. Either one or two divisions could be done at a sitting, that is 112 (or 126) or 224 (or 238) judgments. This was left to the discretion of the *O* and was decided on the basis of fitness for further work. Not more than 350 judgments in succession were taken under any one instruction. A similar number were than taken in another attitude, and so on. Each *O* had a preliminary series. A 'warming up' practice of at least 14 judgments preceded each period of work.

Each *O* made 2100 judgments under each of three instructions. The following instructions were given to the *O*s in the first series (A).

"Certain stimuli will be presented to you which will be lifted— in so far as the rhythm and manner of lifting is concerned— in accordance with your former instructions.

"You will report only on the intensity of the pressure sensations on the tips of the fingers. Attend to these pressure sensations only. Three categories of judgment are permitted—'greater', 'less', and 'equal'. A greater or a less judgment indicates that the intensity of the pressure sensations on the tips of the fingers aroused by the second stimulus was greater or less respectively than the intensity of the pressure sensations aroused by the first stimulus of the same pair. An equal judgemnt indicates that the intensities of the pressure sensations aroused by the two members of a pair were subjectively equal.

"If you fail to live up to these instructions, either with regard to the categories of judgment or with regard to the formation of the judgment on the basis of the intensity of the pressure sensations only, you will not give a judgment but will indicate the fact by reporting the word 'failure.'

"Intensity of pressure sensations in the above is to be taken the sense of mental process only."

The second series of judgments (B) was made under instructions which required judgment on the basis of the kinaesthetic sensations in the wrist. The instructions were identical with those for series A except that the words "intensity of kinaesthetic sensations localized in the wrist" were substituted for the words "intensity of pressure sensations on the tips of the fingers."

The instructions for series C were framed with the intention of inducing the stimulus attitude. They were as follows.

"Certain stimuli will be presented to you which will be lifted—in so far as the rhythim and the manner of lifting is concerned— in accordance with your former instructions.

"You are to take the stimulus attitude and are to judge the weights themselves. Three categories of judgment are permitted—'greater', 'less', and 'equal.' A greater or a less judgment indicates that the second weight was greater or less respectively than the first weight of the same pair. An

equal judgment indicates that the two stimuli of a pair were of equal weight.

"If you fail to live up to these instructions for any reason you will not give a judgment but will indicate the fact by reporting the word 'failure.' "

Following this work an introspective study of the process involved in comparing the members of the pairs of weights was carried out under each of the three instructions. After a preliminary practice at each experimental sitting the same instructions, which were previously used, were read to the *O*. He was asked to make judgment only on two pairs of weights successively and then to write an introspective report on the mental processes involved in making judgment on the second pair. An introspection or report was given on each of the 14 pairs by each *O* under each instruction. The conditions of lifting were the same as in the previous work.

Results

A record of all judgments, along with their distribution, is given in Tables I-IV. The observed frequencies of judgment for each *S* are found each in a table. All tables have a similar distribution. In the first columns are found the letters representing the different attitudes under which the results were obtained. The remaining columns are grouped in threes and give successively the frequencies of lighter, equal and heavier judgments for each of the seven comparison pairs. The frequencies for each group are found in a single row. In the three rows at the bottom of the table will be found the totals of frequency of judgment under each of the three attitudes.

In Tables V-VIII are presented the h-values, the limens (S_1 and S_2), the intervals of uncertainty, and the points of subjective equality for each *O* under each instruction. Table IX is an exhibit of the tendency of correct judgments and of the tendency of all judgments. In Table X we have recorded the relation between the qualitative and the quantitative results. The first column indicates relative fidelity to instructions (purity of attitude) in the several series. Highest excellence is indicated by the numeral 1 and lowest by 3. The letters refer to the several series and the work under them. B opposite 1 in the 'Purity of Attitude' column indicates that for the *O* designated on the margin the kinaesthetic instructions were best fulfilled. C opposite 1 in the 'h' column indicates that for the *S* concerned the greatest accuracy was achieved under the stimulus attitude. Under practice effect we show the relative effect of progressive practice under the several instructions, the greatest amount of such effect being set opposite 1. Table XI records the indices of significance of difference computed according to Boring's formula.[18]

The relative standing of the several *Os* may be determined by reference to Tables V-IX. *M* ranks third in precision of

[18]E. G. Boring, this JOURNAL, 27, 1916, 315ff; 28, 1917, 454ff.

judgment, with an average *h* of 0.086 and a range extending from 0.054 to 0.118. He is third also in smallness of the intervals of uncertainty (I. U.) with an average of 6.33 and a range from 2.65 to 11.17; this value is indicative of the degree of sensitivity. As to magnitude of the point of subjective equality (P. S. E.) he holds second place; the point is 95.32 and the variation ranges from 94.17 to 96.55. This indicates that only one other *O* was less influenced by the time error. As to the number of correct judgments M stands third with 3586 out of a total of 6300. He gave the smallest number of 'greater' judgments and the second largest number of 'less' and 'equal' decisions.

O has the second largest average *h*, 0.0961. The spread of these values extends from 0.079 to 0.135. He holds the same rank as to the I. U.'s with an average of 5.50; the figures vary from 4.39 to 6.76. In regard to the P. S. E.'s O's position is third. The average is 94.88 and the spread is from 93.96 to 96.44. He gave the second largest number of correct judgments, namely 3704; he occupies the same rank for total number of 'greater' judgments, while he stands third in number of 'less' and 'equal' decisions.

As to accuracy in comparison decisions S is in fourth position. His average *h* is 0.0753 while the variation extends from 0.040 to 0.132. As respects the I. U.'s he ranks fourth also; his average is 7.31 and the range is from 2.04 to 13.42. His mean P. S. E. places him in the same position; the extremes are 90.95 and 97.61. S gave the lowest number of correct judgments. He ranks third in number of 'greater' judgments with 2587, fourth on the 'less' judgments with 2248, and first on 'equal' comparisons with 1465.

St holds first place in every respect except in number of equal judgments. His average *h* is 0.1095 and the spread is from 0.089 to 0.153. The mean I. U. is 1.27 with a deviation from 92.68 to 97.42. He gave 4515 correct judgments, 3096 'greater,' 2957 'less' and 247 'equal' comparisons. He is in fourth place for equality judgments. While this measure is fourth as to number of comparison decisions it indicates highest sensitivity.

B. Purity of Attitude

In this section we present a detailed account of the carriage of instructions as revealed by the 18 introspections or reports given by each *O* under each instruction. A sampling of three characteristic reports taken at varying stages of the work accompanies the account. The numbers attached to the sample reports refer to divisions.

M ranks second among the *O*s as to purity of attitude. Under instruction A he judged in terms of the size of the area

stimulated on the finger-tips. This criterion remained constant throughout the experimentation after some initial practice. It is specifically mentioned in 15 out of 18 introspections. In the first and second divisions there was difficulty in ruling out the kinaesthesis in the forearm and in the fifth division stimulus attitude interfered to some extent. There was marked visual imagery of the arm, the hand, the fingers and the weights throughout. Low to fair assurance as to the correctness of judgments prevailed.

Observer M: Attitude A

2. "The method of judgment remains the same, *i. e.*, in terms of the size of the area stimulated on the finger-tips. If much of the finger-tip must touch the weight, then it is greater; if it may be lifted by simple contact of the fingers, judgment is less. I find it particularly difficult to keep a constant attitude and rule out kinaesthesis in the forearm muscles."

13. "Judgment is in terms of the area stimulated on the finger-tips as previously described under this instruction. The visual imagery is shifted forward from the wrist and forearm muscles to the fingers and hand and the weight itself. But the finger-tips stand out especially."

17. "Judgment is still in terms of amount of areal pressure on the finger-tips. The visual imagery of the fingers is marked and constantly present."

While working according to instruction B, M made his comparisons on the basis of kinaesthetic sensations in the wrist. Every one of the introspections cites this basis. In the second division weight and pressure attitudes interfered somewhat. Fatigue came on rapidly. There was visual imagery of wrist and forearm, especially in the latter half of the work. Clearness of kinaesthesis varied from sitting to sitting.

Attitude B

1. "Judgment in terms of kinaesthetic sensations of the wrist not so difficult as I had thought it would be when instructions were first read. I interpret wrist liberally as the cues seem to come largely from a muscle on the volar side of the forearm. Its contraction, judged in terms of fatness or leanness when contracted, seems largely visual. The muscle tires soon and gives no cues so I have to rest more frequently than under other instructions."

7. "Judgment on the basis of the kinaesthetic sensations in the wrist. But this morning these sensations seem to be rather vague and indistinct."

9. "This morning the wrist kinaesthesis was clear and the judgment on this basis seemed to be fairly accurate."

13. "Judgment in terms of kinaesthesis in forearm. The visual imagery shifts to the forearm but is not so constant as in some other cases. The arm seems to tire more quickly in this instruction,—perhaps because I am better aware of the tired muscle when paying attention to it. The failure was due to a lapse of attention."

[illegible] instruction C were made on the basis of weight itself. In all but one report (1) stimulus attitude is definitely claimed. In the first and seventh divisions it is stated that judgments were made on all cues. There was some contamination from kinaesthesis in the eighth divi-

sion. Visual images of hand and weight were experienced under this instruction. This instruction was easier of fulfillment than others.

Attitude C

1. "Judgment much easier with this instruction. Am able to form judgment on the basis of all clues which are present, instead of limiting attention and judgment to one particular one. All of this makes me feel how much the stimulus attitude must have entered under former instructions." (A, first group.)

9. "Judgment in terms of weight. Physical and mental condition not so good this morning, so assurance as to judgments is rather lower than usual. The failure reported was due to lapse of attention."

17. "Stimulus attitude—but feel fairly certain of the judgments although I find my mind wandering at times. One seems to be able to judge weight so much more easily. Visual imagery scantily present."

M maintained B-attitude in greatest purity, with C and A following in order. (These ranks are found in Table X.) Of the 54 divisions contamination is reported for only seven. This indicates 87% purity of attitude.

O holds the first place in rank order as to fulfillment of instructions. Pressure on the finger-tips was the criterion of judgment under instruction A. Every report asserts obedience to instructions. In the eighth division stimulus attitude intruded. In the tenth division instructions were fulfilled "to the best of my ability." No imagery is reported.

Observer O: Attitude A

1. "I noticed the difference in instructions which called for pressure at the finger-tips—whereas in the former weight experiment the judgment of weight lifted was requested—and I concentrated my attention on the tips of the fingers. For each standard weight I found a judgment of 'pressure' and when the comparison weight came, the difference in pressure seemed very apparent. The failures reported were due to slow reaction time—delay in giving the judgment of the preceding comparison weight which made the lifting of the next standard weight practically automatic."

14. "The pressure of the finger-tips on the weight was the basis on which my judgments were made. Nothing unusual seems to occur during the experimentation. I keep my attention on the pressure on the finger-tips and report verbally."

18. "This series moved along much faster than the series this morning. It seems that the practice and rest aided in determining the judgments based on the intensity of the pressure sensations on the tips of the fingers."

O judged on the basis of kinaesthesis in the wrist under instruction B. This criterion is claimed in every written statement. In division 1 there was some admixture of the pressure criterion and in division 7 stimulus attitude "attempted to persist." Visual imagery of wrist was experienced in division eleven. Judgment at first seemed more difficult than in A.

Attitude B

1. "At first it was very hard to get away from the pressure idea which had been gone through immediately before this. As I proceeded I

began to note what seemed to me to be a definite greater or less degree of kinaesthesis in the wrist, and there was generally some small deciding factor which turned what might have been doubtful into definite results. This factor was the muscular feel which I can best describe as being from the wrist up to the fingers."

10. "By forcing or concentrating upon the kinaesthetic sensations in the wrist I feel that I made comparatively accurate results during the division. The change to this attitude from the stimulus attitude which preceded was not so hard to accomplish as I anticipated."

17. "The attitude this time was for the purpose of judging the kinaesthetic sensations in the wrist—a standard and a comparison weight. Fatigue seems to enter—I am thinking entirely of wrist movement, and the feeling toward the end of the series is that of semi-numbness."

In following instruction C this *S* made comparisons of the weights themselves. This standard of judgment was asserted in every one of the 18 protocols. There is no hint of any kind of contamination. No imagery is reported. This instruction was easier to follow than the other two.

Attitude C

1. "The experiment today was relatively easy for me, for in thinking of the weights I had little difficulty in distinguishing what to me were greater or less than the comparison weight. I made judgments purely on the basis of the objective stimulus."

10. "The stimulus attitude is by far the easiest method of judging the weights and the series of eight (rounds of the table) was completed with scarcely a sign of fatigue. One failure, reported near the end of the division, was due to a lapse of attention."

17. "Concentration was very poor today, having spent three days reading examination papers. The judgments were made on the basis of the weights themselves. I cannot give a high degree of assurance for these results."

O obeyed instructions best under C. A and B follow in order. Interference by foreign criteria is reported for but 3 divisions out of the 54. Therefore, he maintained purity of attitude to the extent of 94%.

S ranks fourth as to purity of attitude. Judgments under A were accomplished by means of concentrating attention on the tips of the fingers, sometimes the tip of the thumb alone. This attitude is claimed for every division except the first. In divisions 7, 8, 13, 14, 15, 16 and 18 the size of the area of the finger or thumb stimulated was the criterion. Stimulus attitude intruded in 4, 5, 14 and 18. There was an admixture of kinaesthesis in 2, 3, 6, 12 and 17. Rate of lift played a part in 1. Maximal attention was required throughout in order to maintain the necessary attitude. Assurance was low in much of the work. There was often difficulty in isolating the pressure patterns. In 9, 10 and 14 some judgments would have been different under other instructions. This S had some difficulty in keeping the rhythm with the metronome. Some judgments of equality expressed only inability to discriminate greater or less rather than positive decisions of equal.

Observer S : Attitude A

1. "There is a decided degree of difference in assurance of judgments of less, and too in the case of greater judgments. At times the hand seems almost to spring up and my judgment is unhesitatingly less; at others the pull at the finger-tips is distinctly greater. I find that the memory image of the standard weight fades rapidly, and especially in the first few rounds. There I could detect no subjective difference in the pull."

12. "As far as I know I adhered to the conditions of confining judgments to the pressure sensations at the finger-tips. But there may have been a few in which there was present a combination of these sensations (at the base of the thumb and index finger) from squeezing the weight in grasping. These might be confused with an extension of the pressure sensations along the fingers. Confidence was no more increased than in other sittings. There were no failures."

18. "This session, more than any others, I feel that my judgments were colored by stimulus attitude. I had little confidence in my judgments, but made them as well as I could on the basis of the mental processes (pressure sensations of the tips of the fingers). Again I find that I confine them to those (sensations) from the thumb in almost every case, and that the intensity is estimated in terms of extent of skin stimulated."

When B-instruction was given S used as the criterion of comparison the intensity of kinaesthetic sensations in the wrist as far as possible. All introspections report this basis of judgment. In divisions 1 and 5 fatigue interfered. Fatigue was rapid but offered no interference elsewhere. Visual imagery of the moving wrist played a part in 5. In 6, 15, 17 and 18 stimulus attitude forced itself in somewhat. Decisions would have been different if another attitude had been used in 6, 7, 10, 11, 12 and 14. "Other fields" claimed attention in 16. It was difficult to rule out foreign attitudes. Maximal attention was required at all times. Isolation of kinaesthetic criteria was enhanced at times by the *O* imagining that he was looking at the wrist (eyes were closed). There was some difficulty in keeping the rhythm. A good number of equality judgments were negative.

Attitude B

1. "When my attention is to be directed to the kinaesthetic sensations at the wrist I find that judgment of greater or less becomes extremely difficult, first, because of the difficulty of attending solely to the wrist sensations (by imagining I was looking at the wrist I made it a little less difficult), and secondly because of the slight discriminable difference in the sensations. In the early part of the division I was much less sure of my judgments, and equal was given often as representing no subjective difference. Fatigue was more noticeable when attending to these sensations and must have influenced my judgments."

7. "By imagining that I was fixating on the wrist itself I found no trouble in keeping the attention on the kinaesthetic sensations of the wrist. Fatigue seemed to set in more rapidly under these conditions, but not enough to interfere with judgments. Difficulty of discrimination was marked by the large number of equal judgments. And a number of the other judgments would have been different if I had been able to use any other criteria than the kinaesthetic sensations of the wrist. There were no failures."

16. "Immediately I experienced difficulty in isolating from the host of other sensations present only those of kinaesthesis at the wrist. Accordingly there are a large number of subjectively equal judgments. It requires a maximum of attention at all times to make any sort of judgment, and at times I find myself in other fields so that I cannot always discriminate with any definiteness."

Under stimulus instruction (C) S made his comparisons on the basis of the stimulus itself. Fulfillment of instructions is definitely claimed in every report. In 2, 3 and 9 kinaesthesis intruded. Rate of lift played a part in 5 and 8. No imagery is reported. This attitude was easiest to maintain. Maximal attention was not, as a rule, required. Many equal judgments were negative.

Attitude C

1. "I felt less sure of my judgment today than in other sittings, although this time I directed my attention strictly to the stimulus itself, disregarding sensations at the finger-tips, wrists, etc. After lifting a distinctly heavier weight the standard seemed quite light, and the comparison following it I judged most of the time to be heavier. My assurance was of a low degree on all but the very lightest weight."

9. "This morning I was in fine fettle and was able to finish the sitting with only two rests as I fatigued more slowly. The stimulus attitude, I find, requires less concentrated attention than the other attitudes. The second set of results (after the first rest) contained, it seemed, more equals than others, and I should account for these by the increased intensity of sensation at the wrist from the effects of fatigue which made discrimination difficult. There was one failure, due to the fact that I lost track of whether a weight was the stimulus or comparison one, due to wandering of attention."

17. "I followed instructions and made judgments on the basis of the weights themselves. At times I found it difficult to discriminate between them as the image of the standard weight faded so rapidly (probably lapsing of attention); this time there were many equality judgments."

According to the characterizations of judgment 44 out of 54 divisions were carried out with uncontaminated instructions. S's purity of attitude may therefore be expressed as 82%. Carriage of instructions for this *O* was best in attitude C, second in B, and least complete in A.

St ranks third as to excellence in the carriage of instructions, having a percentage of 87, the same as that of M. Comparison decisions in attitude A were arrived at by means of finger-tip pressure criteria. Pressure standards are definitely reported for divisions 1, 2, 3, 4, 5, 6, 7, 8, 10, 13, 14, 15, 17, and 18. In 1, 2, 7, 8, 10, 15, 16, 17 and 18 we find statements which, according to one interpretation, indicate somewhat of a mixed attitude, partaking of pressure and a muscular phase. We were inclined to rank St low on obedience to instructions for this reason, but when the matter was called to his attention [illegible] insisted that he did not mean to infer that muscular sensations played any part in the judgments, and that the statements referred to were intended to define more carefully the pressure

experience which was focal. In the light of the *O*'s claims we have not graded him down in the percentile rating; but in view of the fact that M was a much more highly practised *O* than St, and had had a longer introspective training, we place St in third place as to excellence in the fulfillment of instructions.

Observer St: Attitude A

1. "The attempt made here was to attend to the sensation on the finger-tips. I tried to do this by judging the relative firmness with which I had to grasp the weights with my finger-tips to make them stay. It seemed that there was a difference in whether the weight was immediately in front of my hand or the least bit to the side."

4. "In judging from pressure on the finger-tips this pressure must be clearly differentiated from the pressure of the finger-tips on it; thus if the object is grasped more firmly in the one case than in the other the pressure sensation is greater. Pressure can best be judged if it is exerted exactly over the same surface from time to time, hence there is a noticeable difference when the weight is grasped with the hand in a different position.

16. "Judgments made from sensations on the finger-tips. Same difficulty was experienced here as formerly, *i. e.*, the manner of grasping has a great influence on the judgment; attempt was made to grasp them in as uniform a manner as possible. In some cases the assurance of judgment was very low."

With instruction B the *O* judged on the basis of kinaesthetic sensations in the wrist. This criterion is definitely claimed for all divisions except the second. Pressure sensations forced their way into 1, 3, 4, and 11. Totality of sensations intruded in 7, 8 and 15. There was no visual imagery. In some parts the weights were lifted high in order to clarify the kinaesthetic sensations.

Attitude B

3. "Particular attention was paid to any equality judgments. Seemed to have great difficulty today in judging wrist sensations, and excluding finger sensations."

9. "Kinaesthesis of wrist was used as a criterion. Difficult, but found by raising weights higher in the air each time the sensations became more clear and hence easier to judge. There did not appear to be nearly so much hang-over from stimulus to stimulus today."

17. "Sensations in the wrist were the criteria from which judgments were made. I worked with the wrist free, not allowing it to rest upon anything and I lifted the weights high in order to emphasize the wrist sensations. Toward the latter part I noticed a decreasing sensitivity to these sensations."

Judgments were based on the stimulus itself or the totality of sensation under instruction C. Stimulus attitude is specifically affirmed in every division except 8. No visual imagery is reported. No corruption of instructions is reported, unless summation of all sensations is to be so interpreted. But St's reports hardly justify us in so regarding it. High lifts helped to clarify judgments.

Attitude C

1. "A stimulus attitude was taken here. I take this to mean the summation of everything—is it heavier? There was a decided hang-over

here due to fatigue—the judgments on the whole, however, are easier to make than the others (A and B.)"

6. "There seemed to be somewhat of a carry-over from standard to comparison weight that was confusing here. This was not noticed until we had gone around the table two or three times. Generally speaking, judging from the stimulus attitude is much easier than from either of the others."

16. "Judgments made from the stimulus attitude. The blocks were lifted rather high and the judgments made from the totality of the sensations. Care was taken to have the hands as nearly as possible in the same position during the lifting of the standard and its comparison weight. Question here in my mind was always 'Is it heavier or lighter?'—not 'What are the different sensations?' "

A and C are without specific report of any admixture of foreign attitude. But since specific fulfillment of instructions is more frequently claimed under C we are rating this attitude first as to purity, with A and B following in order.

For the four *O*s combined there is an 87% purity of attitude. For several divisions in which the particular attitude is not specifically claimed it is, we believe, clearly implied, while particulars are dwelt upon. All regarded stimulus attitude as of greatest ease and the kinaesthetic most difficult. The disclaimers of contamination are really positive evidence for the fulfillment of instructions, since they express the ability of the *S*s concerned to distinguish between fulfillment and non-fulfillment of directions. While these percentile ratings by no means tell the entire truth, they are sufficiently indicative of the facts to reveal in a general way what happened. And although the contaminated divisions are but 13% of the entire amount of work, they really represent inconstancy of attitude to at least that extent.

C. The Relation of Qualitative and Quantitative Results

Table X is a record of the correlation of purity of attitude with statistical results. The results under the several instructions are ranked in order of preference with the highest excellence set opposite 1. The categories of highest preference are the greatest magnitude of *h* and P. S. E., the least value of the I. U., the least contamination of comparison attitude and the most pronounced effect of progressive practice. Practice effect is measured principally by varying degrees of progressive increase in accuracy (*h*) but also to some extent by the decreasing interval of uncertainty.

For M there is a full correspondence between purity of attitude and accuracy. As to I. U.'s (measures of sensitivity) the medium positions (2) are in harmony while the other two are [illegible] the remaining values are reversed; B's are better than C's and A's. As regards practice effect C's under *h* are better than A's, but the B's are reversed. The I. U.'s show no effect.

The *h* of O shows a complete correlation with fulfillment of instructions. The I. U's correspond in B-attitude. A and C reverse positions, but are better than the B's. For the P. S. E.'s the A's are better than the B's but the C's are in opposition to one another. Under practice effect the I. U.'s are of no significance while the *h*-values correspond in that the A's are better then the B's but the C's occupy the two opposite extremes.

The correspondence between attitude and precision for S is not complete. The C's occupy first position while the A's and B's oppose one another in the two lower places. C's are better than A's and B's. The case with the I. U.'s is identical. For the P. S. E.'s the A's are in harmony while the B's and C's are in opposition. B's and C's are better than the A's. There is a complete correspondence as to the practice effect in the *h*-values, while under the I. U.'s the A's are opposite extremes and C's are better than B's.

St's figures present a full correlation between attitude and P. S. E.'s. As to precision the C's occupy first place while the other two are in opposition. With the I. U.'s there is no correspondence except that the A's fall into mid-position. The C-attitude yielded the most pronounced practice effect (*h*), and is in harmony with attitude, but the other two attitudes are at variance. No practice effect is shown by the I. U.'s except in C.

Practice effect will be noticed in the next Section. There is a complete correlation between precision and fidelity to instructions in the case of two *O*s and a pronounced tendency in the same direction on the part of the other two. We discover a strong tendency toward correspondence between attitude purity and I. U.'s on the part of one *O*, a slight tendency in the case of another, while the other two are practically without tendency. There are weak tendencies toward agreement of attitude purity and decreased effect of time error in the case of three *O*s and a complete correlation for the fourth. There is found a full correspondence between purity of attitude and degree of practice effect, as shown by the *h*-values, for one *O*. There are marked tendencies in the same direction on the part of the other *O*s. The strong general tendency toward correspondence between precision and purity of attitude is in harmony with the findings of Reid.

D. The Effect of Progressive Practice

An examination of the effect of progressive practice reveals a complete correlation with purity of attitude in the case of S, and a pronounced tendency in the case of St, while weaker tendencies in that direction are discernible in the statistical results of M and O. The relative amounts of increase in the value

TABLE I—Record of Judgments

Observer M

Instruction	84 L	84 E	84 G	88 L	88 E	88 G	92 L	92 E	92 G	96 L	96 E	96 G	100 L	100 E	100 G	104 L	104 E	104 G	108 E	108 G
A	47	2	1	32	11	7	27	8	15	23	4	23	8	12	30	11	14	25	[illegible]	46
B	46	3	1	36	7	7	30	9	11	22	5	23	14	7	29	8	5	37	[illegible]	44
C	43	5	2	30	8	12	28	10	12	15	9	26	8	9	33	8	3	39	5	45
A	41	5	4	36	6	8	24	8	18	20	12	18	8	7	35	4	9	37	3	46
B	44	5	1	31	10	9	31	8	11	13	13	24	13	9	28	6	3	41	3	45
C	46	4	0	35	7	8	24	15	11	10	24	16	7	12	31	5	8	37	3	45
A	40	8	2	34	6	10	23	9	18	15	21	14	7	10	33	5	8	37	3	46
B	43	3	4	39	6	5	25	11	14	17	20	13	9	11	30	6	6	38	2	47
C	42	6	2	34	10	6	25	12	13	18	14	18	7	14	29	5	8	37	6	43
A	45	4	1	35	12	3	21	16	13	18	13	19	13	12	25	5	5	40	4	45
B	46	3	1	26	22	2	19	24	7	16	20	14	4	16	30	2	6	42	3	46
C	45	5	0	38	7	5	27	13	10	14	15	21	8	15	27	2	6	42	3	46
A	44	6	0	31	16	3	22	18	10	14	24	12	8	14	28	4	15	31	[illegible]	45
B	48	2	0	34	12	4	20	26	4	15	22	13	7	16	27	1	7	42	2	47
C	43	4	3	35	6	9	20	21	9	14	16	20	10	11	29	2	9	39	[illegible]	47
A	43	7	0	30	18	2	25	13	12	13	10	27	7	22	21	2	10	38	5	43
B	46	2	2	37	10	3	21	15	14	13	21	16	7	23	20	3	13	34	3	4
C	44	6	0	31	16	3	18	20	12	13	21	16	7	12	31	4	15	31	[illegible]	48
TOTALS A	260	32	8	198	69	33	142	72	86	103	84	113	51	77	172	31	61	208	22	[illegible]71
TOTALS B	273	18	9	203	67	30	146	93	61	96	101	103	54	82	164	31	46	223	[illegible]	68
TOTALS C	263	30	7	203	54	43	142	91	67	84	99	117	47	73	180	26	49	225	[illegible]	74

TABLE II

Observer O

Instruction	84 L	84 E	84 G	88 L	88 E	88 G	92 L	92 E	92 G	96 L	96 E	96 G	100 L	100 E	100 G	104 L	104 E	104 G	108 E	108 G
A	46	4	0	33	8	9	24	18	8	23	15	12	7	13	30	1	6	43	1	49
B	44	4	2	30	12	8	29	9	12	16	15	19	14	11	25	6	6	38	5	44
C	48	1	1	33	13	4	28	12	10	23	15	12	6	16	28	3	9	38	2	47
A	47	3	0	38	6	6	30	9	11	16	18	16	7	18	25	7	8	35	1	49
B	43	5	2	39	7	4	31	9	10	17	13	20	10	9	31	3	11	36	[illegible]	49
C	48	0	2	42	3	5	29	7	14	21	7	22	11	15	24	6	8	36	2	45
A	42	6	2	39	6	5	36	10	4	11	17	22	5	15	30	5	10	35	[illegible]	84
B	46	3	1	32	10	8	21	20	9	16	9	25	5	16	29	3	7	40	5	44
C	44	6	0	34	8	8	31	11	8	13	12	25	7	14	29	4	9	37	[illegible]	48
A	48	1	1	28	12	10	26	15	9	11	15	24	8	12	30	4	7	39	[illegible]	49
B	47	1	1	35	9	6	20	15	15	14	11	25	5	12	33	2	11	37	[illegible]	49
C	45	4	1	38	6	6	27	12	11	9	22	19	2	16	32	1	8	41	2	47
A	46	4	0	34	13	3	20	18	12	9	12	29	5	15	30	1	8	41	5	44
B	46	4	0	36	12	2	18	21	11	8	18	24	2	19	29	4	9	37	1	49
C	48	2	0	26	18	6	24	17	9	7	24	19	1	16	33	0	11	39	[illegible]	49
A	48	1	1	37	12	1	19	15	16	14	13	23	3	14	33	2	9	39	[illegible]	49
B	45	4	1	33	14	3	21	15	14	10	15	25	6	7	37	2	8	40	[illegible]	45
C	48	0	2	32	11	7	16	19	15	13	18	19	3	14	33	1	14	35	[illegible]	46
TOTALS A	277	19	4	209	57	34	155	85	60	84	90	126	35	87	178	20	48	232	[illegible]	88
TOTALS B	271	21	8	205	64	31	140	89	71	81	81	138	42	74	184	20	52	228	[illegible]	80
TOTALS C	281	13	6	205	59	36	155	78	67	86	98	116	30	91	179	15	59	226	3	282

TABLE III

Observer S

Instruction	84			88			92			96			100			104			108		
	L	E	G	L	E	G	L	E	G	L	E	G	L	E	G	L	E	G	L	E	G
A	38	10	2	33	10	7	28	11	11	11	15	24	5	22	23	6	7	37	2	7	41
B	38	10	2	26	16	8	28	15	7	14	19	17	11	14	25	8	15	27	2	13	35
C	37	8	5	35	9	6	24	13	13	11	16	23	2	18	30	2	8	40	3	3	44
A	35	8	7	27	16	7	14	25	11	13	19	18	6	23	21	5	21	24	5	7	38
B	35	9	6	23	12	15	21	[illegible]	20	16	13	21	11	10	29	7	16	27	4	11	35
C	42	2	6	31	6	13	28	11	11	11	12	27	11	12	27	3	16	31	0	5	45
A	42	7	1	28	13	9	19	18	13	16	15	19	9	21	20	9	8	33	5	3	42
B	33	13	4	28	15	7	15	15	20	10	20	20	12	16	22	5	16	29	2	7	41
C	46	2	2	40	6	4	26	13	11	13	16	21	6	10	34	6	7	37	0	1	49
A	37	12	1	33	14	4	21	20	9	12	15	23	6	12	32	1	17	32	3	7	40
B	37	10	3	34	10	6	18	19	13	14	15	21	6	18	26	4	10	36	3	6	41
C	47	2	1	35	11	4	28	13	9	20	[illegible]	21	10	10	30	5	11	34	3	1	46
A	39	11	0	29	11	10	17	20	13	14	23	13	6	18	26	6	12	32	0	8	42
B	36	12	2	36	6	8	29	16	5	21	12	17	10	13	27	13	12	25	5	6	39
C	45	5	0	39	8	3	26	15	9	12	14	24	11	11	28	3	6	41	2	2	47
A	39	8	3	30	14	6	22	17	11	17	11	22	4	13	33	6	9	35	4	10	36
B	41	7	2	32	14	4	24	13	13	16	21	13	12	11	27	5	14	31	0	4	46
C	48	1	1	36	8	6	26	10	14	19	15	16	9	10	31	1	8	41	0	1	49
TOTALS A	460	56	14	180	77	43	121	111	68	83	88	119	36	109	155	33	74	193	19	42	239
TOTALS B	220	61	19	179	73	48	135	87	78	91	100	109	62	82	156	42	83	175	16	47	237
TOTALS C	265	20	15	216	48	36	158	75	67	86	82	132	49	71	180	20	56	224	7	13	280

TABLE IV

Observer St

Instruction	84			88			92			96			100			104			108		
	L	E	G	L	E	G	L	E	G	L	E	G	L	E	G	L	E	G	L	E	G
A	47	0	3	43	0	7	25	1	24	21	0	29	8	0	42	4	1	45	2	0	48
B	2	6	41	41	1	8	25	2	23	17	0	33	10	3	37	3	1	46	1	1	48
C	45	0	5	38	4	8	25	2	23	12	5	33	5	4	41	1	3	46	1	1	48
A	48	0	2	46	0	4	35	0	15	19	1	30	9	0	41	5	0	45	1	0	49
B	49	0	1	47	0	3	32	0	18	23	2	25	8	12	30	3	2	45	1	1	48
C	48	0	2	45	3	2	40	1	9	23	4	23	13	17	20	8	3	39	0	2	48
A	48	2	0	43	1	6	41	0	9	25	2	23	11	12	27	5	0	45	1	0	49
B	48	0	2	43	0	7	42	0	8	24	2	24	8	2	40	3	2	46	0	0	50
C	50	0	0	43	0	7	40	1	9	24	4	22	9	15	26	5	1	44	1	0	49
A	47	1	2	48	0	2	36	1	13	23	1	26	12	8	30	7	0	43	4	0	46
B	49	0	1	42	2	6	32	0	18	30	0	20	7	11	32	4	1	45	5	0	45
C	50	0	0	47	0	3	48	0	2	31	2	17	7	18	25	3	1	46	1	1	48
A	50	0	0	45	0	5	41	0	9	25	0	25	4	14	32	6	0	44	1	0	49
B	50	0	0	46	0	4	36	1	13	24	0	26	8	8	34	1	0	49	1	0	49
C	48	1	1	43	0	7	33	0	17	31	0	19	3	9	38	2	0	48	2	0	48
A	48	0	2	43	0	7	36	0	14	23	0	27	7	16	27	1	1	48	2	1	47
B	50	0	0	41	0	9	43	0	7	29	0	21	3	15	32	2	0	48	1	0	49
C	49	0	1	41	0	9	42	0	8	23	0	27	2	12	35	2	0	48	2	0	48
TOTALS A	288	3	9	268	1	31	214	2	84	136	4	160	51	50	199	28	2	270	11	1	288
TOTALS B	288	2	10	260	3	37	210	3	87	147	4	149	44	51	205	16	5	279	9	2	289
TOTALS C	290	1	9	257	7	36	228	4	68	144	15	141	40	75	185	21	8	271	7	4	289

TABLE VIII
Observer St

Instruction	h_1	h_2	S_1	S_2	Interval of uncertainty	Point of subjective equality
A	0.101	0.099	93.96	94.12	0.16	94.04
B	0.092	0.089	93.01	93.76	0.75	93.32
C	0.109	0.103	92.06	93.63	1.57	92.68
A	0.116	0.119	95.08	95.16	0.08	95.11
B	0.126	0.150	95.04	95.50	0.46	95.29
C	0.105	0.097	96.35	98.66	2.31	97.41
A	0.113	0.108	95.22	97.18	1.96	96.13
B	0.125	0.124	95.45	98.19	2.74	96.81
C	0.109	0.101	95.77	97.34	1.57	96.53
A	0.098	0.099	96.15	97.19	1.04	96.67
B	0.094	0.092	95.63	96.71	1.08	96.16
C	0.153	0.110	96.85	98.22	1.37	97.42
A	0.125	0.105	95.79	98.92	3.13	97.22
B	0.132	0.105	95.25	95.96	0.71	95.56
C	0.113	0.110	95.16	95.74	0.58	95.45
A	0.109	0.096	94.89	96.48	1.59	95.67
B	0.126	0.109	95.59	96.51	0.92	96.00
C	0.121	0.110	95.06	95.91	0.85	95.45
TOTALS A	0.662	0.626	571.09	579.05	7.96	574.84
TOTALS B	0.695	0.624	569.97	576.63	6.66	573.14
TOTALS C	0.710	0.631	571.25	579.50	8.25	574.71
AVERAGES A	0.110	0.104	95.17	96.51	1.33	95.81
AVERAGES B	0.116	0.104	94.99	96.10	1.11	95.52
AVERAGES C	0.118	0.105	95.21	96.58	1.37	95.78

TABLE IX
Tendency of Judgment

Instruction	Correct Judgments: Order G-L	Correct Judgments: Order L-G	Correct Judgments: Totals	Entire Number of Judgments: Greater	Less	Equal
			Observer M			
A	703	478	1181	792	891	417
B	718	496	1214	858	815	427
C	692	499	1191	913	769	418
Total	2113	1473	3586	2563	2475	1262
			Observer O			
A	725	520	1245	922	780	398
B	696	508	1204	940	762	398
C	747	508	1255	912	777	411
Total	2168	1536	3704	2774	2319	1207
			Observer S			
A	614	432	1046	831	702	567
B	625	412	1037	822	745	533
C	725	504	1229	934	801	365
Total	1964	1348	3312	2587	2248	1465
			Observer St			
A	904	558	1462	1041	996	63
B	906	568	1474	1056	974	70
C	919	560	1479	999	987	114
Total	2729	1786	4415	3096	2957	247

TABLE X
Relation between Qualitative and Quantitative Results

	Purity of Attitude	h	I. U	P. S. E	Practice Effect: h	Practice Effect: I. U
M 1	B	B	A	B	C	—
2	C	C	C	A	A	—
3	A	A	B	C	B	—
O 1	C	C	A	A	A	—
2	A	A	C	B	B	—
3	B	B	B	C	C	—
S 1	C	C	C	B	C	A
2	B	A	A	C	B	C
3	A	B	B	A	A	B
St 1	C	C	B	C	C	C
2	A	B	A	A	B	—
3	B	A	C	B	A	—

TABLE XI
Indices of Significance of Difference

		Observer M			
I. U.,	A & B,	0.2641	P. S. E.,	A & B,	0.5254
I. U.,	A & C,	0.2893	P. S. E.,	A & C,	0.3242
I. U.,	B & C,	0.1233	P. S. E.,	B & C,	0.6786
h_1,	A & B,	0.7904	h_2,	A & B,	0.3340
h_1,	A & C,	0.0752	h_2,	A & C,	0.1656
h_1,	B & C,	0.6121	h_2,	B & C,	0.4739
		Observer O			
I. U.,	A & B,	0.8604	P. S. E.,	A & B,	0.0269
I. U.,	A & C,	0.0054	P. S. E.,	A & C,	0.2691
I. U.,	B & C,	0.5337	P. S. E.,	B & C,	0.2385
h_1,	A & B,	0.5337	h_2,	A & B,	0.2893
h_1,	A & C,	0.6515	h_2,	A & C,	0.7753
h_1,	B & C,	0.7371	h_2,	B & C,	0.8692
		Observer S			
I. U.,	A & B,	0.6515	P. S. E.,	A & B	0.5778
I. U.,	A & C,	0.9766	P. S. E.,	A & C,	0.6194
I. U.,	B & C,	1.0000	P. S. E.,	B & C,	0.1814
h_1,	A & B,	0.8373	h_2,	A & B,	0.1073
h_1,	A & C,	0.9996	h_2,	A & C,	0.6046
h_1,	B & C,	1.0000	h_2,	B & C,	0.6008
		Observer St			
I. U.,	A & B,	0.2843	P. S. E.,	A & B.,	0.3389
I. U.,	A & C,	0.0591	P. S. E.,	A & C,	0.0269
I. U.,	B & C,	0.4651	P. S. E.,	B & C,	0.2385
h_1,	A & C,	0.0215	h_2,	A & C,	0.4651
h_1,	B & C,	0.0464	h_2,	B & C,	0.1551

TABLE V—Principal Measures
Observer M

	Instruction	h_1	h_2	S_1	S_2	Interval of uncertainty	Point of subjective equality
	A	0.075	0.075	93.71	98.85	5.14	96.26
	B	0.074	0.117	94.93	97.58	2.65	96.55
	C	0.073	0.079	92.22	96.49	4.27	94.44
	A	0.085	0.077	92.34	96.80	4.46	94.46
	B	0.078	0.085	92.82	97.35	4.53	95.18
	C	0.099	0.053	91.99	98.24	6.25	94.17
	A	0.103	0.078	91.93	97.28	5.35	94.23
	B	0.087	0.085	91.80	97.99	6.19	95.44
	C	0.085	0.081	92.19	98.42	6.23	95.23
	A	0.076	0.104	92.58	98.37	5.79	95.93
	B	0.054	0.093	89.26	93.40	11.17	96.35
	C	0.088	0.118	91.32	97.87	6.55	95.56
	A	0.086	0.091	91.49	99.89	8.40	95.75
	B	0.103	0.115	91.87	98.98	7.11	95.62
	C	0.090	0.091	91.64	98.59	6.95	95.14
	A	0.088	0.082	91.35	98.63	7.28	94.86
	B	0.095	0.079	92.15	99.86	7.71	95.65
	C	0.087	0.090	91.01	98.92	7.91	95.03
TOTALS	A	0.514	0.507	553.40	589.82	36.42	571.49
	B	0.491	0.574	552.83	585.16	39.36	574.79
	C	0.522	0.512	551.37	588.53	38.16	569.57
AVERAGES	A	0.085	0.084	92.23	98.30	6.07	95.23
	B	0.082	0.095	92.13	97.51	6.56	95.80
	C	0.087	0.085	91.89	98.09	6.36	94.93

TABLE VI
Observer O

	Instruction	h_1	h_2	S_1	S_2	Interval of uncertainty	Point of subjective equality
	A	0.097	0.099	92.61	97.67	5.06	95.15
	B	0.092	0.079	92.80	98.39	5.59	95.37
	C	0.098	0.100	93.05	98.88	5.83	95.99
	A	0.095	0.086	93.67	98.93	5.26	96.17
	B	0.094	0.095	93.33	98.86	5.53	96.11
	C	0.094	0.081	94.79	98.34	3.55	96.44
	A	0.102	0.096	93.15	98.37	5.22	95.69
	B	0.098	0.086	91.64	97.64	6.00	95.53
	C	0.094	0.093	92.46	98.77	6.31	95.60
	A	0.089	0.090	91.57	97.01	5.44	94.35
	B	0.110	0.091	91.74	96.80	5.06	94.03
	C	0.135	0.099	92.04	96.43	4.39	94.32
	A	0.117	0.091	91.11	97.51	6.40	93.91
	B	0.108	0.101	91.28	97.90	6.62	94.47
	C	0.135	0.086	90.33	97.08	6.76	92.96
	A	0.108	0.102	92.16	97.04	4.88	94.53
	B	0.094	0.097	91.27	96.95	5.68	94.15
	C	0.119	0.082	90.89	96.41	5.52	93.14
TOTALS	A	0.608	0.564	554.27	586.53	32.26	569.79
	B	0.596	0.549	552.06	586.54	34.48	569.66
	C	0.675	0.541	553.56	585.91	32.36	568.45
AVERAGES	A	0.101	0.094	92.21	97.75	5.37	94.96
	B	0.099	0.091	92.01	97.75	5.74	94.94
	C	0.112	0.090	92.22	97.65	5.39	94.74

TABLE VII
Observer S

	Instruction	h_1	h_2	S_1	S_2	Interval of uncertainty	Point of subjective equality
	A	0.077	0.074	91.21	98.82	7.61	94.94
	B	0.062	0.061	91.02	101.64	10.62	96.28
	C	0.082	0.078	90.64	90.64	5.36	93.24
	A	0.057	0.052	88.24	101.66	13.42	94.85
	B	0.052	0.040	87.45	99.64	11.56	93.45
	C	0.083	0.060	91.74	97.66	5.82	94.21
	A	0.059	0.068	90.81	99.64	8.83	95.52
	B	0.057	0.058	88.12	99.41	11.29	93.82
	C	0.110	0.097	93.05	97.50	4.45	95.14
	A	0.082	0.078	90.50	98.83	8.33	94.65
	B	0.073	0.076	90.39	98.69	8.30	94.63
	C	0.081	0.113	95.08	98.45	3.37	97.04
	A	0.071	0.066	89.97	92.01	2.04	90.95
	B	0.056	0.066	93.27	101.13	7.86	97.61
	C	0.098	0.090	92.86	96.58	3.72	94.63
	A	0.068	0.068	90.91	98.94	7.53	94.92
	B	0.073	0.082	91.99	99.50	7.51	95.31
	C	0.111	0.132	93.06	97.12	4.06	95.27
TOTALS	A	0.414	0.406	551.64	589.90	47.76	565.74
	B	0.373	0.383	552.24	599.38	57.14	571.10
	C	0.565	0.570	556.43	583.26	26.83	569.53
AVERAGES	A	0.069	0.068	91.94	98.31	7.96	94.29
	B	0.062	0.064	92.04	99.89	9.52	95.18
	C	0.094	0.095	92.74	97.21	4.47	94.92

of h in each of the three series are the sole determinants of practice effect considered here. An increase of such effect is found in every attitude and in the case of every *O*. It is least noticeable in the h_2 for St under instruction A. This fact stands in strong contrast to the results of Reid, in which intra-serial practice effect was discernible, with one exception, only in the cases of uncontaminated instructions. In a sense this result is in harmony with our correlation of purest attitude with greatest practice effect. But a wide discrepancy remains to be explained. In Reid's experiment all work under one instruction was completed before a new instruction was given, while in our investigation any group under any given instruction was followed by a group under another instruction. Between the taking of any two groups of judgments under any one instruction, there intervened 700 judgments under the two other attitudes. The conditions of our experiment do not, then, permit of the isolation of inter-serial practice effect. If there is inter-serial effect, it is mingled with the intra-serial influence. Without a doubt there are sufficient common factors in the three attitudes to secure that, with two heterogeneous groups interspersed between two homogeneous groups, the summation of inter-serial practice effect would show itself, added to the intra-serial effect, in the third group. It is natural, then, that we should find a larger amount of the progressive effect of practice in our own results. But in consideration of the relatively small amount of such influence found by Reid it is extremely doubtful whether these facts completely account for the discrepancy. In Reid's experiment the greatest degree of practice effect was discernible under the initial instruction. Within limits the same condition prevails in our own work, and is naturally to be expected. Our I. U's furnished no clue to the solution of this problem, since they either showed no effect whatever or were in inverted order. It should also be noted that Reid worked with both time orders while we worked with only the first time order,—a situation which tends to definitize the standard stimulus,—and these facts may also have some influence on the discrepancies of the results.

E. Failure Judgments

Account of failure judgments was taken with M and S after the first 2100 judgments had been given, and with O and St after 1050.

M reported 22 failures. Lapse of attention was given as the [illegible] except in one case, when it was mechanical. O gave 9 failure judgments. He accounted for 7 by [illegible] 1 by distraction upon opening his eyes when the memory of the standard weight failed, and 1 by confusion at the beginning of a division on account of the bell ringing on the odd count. S

had 15 failures. He explained 11 by mechanical interference or failure to keep the rhythm, 2 by lapse of attention, and 2 by distraction from noise in the adjoining room. St gave no failure judgment throughout the entire experiment.

Failure judgments are recorded for every comparison pair, but the weights of 88, 92 and 96 gm predominate. 25 were given under A-attitude, 6 under B, and 15 under C. These failures of judgment can not, so far as we can determine, be significantly correlated with any other facts. According to our instructions these failures were eliminated from the reports and calculations. They are so few in number that such an exclusion would hardly have any significant effect on the statistical results.

F. Significance of Difference

Table XI gives the indices of significance of difference between the several attitudes. These values are calculated for both *h*'s, for the I. U. and for the P. S. E. St, the *O* who ranks first in precision, has the lowest average significance of difference, while S, whose accuracy is fourth in order, has the largest average index. For O, 7 cases out of 12 show indices of significance of over .5000; M shows 4; S has 10 such; St has only 1. These results are exceedingly equivocal when one considers the values of each *S*, or when one compares the values from *O* to *O*. In a very few cases are the values great enough to be of any significance.

G. Tendency of Judgment

Every *O* gave more greater than less judgments. The difference amounts to 461 for O while for M it is but 88. St gave the smallest number of equality judgments. The highest number of such decisions was given by S. These figures furnish a direct check upon the measure of sensitivity. The *O* with the highest precision has the smallest number of 'equal' decisions to his credit, and conversely. 4515 correct judgments were given by St; 3704 by *O*; 3586 by M; and 3312 by S. The complete figures for tendency of judgment are found in Table IX. In the first column of this table are given the letters indicating the instructions under which the *O*s worked. In the second column, headed G-L, are given the totals of correct judgments when a lighter weight followed a greater. In the third column are similar totals of correct judgments when a greater weight followed a lighter. The totals of correct judgments, irrespective of the order of weights, are given in the fourth column. In the next three columns are given the number of judgments of greater, less and equal respectively.

H. The Comparison Consciousness

We present here a detailed account of the comparison consciousness as revealed by a series of introspections or reports.

Each of the four *Os* judged each pair of weights under each of the three instructions and immediately wrote a descriptive account of the criteria of judgment. This study was made after we had obtained the statistical results, with the reports already discussed, and was performed in an effort to check further and more minutely the constancy and purity with which the *Os* maintained the three attitudes. Judgment was made on the pair immediately preceding the pair of weights for which report was desired, but the processes involved in the second judgment only were reported. In this way the conditions for the study were rendered identical with those in the earlier part of the investigation. Each *O* gave 14 reports under each instruction, or 42 for the three attitudes. Three sample characterizations, taken from various stages of the work, accompany the account.

M in attitude A reports judgment made on the amount of area stimulated on the finger-tips for 8 pairs; that is, when the area for the comparison weight was less than for the standard the judgment was less. In three of these it is definitely stated that the amount of the area was held in mind visually (in one case pressure translated into area and compared). In the second judgment finger-tip pressure was thought of after the first pair. Three judgments showed scant consciousness and were on the meaning level. Vague kinaesthesis was hard to rule out in one case. Eight times marked visual imagery was reported. Kinaesthesis was a partial criterion in one decision. For two pairs pressure was so clear that it seemed *O* was living in the finger-tips.

Observer M: Attitude A

84-Less. "The lifting of the first pair of weights prepares me for the second. In lifting the first of the second pair of weights I try to get and hold in mind visually the amount of area stimulated on the end of the fingers, for this is the basis of judgment. In this case I find the area stimulated by the second weight of the pair to be less and so judged less."

88-Less. "Pressure patterns in second of the pair had less area, so less intense, so judgment less. Visual imagery marked. Whole thing mechanized. Scant consciousness. No consciousness of the stimulus attitude."

108-Greater. "The first weight of the introspective pair gave distinctly greater pressure pattern than the comparison weight preceding it, but the comparison weight of the introspective pair gave a still greater area of pressure on the finger-tips, so judged greater."

In the B-attitude (kinaesthesis in wrist) the *O* declared in the first report that he no longer used the old cues (pressure) and a new set had to be built up. In 7 cases he visualized the contraction of the forearm muscle; if it became fatter he judged greater, if less the judgment [illegible] there was visual imagery of the hand, table and weight. Imagery of the muscle appeared in 7 judgments. Seven times more kinaesthesis is mentioned. Once the judgment was on the [illegible]

the muscle pressing on the chair. The last judgment was reported as mechanized and immediate.

Attitude B

104-Less. "Judgment in terms of the muscle size taken in visual terms. The visual imagery of the table, arm and especially of one of the muscles in the forearm is marked. I feel it pull together and see its size and shape in imagery. The discrimination by this method seems much more crude than by other methods."

84-Less. "With the first weight of the introspective pair there is a definite pull in the forearm muscles,—with the second weight hardly any pull at all,—so I judge less. Visual imagery present but not focal."

100-Greater. "The judgment is getting mechanized so that more muscle sensation on second stimulus is immediately greater in report. Visual imagery, especially of the muscle—its size and shape in contraction."

In the C-attitude the judgments were definitely on weight for 5 pairs. Visual imagery of the hand, table and weights appeared in 10 comparisons. Kinaesthesis and pressure entered into 8 reports, kinaesthesis alone in two (played main part in three), and pressure alone in three. Eight reports show ease or difficulty of lift as a criterion, mingled with other criteria. Attitude was passive in two cases. In one judgment any helpful cues were used. Once a complex basis is reported. In one the judgment was checked with kinaesthesis. Once the *O* saw the muscle contract visually.

Attitude C

96-Greater. "Under this instruction I judge weight, using any cues that seem helpful. This judgment was made on a combination of finger-tip pressure and kinaesthesis in the forearm. There was more of these in the second weight, so I judge greater."

104-Less. "Judgment of less arrived at through finger-tip pressure and weight (weight meaning 'does it come up from the table or not'). This time the difference between the weights was small but enough for the judgment. Visual imagery not marked."

88-Equal. "I make these judgments in a passive way—just sit back and lift the weights and then decide which was heavier. In this case, however, I am sure that the judgment was largely in terms of finger-tip pressure; the two pressures being equal, I judged equal."

For O, the A-attitude yielded 13 cases in which the pressure criterion is reported, but there was an admixture of kinaesthesis in 8 comparisons. An effort to make an equal lift for the two weights is once reported.

Observer O: Attitude A

84-Less. "Less undoubtedly. The standard pulled on all of my fingers, and when I lifted the comparison weight the pressure I put on it made (seemed to make) the comparison weight much lighter."

96-Greater. "The pressure was greater—more of a pull at the tips of the fingers. By pull I mean that weight drew down the fingers and I naturally grasped hardly."

108-Greater. "Greater, the weight of the comparison stimulus made it necessary for me to grasp it tighter, and as a result the pressure was much greater. No doubt in my mind about this being greater."

Under instruction B this *S* reported kinaesthetic attitude in 13 comparisons, 9 of which refer to wrist muscles. Rate of movement intruded once. Kinaesthesis was especially clear in two cases. High assurance is reported in three decisions and low in one judgment.

Attitude B

84-Less. "The kinaesthetic sensation was less. The thing I think about is the wrist movement, and there was not so strong a sensation produced while lifting the comparison weight."

108-Greater. "Greater. The kinaesthesis is more clear to me as I progress. I have forgotten about correctness of judgments, *i. e.*, wondering whether I am right or wrong. The kinaesthetic sensation begins to stand out."

108-Greater. "Greater—4 to 1 on it. The pull here seems vivid. I visualize the wrist performing more work. Judgment based on that feeling of kinaesthesis in the wrist."

Under the C-attitude the *O* reports that he judged weight itself in all but one case. In 3 cases other sensations were said not to be in mind, and in two others it is definitely claimed that the judgments were not on kinaesthesis or pressure. There were two cases of exceptionally high assurance. Once he speaks of the stimulus attitude as the common attitude. Visual imagery is reported but once.

Attitude C

104-Greater. "Greater—more weight involved and a pull felt as I lifted the weight. The stimulus, *i. e.*, the weight is thought of—and my judgment is made on that basis."

100-Greater. "The judgment of the first comparison weight was greater, the second pair less. The weights themselves determine this, rather than any pull or pressure in hand or fingers. It seems that this is the easiest method of judging weights."

96-Equal. "I am not aware of the pressure or kinaesthetic sensations as I am under other conditions. I feel that the judgment is made on the weight itself."

S, while working under the pressure instructions (A), definitely claimed pressure sensations as the criterion in 9 cases. There was visual imagery of the hands lifting weights, etc., in 6 comparisons. Attention was given to the thumb chiefly in 8 judgments. In four cases it was difficult to rule out stimulus attitude. Kinaesthesis entered into two judgments. There was expectancy for less in three cases. Two times it is reported that the memory image faded rapidly. Once the *S* imaged the finger-tips. There was trouble with the rhythm once. Vocimotor images of the judgment word were experienced twice. Once there was unlocalized strain.

92-Less. "I judged the first or practice weight as less, and wondered [illegible] result had been different from before. There seemed then to be an expectancy of less for the next pair. I had a visual [illegible], it is true, of the lifting thumb, in particular, depressed because of

lifting the weight. Accompanying it was a feeling of cold. The intensity of the pressure sensations seemed less."

104-Equal. "The memory image of the standard weight—not particularly intense under this attitude—fades rapidly, and this time I could not distinguish the image of the pressure sensations of the comparison weight from it. I felt doubtful, but as I could not distinguish the intensities, one from the other, I reported equal."

96-Less. "I seem to have a tendency to judge the sensation resulting from the lifting of the second weight as less, although at the time of lifting the first it seemed to me that the intensity was not great. Yet clearly the sensations of pressure, this time, again, chiefly in the thumb, but partially in the other fingers, did seem of less intensity. Certainly, for me the memory image under this attitude fades rapidly."

Under instruction B this *O* reports kinaesthetic attitude in 13 comparisons, in 6 of which the sensations spread over the hand, wrist and back of the hand. The stimulus attitude entered into three decisions. Twice there was a negative equality decision. There was a vocimotor image of the judgment word in one case. Once expectancy of a greater or less is mentioned. In connection with four pairs there was trouble to keep the rhythm. In three comparisons there was visual imagery of the hand lifting. Pressure intruded one time.

Attitude B

84-Less. "I have always found it difficult in these judgments to entirely eliminate the stimulus attitude, but here as an aid I deliberately endeavored with some success to image visually my hand only, *i. e.*, without the weight, in the process of lifting. Then with attention fixed,—and it is as if I focussed my eyes on the wrist itself,—on the intensity of the kinaesthetic sensations, I judged. The pattern is spread over the upper surface of the wrist, and in particular intensity over the back of the hand. That is, the lesser extent of the sensations, localized in the wrist, arising from the second stimulus, gave rise to the verbal report of less."

92-Less. "The kinaesthetic sensations were localized this time quite clearly in the wrist, extending, however, in the second case but slightly up the hand. And as I base intensity here rather on extensity, I reported less."

100-Equal. "This time I lifted more slowly, apparently increasing the intensity and duration of the kinaesthetic sensations. All four of the weights lifted apparently aroused equal sensations, and I gave an equal report. I wondered at the time whether I was following the rhythm in lifting the comparison stimulus."

While judging in the stimulus attitude (C) S reports the use of definite weight attention in 5 cases. Kinaesthesis entered in 6 cases. The hand was visualized in 6 judgments. Once there was a positive equality decision. A vocimotor image of the judgment word was experienced three times. There were 7 cases of rhythm trouble. Rate of movement influenced one comparison, and the 'general feel' decided in one.

Attitude C

104-Greater. "The time or rhythm of lifting seemed to be increased. I was hurried, in fact, I neglected to give judgment of the first pair because I was concentrating on the second. I visualized my hand in the down motion, and in lifting the standard weight. Then I hurried to the compar-

ison, not too sure of the rhythm, and immediately had a vocimotor image of 'greater', which was the judgment given."

88-Less. "I was wondering whether the second pair would be as easy to discriminate as the practice pair, when suddenly I found that I had lifted the stimulus weight, and visualized myself replacing it. The comparison weight seemed to 'jump up' in comparison, and I called it less."

108-Greater. "In assuming this attitude I had a visual image of lifting the weights, and, in fact, find such a visualization a distinct aid in keeping the stimulus attitude. Relatively the standard weight felt heavy, *i. e.*, my hand came up with more effort, more feeling of strain than with a less judgment. Then I seemed to be set for a less judgment, but the comparison weight this time felt heavier and I gave the judgment greater."

St, working in the A-attitude, definitely claims pressure criteria in 12 protocols. In four decisions of this number other sensations are said to have been ruled out. Kinaesthesis entered in two cases. For 6 judgments the weights were barely lifted from the table.

Observer St: Attitude A

84-Less. "Pressure sensations on the tips of fingers attended to. Question here was how much pressure was experienced on the finger-tips as compared with the standard."

88-Less. "When holding the block with sufficient firmness to keep it from falling the pressure on finger-tips was greater. Care must be taken not to squeeze as I believe any results could be obtained if the degree of firmness is not more or less calibrated."

100-Greater. "Pressure sensations on the finger-tips were more intense than in the standard. Barely lifted clear of the table. Other sensations ruled out."'

In the kinaesthetic attitude (B) the *O* claims kinaesthesis in the wrist as the basis of judgment in every judgment. In 8 of them a high lift brought out the kinaesthetic sensations and in one it is said to have eliminated the other sensations. Once there was difficulty in ruling out other sensations. In two cases a slower lift helped.

Attitude B

84-Less. "Sensations localized in the wrist less than in standard. Weights were lifted high with a large movement of the wrist—this seems to accentuate those sensations and repress others.

92-Less. "Weights not lifted so high. Judgment much harder as it was harder to rule out other sensations, however, these were ruled out as much as possible and judgment made."

104-Greater. "I lifted more rapidly and the judgment made was heavier,—made from intensity of sensations localized in the wrist."

S claims that under attitude C the criterion of judgment was the totality of sensations in 10 comparisons, but in 8 of these he states that it is not an additive affair, but a unit. Three reports say that the answer is to the question "Is it [illegible]?" Two claims are made for an immediate rather than an analytic judgment. In two cases local sensations interfered. General feeling is given as the basis in two comparisons, and the fact that the weight came up easier was the criterion in two others.

Attitude C

84-Less. "I lifted the standard weight and compared the totality of sensations from it with the comparison weight. Here everything was fused together in general judgment, not sensations of finger-tips + wrist + etc., but the totality."

108-Greater. "As soon as the weight is lifted the judgment comes into mind. No attempt at analysis. It seems to me that this is the way judgments are usually made."

100-Equal. "The judgment here was made by a process of inference. The previous one was much lighter and there was so little difference that I thought the table had not been moved; the totality of sensations seemed identical."

No one of our *O*s was able completely to follow instruction A. Even when one judgment only was made, kinaesthetic sensations intruded to a greater or less degree for every *O*. Visual images were prominent in the comparisons of M and S. The thumb was prominent for S, and he was troubled by failure to keep the rhythm as well as by the rapid fading of the memory image. All seem to have used pressure as the main criterion, but not as the sole basis. Areal pressure determined the issue for M and S..

For the kinaesthetic attitude also there is contamination reported by every *O*, though it is not the same in every case. All used the kinaesthetic basis primarily. Pressure interfered for M and St. Rate of movement influenced the decisions of O. Stimulus attitude intruded into the judgments of S. Visual imagery was prominent in the judgment processes of M and S, M visualizing the contraction of a forearm muscle. S again had trouble with the rhythm.

Even stimulus attitude, by some designated as the common attitude of judgment, was kept pure by but one *S*. All used plain weight as the principal basis of decision. M and S used kinaesthetic sensations to some extent. M used pressure also. Unspecified local sensations interfered in the case of St. He used totality of sensations (a unit) as the main criterion, identifying the totality with weight itself. Visual imagery entered into the decisions of M, S and O. O alone reports no contamination. The complex nature of the comparison consciousness in this attitude as revealed by the characterizations corresponds in a general way to the findings of Fernberger.[19]

The results of our specific study of the comparison consciousness correspond, with few exceptions, to the information gained in the earlier part of the present experiment. In attitude A O reported no contamination in the earlier reports, while M and S reported admixture of stimulus attitude. In B-attitude there is but one noteworthy exception, the influence of rate of

[19] S. W. Fernberger, An Introspective Analysis of the Process of Comparing, *Psych. Mon.* 26, 1919, 95-103.

movement for O. Under the stimulus attitude there are several exceptions. St reported no contamination in the earlier reports while M was influenced by fingertip pressure.

It is of interest, at this point, to make comparison of our accounts of the comparison consciousness, under the several instructions, with the tentative analysis offered by Reid, as well as to check the reports of his *O*s with that analysis. Under the stimulus attitude he believes that an *O* may judge either in terms of projected pressure or projected kinaesthesis. His *O*s reported, under this instruction, the following criteria of judgment: weight (something to lift), kinaesthesis, pressure and rate of movement. While there is no mention of the projection of sensations in the reports (even that of Reid as *O*) his analysis is probably justifiable to some extent, since an objective criterion is involved. The reports of our *O*s claim as criteria weight, kinaesthesis, pressure and rate of movement; but in most cases it is sheer weight, with a number of assertions that sensations were not involved. Certainly the suggested analysis cannot cover nearly all cases. We are compelled by our protocols to add at least one other category, namely that of weight without reference to sensation. This "sheer weight" without reference to sensation is undoubtedly the every-day, man-of-the-street meaning level. It is a process in which meaning maintains such a large place in consciousness that the specific and minute sensations underlying such meanings are insignificant as compared to the meanings themselves.

It is suggested by Reid that under the kinaesthetic instruction there are two possible criteria, a projected kinaesthesis or a process attitude so complex as to baffle the *O*. The reports of his *O*s show the use of wrist kinaesthesis and rate of movement. There is nowhere reference to the projection of sensations. Reid, as *O*, states: "It seemed impossible to judge in terms of wrist only." Our own *O*s reported as bases of judgment kinaesthesis, pressure, weight, and rate of movement, with kinaesthesis predominating. There was visual imagery in the earlier judgments. There is nothing in the reports to indicate projection, except in the cases in which weight was used. The complex process attitude was probably used in those cases in which pressure and kinaesthesis are indicated. But the majority of comparisons, in which kinaesthesis alone is reported, are not accounted for in the suggested analysis. We cannot agree that it is impossible to isolate kinaesthetic sensations, for every one of our *O*s did isolate them in a large proportion of judgments. We, therefore, add the criterion of fulfilled kinaesthetic instructions.

The tentative analysis, to which we have referred, suggests two possibilities under pressure instruction, namely decision on the basis of projected pressure or judgment with confused process attitude. Reid's *O*s reported under this instruction that they judged on the basis of pressure, weight, strain, and the quality, extent, and intensity of pressure. There is no suggestion of projecting any sensations, though in the case of the weight criteria it is not improbable. Our own *O*s state that they judged by the criteria of pressure (preeminently), weight, kinaesthesis, and visual images of the area stimulated. Only the weight criteria may be said to lend them- [illegible] interpretation. Confused process attitude [illegible] to be a plausible explanation of [illegible] [illegible] there are many judgments which do not fall into any of these classifications, there must be at least one other criterion, *i. e.*, pressure patterns [illegible] to this type of basis.

Summary

We have discovered a strong tendency toward correspondence between carriage of instructions and accuracy of judgment. For two *O*s there is a complete correlation. The relationship between purity of attitude and practice effect in the several series is uncertain, although in the case of one *S* there is a full correlation. The conditions of our experiment, however, were not favorable to sharp differentiation in this respect. The failure to discern any noteworthy correspondence between purity of attitude and the I. U.'s and the P. S. E.'s is at first glance disappointing. Equally disappointing seems to be the case of the relative order of the size of the I. U.'s and the P. S. E.'s under the three instructions for the four *O*s (one with another). But it is difficult to see how one could have expected any such correlation with results of such totally different sense modalities as pressure and kinaesthesis, when compared with each other or when compared with the equivocal stimulus attitude. It would be equally reasonable to compare limens for visual and auditory sensations with each other and with a mixed sensory attitude.

However, the correlation which is found between purity of attitude and index of precision seems as reasonable as one might have expected under equivocal conditions, and indicates the necessity of purity of attitude in future psychophysical experiments. This may eventually mean the abandonment of a rapid series of judgments which has previously been the ideal in psychophysics. The advantage of such an instrument as the turning-top table for lifted weights is that the *E* acquired a sufficiently large number of judgments for statistical treatment in a relatively short time. The statistical value of such a series of judgments, however, is nil if we have no check upon the constancy of attitude under which they were taken.

Our *O*s found it possible to judge according to the three different instructions, but, except in a few cases, no one of these attitudes could be maintained constant for any large number of successive comparisons. It was even found that when one judgment only was required the stipulated basis of decision was very frequently supplemented by foreign criteria. Reid has suggested, with reference to his own instructions, that further refinement is desirable in order to eliminate equivocality. This may prove to be the case; but we are inclined strongly to the view that it is of far more importance that we secure a more thorough introspective check upon the comparison consciousness under the required attitudes. It may well be that we shall have to secure a report upon constancy of attitude after each comparison judgment. But we have found that even under such conditions there will be many judgments rendered under

contaminated instruction. We shall, therefore, be compelled to eliminate from statistical treatment all judgments made in a contaminated attitude. Under these conditions the labor involved in order to obtain sufficient judgments for statistical treatment will obviously be tremendous, but there seems no way out of it. It is obvious also that such a procedure narrows down work on the statistical sensory limen to that of the *S* highly trained in psychological introspection solely. There is one possible exception to this, on which, however, we do not have any data at the present moment. It may be that the *S* who is entirely naive to psychological introspection makes judgments under an uncontaminated stimulus attitude. But, so far as we can see, there is no possible way of ever determining this.

This much can be said, that even our highly trained psychological *S*s find it much easier to keep the attitude under stimulus instruction pure and uncontaminated than either of the process attitudes. And this may be because the *S* who is highly trained psychologically is after all a commonplace human being, who goes through life making many thousands of judgments under the commonplace stimulus attitude for every single judgment he makes under a strictly sensory attitude.

The failure judgments are without significance. The indices of significance of difference are greatest for the *O* with the lowest coefficients of precision and lowest for the *O* with the highest accuracy. The records of tendency of judgment are of no significance except that they are a check upon other statistical results.

Conclusions

(1) The effect of progressive practice is evident, with variations, under each of the three instructions, but it is most pronounced under the stimulus instruction.

(2) There is a high correlation between purity of attitude, as revealed by the introspective reports, and the indices of precision.

(3) The stimulus attitude yielded the highest precision of judgment.

(4) There is little relationship between carriage of instructions and the I. U.'s and the P. S. E.'s.

(5) Judgments on lifted weights under several instructions are of questionable value without an introspective check on every judgment.

(6) The three instructions used in this experiment, namely [illegible] the finger-tips, judgment on the basis of kinaesthetic sensations in the wrist, and judgment on the basis of the stimulus itself, are susceptible of fulfilment for a trained *O*, but there can be no certainty of the constancy of the attitude over more than one judgment at a time.

THE MECHANISM OF CONSCIOUSNESS: PRE-SENSATION

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This is the first of a series of papers describing systematically the mechanism of consciousness as known to introspection using modern methods. It will be convenient to begin with an investigation of the earliest effects of a stimulus on consciousness, and to treat the subsequent effects in an order that corresponds roughly with the classical division into sensation, percept, image and judgment. This paper deals only with the first of these divisions.

The first effect of a stimulus on consciousness, or at least on that part of consciousness which is susceptible to introspection, appears to be, not the full fledged classical sensation with its definite qualities of specific individuality, extension and duration, but something quite different.

Stimulus: a uniform patch of orange without visual boundary produced by alternating red and yellow illumination at a rate just sufficient to prevent flicker. *Instructions:* "Try to see the color 'red' in the color shown." *Preliminary period:* 30 sec. *O:* F. R. B. *Report:* "The (orange) color fairly leaped at me, that is to say, seemed to come accompanied with a feeling of shock. It was not localized at any particular distance and had no particular form. There was a decided kinaesthesis of drawing back. The color had no meaning. It was a feeling, pleasant and warm, though this was not the sensation 'warm' but the feeling. In the last experiment I attended to this warm feeling and had the sensation of bodily warmth which usually follows when anything is pleasant. This time there were the color feeling (not recognized as orange), the warm feeling (not recognized), and a pleasant feeling (this last intense, but not recognized). I can only talk about it because as I try to recall the state these pre-feelings are connected with the actual sensation. They did not do this during the experiment. Then they were timeless and did not seem to be located in space, at first. Then I was aware of a gradually developing strain *plus* a sort of drawing in of orange (entoptic) cloud figures, of which I was gradually becoming aware. These figures I did not interpret to be entoptic; they then seemed part of the feeling 'strain in the eyes' which was not fully interpreted as such. The strain-part of the total situation seemed to grow, the color and luminous aspect to decrease in amount. At this time I was beginning to feel the coming interpretation which was the kinaesthetic vocal image 'orange'; that is to say, I seemed to say 'orange' to myself. Up to this time the orange feel was non-focal, that is, was hardly 'seen' at all. On conclusion of the kinaesthetic vocal image [percept], the orange sensation was definitely located in space with a rough circular boundary formed of the orange-brown cloud-like figures. Suddenly kinaesthetic vocal strain intensified. The kinaesthetic vocal image 'red' developed without warning or other fore-phenomena. It did not possess meaning. There was rapid movement of the entoptic figures, quick pre-sensations from the diaphragm, back of throat, and the chest, all of which ordinarily accompany the consciousness of mental shock. Here only the feeling, not the

recognition, was present. I can not remember all of the details, but I became aware of the *Aufgabe* as a kinaesthetic vocal strain as if trying to repeat the instructions. The kinaesthetic vocal image 'red' stood out particularly. The difficulty in remembering the details seems to have been due to the extremely rapid flow of content and the vividness of the following experiences. Movement of the entoptic border now became very rapid, though the color was less pronounced. Suddenly one fog-figure detached itself and became focal. At first it was purple gray, then reddish purple, then magenta. The figure now had a bright border. It was moving backwards. I was conscious of blinking my eyes and there was a strain in the eyes and throat. After winking the strain had disappeared, the orange background was now at a definite distance from me. It was not unspaced as it had first been. It was more reddish in tone than previously, the border being distinctly an unsaturated orange pink. There was unattended-to kinaesthetic vocal activity growing to a climactic 'It's red'. Reacted."

Though this report is my own, it is believed to be free from bias, being made before the theory of this paper was formulated. It is only slightly more detailed than the best obtained by my other *Os*. The introspections of all (31) *Os* apparently are consistent in all important points and lead to the following picture.

(1) The first conscious effect that can be traced to a stimulus of the sense-organs is a feeling which does not possess spatial or temporal quality, that is to say, is not felt to be located or extended in space or time, or to have the definite qualities and relations usually associated with sensations. Such feelings or pre-sensations, as they will be called, can not be described accurately, as they have none of the substantive or relational qualities necessary for description. They can only be felt.

Thus my *Os* report the first effect of a stimulus to be: "an undifferentiated something", "a feel", "without quality", "indescribable", "could not be attended to", "it just was", "it was not located in time", "in space".

(2) These pre-sensations, however, have emotional tone and feeling quality. They possess intensity. They differ with the kind of stimulus, but this difference is not describable except by incomplete figures of speech.

They were said to have "emotional tone". "They were an emotional tone spreading over the whole of consciousness." "It felt cold" [the pre-sensation blue]. "It was not, however, the cold 'feel' like that due to a cold stimulus." "The feels [of green and of the taste of cream] were both soothing and smooth and of about the same intensity but they were not alike."

(3) The pre-sensations tend to be followed by varying perceptual and imaginal contents which are distinct from them and which appear to be stimulated by them. At least, pre-sensation activity seems to be a necessary condition for perceptual activity.

"The 'feel' was unstable and tended to generate the sensation." "It acted as a stimulus." "It disappeared, the sensation took its place." "There was a closing in of the feel, followed by a sudden perception of position and shape. The feel still remained, however, as a background on which the new perception arose." "I can always feel the coming sensation.

Sometimes, though, I have the feel of a visual something without the perception of position or shape."

This stimulation of one bit of consciousness by others is not restricted to stimulation of percept by pre-sensation but appears to be a characteristic feature of many conscious transitions, especially of those where the transition is "logical" or predetermined.

In the particular case under investigation the stimulation of percept by pre-sensation may be likened to, if indeed it is not, the psychological correlate of the stimulation of a high level reflex arc by the activity of a lower one according to the familiar scheme of Hughlings Jackson and his school (Fig. 1*a*).

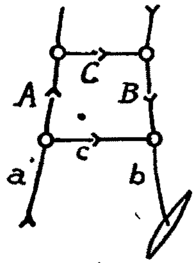

FIG. 1*a*

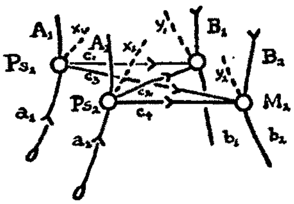

FIG. 1*b*

FIG. 1*a* The Hughlings Jackson scheme of reflex arcs
FIG. 1*b* The psychological relations of pre-sensations

(4) A given pre-sensation tends to stimulate a considerable range of percepts,—a perception pattern,—usually of its own modality. In this, which is not always the same for a given pre-sensation, there is usually a tolerably simple and constant part, the so-called sensation, which may be analyzed out of it, though this is an act involving more than direct perception. This sensation is almost invariably of the modality of the pre-sensation, but differs from it: (*a*) in that it follows a previous feel as if stimulated by it, and therefore has a history which the pre-sensation has not; (*b*) in that it is projected upon pre-sensation activity; (*c*) in that it has space and time relations (at least for sensations of most modes), such as size, shape; and (*d*) in that it has describable modality.

(5) A particular pre-sensation may fail, however, to stimulate its corresponding perception together with its subvariety sensation under a variety of circumstances. (*a*) When the particular sensation that would normally follow is inhibited by the "higher" activities of consciousness,—in a way that is reminiscent of the way in which activity of a high-level reflex arc of the Hughlings Jackson scheme inhibits the activity of a low level arc. (*b*) When for some reason some other percept activ-

ity is particularly susceptible to stimulation, so that the low-level stimulus drains off *via* the path leading to it and not *via* its normal path,—a circumstance which may be pictured, according to the Hughlings Jackson scheme, by assuming cross-paths (Fig. 1*b*) normally of high impedance leading to these centers.

Thus a pre-sensation may stimulate very different percepts from those that would be expected from the nature of the pre-sensation. The percept stimulated may even be of a different modality from the pre-sensation or the sensation normally stimulated by it; *e. g.*, the vocal "image" orange of the introspection quoted above.

This phenomenon, which does not appear to have been noted for a synaesthetic *S* is, as far as can be judged from data at hand, relatively common and forms a link between asynaesthesia and synaesthesia. This is important from the point of view of the student of synaesthesia, because it breaks down the supposedly sharp line between the synaesthetic and the asynaesthetic process, and because it gives strong confirmatory evidence for the view of Wheeler and Cutsforth[1] that synaesthesia is simply a form of perception, differing from ordinary perception only in that it is of a fixed modality regardless of the modality of the pre-sensation. This means, in anatomical terms, the existence of paths from pre-sensation arcs of one modality to percept-centers normally stimulated by arcs of another modality.

For the purposes of this theory, synaesthesia is important because the circumstance of a constant difference of modality between pre-sensation and perception makes it simpler to separate by introspection the "feel" of pre-sensation from the relations of the percept. Thus we can expect synaesthetic *S*s to be most satisfactory for investigation of the nature of activity in the pre-sensation arc. Note, for instance, the descriptions of pre-sensation activity reported by Wheeler and Cutsforth[2]; "I find that I can attend to the sound as long as I do not attempt to analyze it; all the sound of which I am aware is an undifferentiated 'something'; it may be raw sound; it is meaningless; I interpret the experience as sound in terms of the vocal motor image 'sound' or my attention shifts to motor relaxation and I find myself merely responding to the sound situation. I simply can not focalize the sound as such in the absence of visual imagery."

(6) It is doubtful if a pre-sensation can be originated by any activity of the perceptional or other higher levels. Pre-sensations which apparently are originated in this manner (such as the red pre-sensation encountered above) are apparently always due to weak stimulation from the end-organs, such as entoptic phenomena. On this point data are not complete and further investigation is needed.

(7) When perception is inhibited by activity of higher arcs there seems to be no certain proof that the underlying pre-sensation is also inhibited. I originally thought that this customarily occurred, but greater practice in introspection has greatly decreased the number of cases where the pre-sensation

[1] R. H. Wheeler and T. D. Cutsforth, *Univ. of Oregon Publ.*, 1, 1922, No. 10.

[2] R. H. Wheeler and T. D. Cutsforth, *Psych. Rev.*, 29, 1922, 214. What is called a pre-sensation here is called by Wheeler and Cutsforth a primary sensation.

could not be detected even though perception or its subvariety sensation was inhibited. I now suspect that the residual cases where I have not been able to obtain direct introspective evidence of the existence of "sub-conscious" pre-sensations are simply due to lack of introspective technique.

Thus when the attention, that is the image-level, is engaged in seeing an apple, this activity of the higher level often completely inhibits the perception of extraneous smells and sounds of low intensity. Nevertheless, the existence of the corresponding pre-sensation of smells and sounds makes itself evident by the general affective tone. Thus the picture of an apple feels more pleasant in the presence of an unperceived fruity odor than in the presence of an unperceived putrid odor. The free association reaction is also markedly affected. Thus, to take an informal example, an unperceived spicy odor tends to suggest appropriate after-images associated with cooking, *e. g.*, pie, etc.

(8) When two or more end-organs are stimulated together so that two or more separate pre-sensations might be expected, apparently in every case fusion of some sort takes place; there being but one joint pre-sensation, not two separate ones. This is true even when the simultaneous stimuli are as diverse as, say, a visual stimulus in the median line and a tactile stimulus applied to the back of the hand. When two such incongruous stimuli occur the first effect on the consciousness is not that of two stimuli but that of a single fused pre-sensation partaking of the quality of both tactile and visual pre-sensation without spatial discrimination of its two components but with decided emotional tone, normally unpleasant, if the stimuli are moderately intense. This non-discriminating pre-sensation fusion usually, of course, almost immediately stimulates the appropriate separate percepts, or to be more exact, it stimulates the appropriate pattern, if such a pattern be possible, composed of the two appropriate sensations in the proper spatial relation to each other and projected on the original fused pre-sensation as on a background, a background which retains the emotional tone of the original fusion.

There seem to be no exceptions to this rule of pre-sensation fusion, though pre-sensations are so instable and such difficult objects for introspection that it is hard to be sure. At any event, cases which were once thought to be exceptional (the case used above to illustrate the rule is one) have either yielded to more careful introspection the distinct steps: (1) pre-sensation, (2) a percept-pattern involving both sensations in their appropriate relation; or they have yielded, in cases where the stimuli were even more incongruous, as in cases of binocular rivalry, an unpleasant pre-sensation having the quality of both stimuli, *plus* a percept involving only one of the two incongruous sensations, the other being suppressed temporarily at least. Still a third variety of reaction to incongruous pairs of stimuli appears to occur, namely, partial transfer of quality from the recessive to the dominant percept. Thus examples are reported in binocular vision where the color of an otherwise recessive figure is transferred to the dominant figure. These cases have not been carefully studied from the point of view of this paper.

(9) Pre-sensations may produce motor effects directly, either with or without conjoint stimulation of percept activity. This direct stimulation of motor reactions is, however, subject to inhibition by higher centers. For the higher classes of perception this inhibition is rather the rule. However, for some sorts of stimuli and under some conditions, the direct motor effect of a pre-sensation is more prominent than its effect as a stimulus for perception.

Thus, my *Os* found that an attempt to hold pre-sensations in consciousness by inhibiting percept-activity almost inevitably was partially frustrated by the direct stimulation by the unpleasant (under these conditions) pre-sensation of defensive motor activity of the semi-involuntary type. Thus, an attempt to hold a visual pre-sensation in consciousness almost invariably led to winking and to a consequent stimulation of new muscular and tactile pre-sensations due to that act.

In fact, though no very extensive study has been made, it is felt that winking, often thought to be an unconscious reflex in its normal functioning, is really stimulated by conscious, though involuntary pre-sensations, that is to say, by the "feel" of the visual stimulus. This is probably true for such other semi-involuntary reactions to strained conditions as the various motions in the throat and mouth tending to clear irritations, or as the suspension and intensification of breathing under emotional strain, or as various twitches of the lips, the nostrils and the body which aim to remove the cause of unpleasant cutaneous sensation,—in short many of the so-called "cortical" reflexes. This is also probably true for a whole series of semi-conscious pleasant sensations, *e. g.*, sexual reactions (a case which is complicated by control of even higher centers), the semi-involuntary reactions to the smell and the sight of food, the smile, and others.

The list given above is all that the author has personally investigated, but it is possible that it may be extended, perhaps, to cover the defensive and offensive reactions the back-stimulation from which is a large part of the sensation-content of emotion, as pointed out by the familiar James-Lange theory. It is not supposed that all emotions showing the James-Lange effects involve only direct reactions to pre-sensations; for there are cases where emotion demonstrably involves still higher centers. Restricted to cases where the emotion is felt before perception is involved, investigation shows that in the one case carefully studied here, namely, fear due to to a sudden intense stimulation, the whole conscious effect of the fear-producing stimulus is the conscious, unpleasant 'fear' pre-sensation *plus* the direct stimulation by that pre-sensation of the well-known semi-involuntary muscle reactions characterizing fear, *plus* the direct stimulation by these muscle reactions of the James-Lange pre-sensations.

However, regardless of whether the list of motor effects directly stimulated by pre-sensations can be extended to cover all of the motor reactions underlying the back-stimuli which form a large part of the total conscious situation in cases of emotion, the general connection between pre-sensation and a large number of direct defensive and offensive reactions is definite, and [illegible] with the analogy of the pre-sensation to the activity of a higher reflex are of [illegible] Jackson's [illegible].

The sense of pain is an especially good example not only of the direct [illegible] of motor activity by pre-sensation activity but also of the general characteristics of pre-sensations. This is so presumably because of the high resistance of the paths from the pre-sensation to the percept cen-

ters. Whatever the reason, pain of low intensity, not accompanied by localizing tactile sensations of other modalities, is normally felt as pure pre-sensation, with but little accompanying perceptual activity. That is to say, pain is ordinarily just feeling without extension, position, substantial quality, or relations to other qualities. Nevertheless, or rather because of this fact, it is characterized, as the most elementary introspection will show, by a most pronounced tendency to stimulate defensive reactions, exactly as one would expect on the Hughlings Jackson scheme for an arc where, because of high resistance of the paths leading to and from the next higher arc, that higher arc cannot easily be stimulated by, or easily inhibit, activity of the lower arc.

In fact, it appears that there is no part of the psychological evidence given above which is inconsistent with the Hughlings Jackson model. Since this point is important, it will be wise to review the evidence.

It appears from the purely introspective data summarized in this paper that the relations between pre-sensations and other forms of conscious content, though of course not the qualitative nature of the pre-sensation as given in (1) and (2), can be summarized according to the diagram, Fig. 1*b*. According to this diagram, an impulse travelling up A_1 or A_2 produces at PS_1 or PS_2 a conscious effect, the pre-sensation. The nature of this conscious effect will differ with the kind of stimulus. In the case that two or more pre-sensations, say PS_1 and PS_2, are stimulated together, they simply sum up in consciousness without being discriminated; that is to say, they give a single feel, the resultant pre-sensation (PS_1-PS_2), not two separate feels (PS_1) and (PS_2). A single pre-sensation PS_1 may stimulate motor activity directly, as is indicated on the diagram by the paths c_1 and c_3 leading to the motor centers M_1 and M_2. This direct stimulation of motor activity may be inhibited by impulses originating at higher levels and travelling *via* B_1 and B_2. Also, because PS_2 may also send impulses to the same motor center *via* c_2, the motor reaction of M_1 will depend not alone on the activity of PS_1 and of higher centers, but also on the activity of PS_2, the resultant motor activity being not the sum of the activity that would be separately produced by PS_1 and PS_2, but possibly quite different, as for example when the activity stimulated *via* c_2 is such as to inhibit stimulation *via* c_1.

Besides stimulating motor activity directly, the pre-sensation may stimulate perception *via* the direct path A_1 or, under certain little understood conditions, *via* the high resistance path x_1. If it stimulates *via* A_1, the resultant percept will include what is ordinarily called the normal sensations appropriate to the path a_1A_1. If not, and if the stimulation travels *via* x_1, as it may in cases of synaesthesia, and in the cases described under (5), the product will be a percept which does not include the normal sensation and may even be of a different modality from it. There is, however, no opportunity, as is indicated by the

direction of the arrows, for percepts or higher activity either to stimulate or to inhibit pre-sensations. The pre-sensation is an entirely involuntary and automatic bit of consciousness.

As has been suggested, the diagram is similar to that which might be drawn by a physiologist of the Hughlings Jackson school to explain the physiological facts of the behaviour of the optic thalamus. So interpreted, our 'directions of stimulation' become conduction paths, our arrows unidirectional synaptic gaps, our pre-sensation centers neurones. It is, therefore, natural to assume that the facts of the connections of pre-sensations with other parts of consciousness, as shown on the diagram, are in fact imposed on pre-sensation by the physiological structure of the physiological acts involved, just as many of the psychological facts of vision are known to be conditioned by the physiological structure of the optic apparatus. If this assumption is made, it becomes immediately possible from a study of the effects of thalamic lesions to locate the organ of pre-sensation in the optic thalamus and analogous parts for conduction paths not involving the spinal cord.

Thalamic lesions of three types are known. (1) Those affecting the fillet and associated parts, in the present scheme the paths a_1 and a_2. With lesions of this type, both sensation and pre-sensation are presumably lost. (2) Those affecting the fillet-cortico-thalamus, which bundle presumably includes both the afferent and the efferent fibers A_1, A_2, B_1, B_2, x_1, x_2. Lesions in this area are followed by loss of sensation without loss of pre-sensation. (3) Those that affect the lateral zone of the thalamus without disturbing cortical connections. These lesions more or less interrupt the activity of the pre-sensation arc, especially the return paths B_1 B_2, which ordinarily serve to inhibit its excessive activity. As a result, in lesions of the type (2) and (3), where B_1 and B_2 are cut, the thalamus is freed from the normal cortical control and the patient exhibits the results of excessive thalamic activity, that is to say, the "syndrome thalamique" of Déjerine and his pupils,[3] characterized by spontaneous pains, curious crawling and tingling sensations of the unpleasant type, while sensations of the pleasant type have much greater affective quality. Thus, to quote Head[4]: "In one case, we were able to show that the patient could not recognize any thermal stimulation as such, and yet over the affected half of the chest large tubes containing water at from 38°C to 48°C evoked intense pleasure. This was shown not only by the expression of her face, but by her exclamations 'Oh, that's lovely, it's so soothing, so very pleasant'." Remembering the over reaction which is characteristic of the removal of higher control, these symptoms are what would be expected to follow lesion of the cortico-thalamic paths according to our theory. The only other type of symptom which might be expected from lesions in this area would be from those injuring the direct paths A_1 A_2, but leaving intact the oblique paths x_1 x_2. Anatomically speaking, this effect is probably impossible for gross injuries because of the close association of these paths in the post-thalamic fillet, but if possible the symptom expected would be synaesthesia. Relatively high resistance or low development of these direct paths would also

[3] J. Déjerine and M. Egger, *Rev. neurol.*, 13, 1903, 397: J. Déjerine and G. Roussy, *ibid.*, 14, 1906, 52; J. Déjerine, *Semiologie des affections du système nerveux*, 1914.

[4] H. Head, *Studies in Neurology*, 1, 383.

cause the same symptoms without the necessity of detectable lesions. This physiological theory seems sufficient to explain the phenomena of perceptual synaesthesia completely, and to be free from the objections urged by Wheeler and Cutsforth against previous physiological theories which postulate cross-connections from one percept-area to another with consequent transfer of functions, these objections being: the wide separation of the areas involved in perception; the lack of permanence of synaesthesia; the fact that one may have synaesthetic percepts of tones of a melody without having synaesthetic perception of melody; the high development of the auditory area necessary to account for the completeness of the correspondence of colored tones to stimuli, to take but one example, and yet the absence of the natural function of that area of perceiving the stimulus in its natural modality.

A SCALE FOR SCORING TESTS WITH ALTERNATIVE ANSWERS

By Gilbert J. Rich

Kohs[1] and Richards[2] have shown that the usual method of scoring tests in which the *S* checks one of two, three or four alternative answers to every item (by subtracting from the number of correct responses the number of errors, 1/2 the number of errors, or 1/3 the number of errors, respectively) does not eliminate the factor of chance as it is supposed to do, and permits the guesser to make a significant positive score. Richards, furthermore, suggests a new method of scoring tests of this type. In his procedure, the crude score is equal to the number of correct responses up to the point where the probability of obtaining the given number of correct answers by chance is .04. Beyond this point, the score for each successive number of correct items is obtained by adding to the preceding score the ratio of the probabilities of attaining by chance the two numbers of correct responses concerned.

Several points should be noted in connection with the method of scoring which Richards proposes. On the positive side, it is a definite effort to apply the calculus of probabilities to the problem of scoring and to weight the successive numbers of the test in inverse proportion to the probability of their being reached by guessing, that is, by chance. A performance that might easily be the result of a guess is of little weight; one into which the chance factor is not likely to enter receives a greater weight. In so far as it accomplishes this end to a greater degree than do the older methods, the new scale is a distinct advance. It also avoids negative scores, which are disadvantageous for practical purposes. On the other hand, the scale is mathematically discontinuous. The score is not a definite function of the number of correct answers. For low scores it is one type of function, for high scores an entirely different function. If it had been shown that the *S*'s performance in the test is actually best represented by two discrete curves, it might be proper to use this form of scale. But in the absence of such proof there is no justification for not using a single function. The use of two functions has the disadvantage of making high and low scores incomparable for statistical purposes, since they are derived from different bases. Moreover, the point at which the score changes

[1] S. C. Kohs, High Test Scores Attained by Subaverage Minds, *Psychol. Bull.*, 17, 1920, 1-5.

[2] O. W. Richards, High Test Scores Attained by Subaverage Minds, III, *J. Exper. Psychol.*, 7, 1924, 148-156.

from the one function to the other is purely arbitrary. There is no *a priori* reason for fixing upon a probability of .04 as the critical value.

It would seem possible, however, to construct a scale for scoring these tests which should retain the patent advantages of Richards' method and yet avoid the difficulty of a non-continuous scale. In such a scale it should be possible to express by means of a general formulation the score for any number of correct responses. The scale should: (1) increase from a maximum of zero for no items correct, thereby avoiding negative scores; (2) reach a maximum value of 100 when all items are right; (3) have successive steps which are inversely proportional to the probabilities of obtaining the respective values by mere chance (guessing); and (4) be mathematically homogeneous (expressible by a single general formula), so that all parts of the scale would be statistically comparable. Every one of these requirements may be met by merely carrying Richards' procedure to its logical conclusion. Instead of making the scale-increments inversely proportional to the probabilities only beyond an arbitrarily selected value, we may start with a zero score when all items are wrongly checked, and for the entire scale above zero form each successive value by adding to the preceding score an increment proportional to the ratio of the probabilities as mentioned above. The entire scale will then be homogeneous. A scale of this type is described below.

We may readily define our scale by stating the mathematical formula according to which any performance in the test is to be scored. We shall be dealing with a test of N items, each one of which is to be answered by checking one of M alternative responses (of which only one is correct and the others wrong). We desire the score when R items have been correctly checked. Let us call P_R the probability that exactly R items will be correctly answered purely by chance, T_R the score for R items correct upon an arbitrary scale, and S_R the same score linearly transformed to a scale of 100 units. Richards[3] has shown that:

$$P_R = \frac{N!}{R!\ (N-R)!} \times \frac{(M-1)^{N-R}}{M^N} \qquad (1)$$

If, now, each value of the scale, starting with zero, is formed by adding to the preceding value the ratio of the probability of obtaining by chance the preceding number of correct items to the probability of similarly obtaining the number of correct items in question, then:

$$T_R = 0 + \frac{P_0}{P_1} + \frac{P_1}{P_2} + \ldots\ldots + \frac{P_{R-1}}{P_R} \qquad (2)$$

[3]O. W. Richards, *op. cit.*, 149.

When $R = 0$, none of the fractions can be set up, P_{-1} does not exist, and $T_0 = 0$. A final step is to transform the scores to a scale of 100. Since the maximum score is obtained when $R = N$, and this score is to be 100:

$$S_R = \frac{100}{T_N} \times T_R \tag{3}$$

The value of S_R so obtained meets all the requirements set forth above. To illustrate the use of this scale, we have computed it for the same three examples for which Richards gives the values of his scale, namely; 25 items with 4 alternative responses, 25 items with 3 alternatives, and 50 items with two alternatives. The values of P_R for these three cases are given by Richards and Kohs.[4] As the practical use of a scale usually requires that it be limited to integral values, we have followed Richards' example and have rounded the figures by dropping all fractions in favor of the nearest integer. The completed scales are shown in the accompanying Table.

25 Items			*50 Items. 2 Responses*			
No. Right	Score 4 Resp.	Score 3 Resp.	No. Right	Score	No. Right	Score
25	100	100	50	100	25	5
24	66	67	49	72	24	5
23	50	50	48	58	23	4
22	40	40	47	49	22	4
21	32	33	46	43	21	4
20	27	27	45	38	20	3
19	23	23	44	34	19	3
18	19	19	43	30	18	3
17	16	16	42	27	17	2
16	13	13	41	25	16	2
15	11	11	40	22	15	2
14	9	9	39	20	14	1
13	7	8	38	18	13	1
12	6	6	37	17	12	1
11	5	5	36	15	11	1
10	4	4	35	14	10	1
9	3	3	34	13	9	1
8	2	3	33	12	8	0
7	2	2	32	11	7	0
6	1	2	31	10	6	0
5	1	1	30	9	5	0
4	1	1	29	8	4	0
3	0	0	28	7	3	0
2	0	0	27	7	2	0
1	0	0	26	6	1	0
0	0	0			0	0

An examination of any one of these scales will show that it accords but little value to the first few responses. This is as it

[4] O. W. Richards, *op. cit.*, Tables I and II; S. C. Kohs, *op. cit.*, Table I.

should be. The first few responses may be due to guessing. Indeed, it is improbable that a guess would not result in some correct items. As the number of correct responses increases, the value of each additional right answer becomes larger in proportion as the probability of its being attained by chance decreases.

The final test of this type of scale, as of any method of scoring, is of course its utility in producing results which are significant in interpreting the tests to which it is applied. We may here only suggest the mathematical basis of the scale; its practical value cannot be shown until it has been used and the results of its use evaluated. Thurstone[5] has proposed a method for computing the amount that each error should be penalized in any given test, but his method is applicable only to linear scoring formulae, and our formula is not linear. If the differences between the upper values of our scale are considered too large for educational or other applied use, the scale may be combined with some other scale in the way that Richards suggests, with any weighting desired. This, however, is a question into which we cannot enter, since the additional scale to be used and the relative weighting are arbitrary matters determined in large part by the particular problem at hand.

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[5] L. L. Thurstone, A Scoring Method for Mental Tests, *Psychol. Bull.*, 16, 1919, 235-240.

THE DEFINITION AND MEASUREMENT OF ATTENTION

By H. M. Johnson, Mellon Institute, University of Pittsburgh

I

Contemporary psychologists make use of a single term, 'attention,' to designate two variables indifferently. One of these variables is the degree of consciousness or the clearness of content; the other is the degree of the sensorimotor adjustment of the organism with respect to a particular stimulus. Some authors appear to confuse the two concepts, others regard them as distinct and mutually supplementary. This paper examines them and suggests their mutual independence.

'Attention' as Vividness or Clearness

All of the influential non-behavioristic authors of the present day assert that the particular objects to which one 'attends' are perceived in higher degrees of vividness or clearness than the other objects which one cognizes at the same instant. Hence, the degree of attention which one pays to an object is capable of being indicated by the degree to which one is conscious of it; or, in other words, by the degree of clearness which the object, as an element of content, is observed to possess. The measurement of attention, in this sense, therefore becomes a problem of estimating, as well as one may, the degrees of clearness of each element of a simultaneous complex of content; or, figuratively, of 'distributing' the various elements according to the magnitudes of their clearnesses.

Experienced introspectors assert that the task of comparing the clearnesses of various portions of content, independently of their other characteristics, is not at all easy. The assignment of satisfactory numerical values to these clearnesses is still more difficult. After long practice, a few adepts in the art of introspection achieve at least a consistency in respect to the frequencies with which the several numerical values are recorded.

It is, of course, impossible for anyone but the introspector to make the observations; and some non-behaviorists have debated among themselves whether training actually increases one's acuteness and reliability in this sort of observation or, rather, tends merely to fix a conventionalized language-habit. Since such a question cannot be settled by means which the objective psychologist concedes to be valid, our attitude shall remain agnostic.

'Attention' as Adjustment for Reception and Response

The term 'attention' is also currently used to designate (1) the adjustment of the accessory sensory apparatus to facilitate optimal excitation of a specific group of receptors by a specific complex of stimuli; this adjustment being accompanied by (2) the adjustment of some specific effector-apparatus for appropriate response to those stimuli; together with (3) the inhibition of activities which might interfere with such reception and response; and (4) the innervation of a very diffuse system of musculature which plays no direct rôle in the adjustment, except, perhaps, by serving to drain neural current from inappropriate pathways, and to facilitate, by means of neural flux generated by such systematic activity, the maintenance of the adjustmental attitude.[1]

The details of this process may perhaps be made clearer if they are represented in a neural scheme which would be preposterous if taken too literally, but which possesses some pedagogical advantages. (Cf. Fig. 1.)

Let R_1 indicate a receptor connected by a chain of neurones to an appropriate effector M_1, the receptor being specifically irritable by a stimulus S_1, and being capable of adjustment as to position and direction by a muscle M_2, which may be considered as 'accessory' to R_1. If the stimulus S_1 be presented while R_1 occupies a position unfavorable to its reception (as in the position indicated by the dotted lines), the reaction-arc R_1 M_1 may be [illegible] feebly or not at all. Furthermore, if neural flux aroused in other receptors than R_1 is not being drained into M_1, the latter will possess little 'tonic' innervation; its sensitivity will be low, and S_1 will be subnormally effective in arousing its response.

Suppose, however, that a second receptor, such as R_2, be connected directly with the accessory muscle M_2, and less directly, *i. e.*, through a pathway of relatively higher resistance, with the muscle M_1. If the stimulus S_1 acts on receptor R_2, the result will be (*a*) contraction of M_2, bringing R_1 into a position favorable to stimulation by S_1; and (*b*) an increase in the irritability of muscle M_1, so that the arc R_1M_1, being reinforced by the arc R_2M_1, is now favorable to excitation by S_1.

Moreover, let us suppose that at some past time, a different stimulus, S_2, applied to the receptor R'_2, has been repeatedly presented shortly before, or simultaneously with, the first stimulus S_1. The result may have been the establishment of a conditioned response of M_2 and M_1, which is equivalent to that aroused by S_1 applied to R_2. In this respect the stimulus S_2 has become a substitute for S_1, or, in laboratory jargon, is a 'preparatory signal' for the latter, and has 'aroused attention' to it.

It often happens, however, that the stimulus S_2 is of momentary duration, while a considerable time elapses before S_1 is presented, but the organism, nevertheless, may hold its attentive attitude with respect to S_1

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[1]As attentional adjustment to a particular object becomes habitual, factor (4) tends to become eliminated; so that the attentional attitude is assumed and maintained with greatly diminished 'strain.' This is especially noteworthy in the reaction-time experiment. The fact that the adjustmental process has been simplified by practice does not imply that it has been rendered less effective. The *S*, in this sense, is not less attentive when thoroughly trained than he was when unpractised; although after long practice, in *selective* reaction, he may assert that he reacts unconsciously.

during the period of delay. Now striped muscle cells, of which M_1 and M_2 were tacitly assumed to be composed, do not ordinarily remain in a state of contractile activity unless they receive continuous innervation. We must therefore assume that a second order of substitution of stimuli has occurred; that some stimulus, either external or internal to the organism, has been acting in the meantime. We might follow some of the recent authors of elementary texts and argue that this stimulus is 'mental' rather than physical; but that would not look well. The *deus ex machina* is not

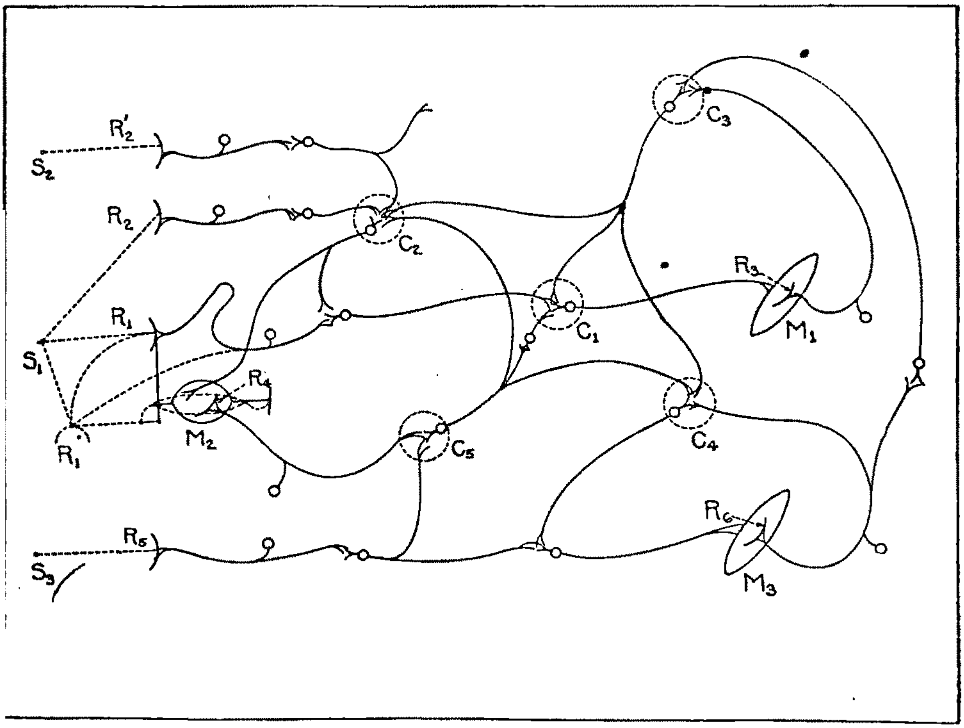

FIGURE I

to be utilized until the author has become helpless. We shall therefore seek the causal agent within the single system we have chosen. The activity of striped muscle is accompanied by chemical changes which constitute adequate stimuli for receptors in the muscle-spindles, and which may thus arouse arcs designated by Dunlap as 'homeodetic.' Let us designate such receptors in the figure as R_3 and R_4, and picture each of them as being connected with the contractile cylinders in M_1 and M_2. Once these muscles [illegible] each [illegible] may [illegible] thrown into circular activity. As long as their current is not diverted, the organism maintains its attitude of adjustment with re- [illegible] the latter.

To illustrate the fact of diffuse innervation, mentioned as factor (4) above, one need only multiply these homeodetic arcs. I shall therefore not complicate the drawing farther.

Let us now suppose that during this period of attentive delay, an irrelevant stimulus S_3 is presented to still another receptor R_5. Let us imagine R_5 as being strongly connected with the effector M_3, and also, but less strongly, connected with M_1; and let us connect the spindles R_3 and R_4 in M_1 and M_2 with M_3 also.

The flux from R_5 toward M_1 is therefore reinforced at the synapse C_5 by flux from R_4 and at C_1 by flux from R_3. If the arcs R_3M_1 and R_4M_1 are sufficiently dominant, the current from S_3 may be diverted into M_1 instead of passing to M_3. In such case, M_1 may be still further sensitized, and the pathway R_1M_1 may be rendered still more permeable to current excited by S_3 in R_1. The stimulus S_3 will therefore facilitate appropriate reaction (of M_1) to the stimulus S_1.

In case M_1 has been sufficiently sensitized by current from R_3 and R_4, the activity of the additional arc R_5M_1 may produce complete contraction, *i. e.*, an 'overt response' of M_1, the *S* behaving practically as if S_1 rather than S_3 had been applied. In such case he may be said to have been 'illuded' or 'hallucinated,' according as S_3 is external or internal to the body.

If, however, the irrelevant, or distracting, stimulus S_3 be sufficiently intense, extensive, enduring, novel, or the like, the arc R_5M_3 may drain the flux from R_3 and R_4 into the final common path from C_4 to M_3, and away from M_1 and M_2, thus diminishing or even abolishing the attentive attitude toward S_1. In such case, when S_1 is presented, the appropriate response of M_1 may be appreciably delayed, retarded or weakened: or it may even fail to occur in overt form.

The Relation between the Degree of 'Clearness' and the Degree of Adjustment

As I suggested at the outset, many authors, without defining the term 'attention' at all, include in their chapters on the subject a treatment of the two separate topics I have just sketched, as if the two somehow belonged together. One recent author asserts that he finds it extremely difficult to relate them; other authors avoid the difficulty by disregarding the question. However, two representative authors, namely, Münsterberg (*13*) and Dunlap (*6, 7, 8*), specifically postulate that the 'degree to which one is conscious' or the 'clearness and vividness' of content, is determined by the degree of adjustment for reception and response; the latter being perhaps theoretically expressible as some function of the number of afferent pathways which combine in producing the appropriate response. Certain other non-behaviorists are less specific in their formulations, but similar postulates seem to be implicit in their treatment.

A Crucial Experiment

Obviously the repertoire of a behaviorist, like the present writer, does not contain the acts which are appropriate to the problem. There is on record, however, an experiment by Cassel and Dallenbach (*2*), which to the present writer appears

to satisfy all the criteria which a non-behaviorist would regard as essential.

In the later and most representative part of this experiment, which alone I shall directly consider (since it was consistent with the earlier and rougher parts of the work), an experienced reactor, whose training in the introspection of attributive clearness had satisfied the standards of the department of psychology at Cornell University, was utilized in the following manner. He was required to react 700 times to an auditory stimulus (S_1), which was preceded by two presentations of a preparatory signal (S_2), given approximately 3 sec. and 1.8 sec.[2] before the appropriate stimulus (S_1). During a part or the whole of the preparatory interval an auditory distracting stimulus (S_3) was presented.—In the scheme given above, it will be seen that if the neural flux excited by S_3 were drained into the effectors M_2 and M_1, the effect would be to increase the *S*'s preparedness for the appropriate stimulus S_1, and to accelerate reaction to the latter. On the contrary, the distracting stimulus S_3 might at other times disturb the adjustment of M_1 and M_2 in at least two ways: either by draining from them the flux excited in the spindles R_3 and R_4 and thus diminishing their irritability; or by overstimulating M_1 so as to produce a premature response of at least gestural magnitude, and thereby necessitating readjustment to S_1 within a time which might be insufficient. In either case the distractor would retard reaction to S_1. From the account given in (*1*), a study to which this one was a corollary, it appears that sometimes one effect and sometimes the other was found, although the general tendency might be consistent throughout a daily series of reactions.

It should now be especially noted that if the magnitude of S_1 was sufficiently great not to tax the acuity of the receptors,—and the report does not suggest the contrary; and since, as we are informed in (*3*), the effects of fatigue and practice were rendered inconsiderable; *the reaction-time is determined chiefly, if not entirely, by the degree of adjustment for reception and response; and hence indicates the degree of 'attention' in the objective meaning of that term.*

Furthermore: after each reaction, while the necessary time (X) was being recorded by an *E*, the *S* recorded a number (Y) as designating the degree of clearness which characterized the noise of the sound-hammer as he perceived it.

Thus were obtained 700 pairs of simultaneous numbers, which were thenceforth capable of objective treatment.

Now, if the assumption is correct, that the degree of clearness is determined by the degree of preparation for reception and response, the coefficient of correlation, r_{xy}, between the gross measures of time (X) and clearness (Y), would approximate to unity provided the relationship be rectilinear; and the correlation-ratio, at least, should approximate to unity, though the relationship be less simple.

In fact, however, no such tendency toward association occurred. The results of such treatment, if resorted to, were not published by the authors: but the present writer, in a general review (*11*), showed, by an indirect and approximate method (the data not having been presented in a form which

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[2] "The first flash, the 'ready' signal, was *set* to occur 3 sec., and the second, the 'now' signal, [illegible] *timed by the Hipp chronoscope* they occurred 3265 ± 32.6σ and 1853 ± 11.8σ respectively before the stimulus" (*1*, 130 f.). Italics are mine. In (*3*) it is alleged that I committed an inaccuracy in using the above values. My review does contain a copyist's substitution of 'Geissler tube' for 'a 2.9 volt electric globe (*sic*).' For this I tender my regrets.

permitted of direct computation) that the correlation r_{xy} was of the order of -0.25,[3] subject to a probable error of about 0.025. With this finding one of the original authors has since expressed himself (*3*) as being "in substantial agreement."

The import of this result is critical. While a correlation which is ten times its probable error is not to be attributed to chance, its significance should be very clear. The quadratic mean of the deviations of the empirical values of Y from the values calculated from the regression-equation is $\sigma_y \sqrt{1 - r^2_{xy}}$, in which σ_y is the quadratic mean of the deviations of the empirical values of Y from their mean M_y. Since in this case $r_{xy} = -0.25$, and $\sqrt{1 - r^2_{xy}} = 0.97$, it follows that the precision with which a particular value of clearness, Y, can be inferred from the corresponding reaction-time, X, is but 3% greater than the precision based on a mere guess from the average M_y. The increase in precision is of the order of magnitude of the probable error of the index of precision itself, and is practically negligible. Furthermore: as I shall show in the second part of this paper, the precision with which the most representative value of a group of clearnesses taken at random can be inferred from the *mean* of their corresponding times is likewise 3% greater than the precision which attends a guess from the general average of clearnesses M_y.

These facts alone should suffice to settle the question. Under the conditions of this experiment the degree of clearness, as indicated by the introspector's numerical values, and the degree of preparedness for reception and response, as indicated by the reaction-time, are *practically* independent variables. The unknown value of neither can be inferred from the known value of the other with a precision which is significantly greater than the precision which attends the best guess that can be made without knowledge of the relationship.

The results, as far as they go, suggest that the degree of clearness and the degree of sensori-motor adjustment are practically unrelated: certainly neither a particular reaction-time nor the mean of a fractional group of reaction-times taken at random yields 'an objective measure of attributive clearness.' At the least, they are sufficient to negate the general proposition that clearness or vividness is determined by the degree of integration of the several reactive mechanisms in the execution of a specific response. That proposition can hereafter be reiterated as general only by an overt or implied "challenge" of the concreteness or of the appropriateness of designation of one or both sets of the correlated numbers. Toward such procedure the present writer has never been seriously inclined.

In the light of the facts, I see no reason why a description of attentive behavior, made in strictly objective terms, need be accompanied by a discussion of clearness of content, to complete the exposition of facts relevant to the former topic.

The term 'attention' might be used to characterize either set of events, but surely not both, unless these results are to be discounted. The second, or objective, meaning of the term

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[3]Two methods of approximation were in fact used. The one outlined in (*11*) yielded a value of -0.24; the one suggested on p. 612 of this paper yielded -0.26. The mean value of -0.25 was considered sufficiently accurate for the purpose for which it was to be employed.

is more nearly in accordance with its use in every-day speech; the first in accordance with the literary fashions in psychology which have prevailed during the last fifty years.

II

Enough has been said about Cassel and Dallenbach's experiment to show that, however subsidiary they may have regarded it, the results have capital importance in the development of a scientific system of psychology. The problem is fundamental; the experimental procedure unimpeachable; and the qualifications of the reactor accepted by the school which sponsors the method. Since a similar experiment is unlikely to be made in a more favorable combination of circumstances, it is desirable that the exhibition of the results be freed from certain ambiguities that still remain.

The original report contains no intimation of the trend of the results which I have pointed out and which one of the authors has now admitted. On the contrary, that report, and also Dallenbach's comment (*3*) on my review (*11*), contain assertions which have been presented by certain lecturers as indicating a perfectly effective tendency in the opposite direction. The printed discussion to which I have referred, together with some private correspondence with Dr. Dallenbach, raises certain doubts in my mind whether this interpretation on the part of my colleagues correctly represented the authors' views, but in any case it is erroneous. I shall therefore examine the assertions which produced this impression.

Having obtained the 700 simultaneous numbers designated as gross measures of time (X) and clearness (Y), the authors treated them in a most unusual manner. The most obvious first step would have been to find the coefficient of correlation between the gross measures or, in its stead, the correlation-ratio. This procedure they rejected as being of no use for their purpose. According to the description in (*3*), that purpose seems to have been to ascertain how strong the tendency toward association would have been if no counter-tendencies had existed. At any rate, they proceeded as follows.

First, they distributed the reaction-times according to the class-values of the clearnesses associated with them. These classes were made large in extent and few in number, so that 96% of the total number of measurements fell within three contiguous classes, the middle one of which contained the mean, and 62% of the total number. Next, they averaged the measurements of times as thus distributed; and finally, they correlated the *averages* of the *distributed* times with the values of clearness which had formed the basis of the distribution. This procedure accomplished two fictitious results: first, it eliminated the variablility of the clearnesses associated with the several times; and, secondly, it eliminated the variations among the times associated with each particular class of clearness. The correlation thus obtained, of course, approximated very closely to unity; but, nevertheless, it does not afford a means of deducing any *unknown* value of clearness—central or particular—from the corresponding value of time. The values of clearness must be directly obtained before the distribution of times can be effected [illegible]; before one can make the inference, one must possess the facts to be inferred.

This treatment of the results, by suppressing whatever tendencies toward independent variability may have existed, imposed upon the data all the characteristics of a perfect correlation which they lacked. The computation which followed merely exhibited those characteristics, with-

out discriminating between those which were inherent and those which were imposed. The resulting coefficient reveals little more than the fact that no large and uncompensated errors were made in the arithmetic.[4] The authors, however, make the following assertions.

(1) "The present experiment shows that reaction-time is closely correlated with clearness" (*2*, 207, footnote).

(2) "We may conclude at any rate that, under these conditions of training, attributive clearness may be measured by the average duration and the mean variation of the simple sensory reaction" (207).

(3) "These data[5] show that *the rate of a simple sensory reaction* and its degree of precision as expressed by the m. v. are both (under our conditions) reliable means of determining the degree of clearness. It therefore follows, since attention itself is measurable in terms of clearness, that these objective measurements also give us a reliable index of attention" (206. Italics are mine).

In the opinion of the several professional mathematicians who have favored me with a critical examination of the foregoing analysis and of my former review, the argument which I have just given is sufficient to make it clear that the propositions in the first and second quotations, and the first proposition in the third, are not valid inferences from the results of the author's statistical treatment. Furthermore, the insignificantly small value (−0.25) of the product-moment coefficient of correlation between the gross measures of time and clearness plainly contradicts the authors' assertion that "reaction-time is closely correlated with clearness," and its corollary, that attributive clearness may be reliably inferred from "the rate of a simple sensory reaction."

Dallenbach, in his remarks (*3*) on my review, has now abandoned these propositions, intimating that they were misconstrued; but he reiterates that "attributive clearness may be measured by the *average* duration..... of the simple sensory reaction" (307). While conceding that my argument is valid, he dismisses it as 'irrelevant' to the latter proposition.

It now becomes necessary to clarify this proposition also, since it tends to obscure the important finding that these two variables— specific adjustment and clearness of content—are practically unrelated.

We must first ascertain the meaning of the expression 'average duration' or 'average reaction-time.' Does it refer (1) specifically to the means of times which were associated with specific class-values of clearness; or does it refer (2) to the averages of times taken without regard to the numerical values of clearness associated with them? Since no restrictive expressions are used, the reader is likely to assume the second meaning. Several readers have done so, I being among their number. However, I now suspect that the restricted meaning was intended, although the authors omitted to indicate the fact when its mention might have abbreviated the discussion. I shall now consider both alternatives.

If the expression be taken in the restricted sense, the assertion just quoted is certainly true. It is, however, subject to the following objectionable features. First, it is without practical significance, since, as I have pointed out, the numerics designated as 'clearness' must be given directly before they can be inferred from the averages of the distributed times. Secondly, the assertion is misleading, since the restrictions on the term

[4]A similar procedure, except as to the grouping in classes, was employed by Dallenbach in (*4*). I am informed that it appears sporadically in the field of educational statistics; but I know of no other author who has used it more than once.

[5]*I. e.*, those of the earlier part of the work, shown in their Table 1, which the later results "corroborate" (207).

'average duration' are not denoted, and since such expressions are so habitually used by readers in the unrestricted sense that their reaction-habits are not likely to be disrupted by the work they are likely to expend on a single paper. And, thirdly, the assertion, taken literally, is irrelevant to a number of passages in the article with which it was connected. Some of these passages are quoted above.

On the other hand, if the term 'average duration' be taken in the unrestricted sense, the assertion quoted is certainly false, for the data from which it was derived actually prove the contrary. This disproof is implied in the fact that the correlation between the gross measures is insignificantly small. I have not seen, thus far, a discussion of the relationship between the coefficient of correlation, r_{xy}, between the gross measures of X and Y and the coefficient of correlation, $r_{m_x m_y}$, between fractional means of simultaneous values of X and Y taken at random. I shall therefore give, without claim to priority, an original proof that $r_{xy} = r_{m_x m_y}$, which should finally dispose of the matter.

Definitions

(1) v, any particular gross measurement made on a variable quantity, such as X or Y.

(2) N, the total number of such measurements, taken at random. In the reasoning which follows N is assumed to be large.

(3) $M = (1/N)\Sigma_0^N v$, the general empirical mean, which, from the value of N, and the manner of sampling, is assumed to be practically equivalent to the 'ideal' mean.[6]

(4) f, the total number of groups in which the N measurements may be fractionated.

(5) n, the number of measurements included in any fractional group.[7]

(6) $m = (1/n)\Sigma_0^n v$, the mean of the measurements included in any fractional group.

(7) $\Delta = v - m$, the deviation of any measurement from the fractional mean.

(8) $d = v - M = (v - m) + (m - M) = \Delta + (m - M)$, the deviation of any particular measurement from the general empirical mean.

(9) $s = \sqrt{(1/n)\Sigma_0^n \Delta^2}$, standard deviation, or quadratic mean deviation, of particular measurements from the fractional mean, m.

(10) $\sigma = \sqrt{(1/N)\Sigma_0^N d^2}$, standard deviation of particular measurements from the general empirical mean, M.

(11) $\sigma_m = \sqrt{(1/f)\Sigma_0^f (m - M)^2}$, standard deviation of fractional means from the general empirical mean.

[6] It should be noted that the restrictions on definitions (2) and (3) are [illegible] of probability to empirical data.

[7] In the treatment which follows it is assumed that the several [illegible] values of n are equal to each other. This method of fractionation is unnecessary to the proof. If a different method be used the relations here calculated would still hold, provided the [illegible] employed where necessary.

(12) $\sigma_m = s \div \sqrt{n-1}$, the standard deviation of the fractional means from the ideal mean; the latter, by restriction on definition (3), being adequately approximated by M, this formula gives the most probable value of σ_m by definition (11), if the empirical value of the latter be unknown.

(13) $r_{xy} = \Sigma_0^N (X - M_x)(Y - M_y)/N\sigma_x\sigma_y$, the product-moment coefficient of correlation between simultaneous particular gross measurements of X and Y.

(14) $r_{m_x m_y} = \Sigma_0^{f} (m_x - M_x)(m_y - M_y)/f\sigma_m\sigma_m$, the product-moment coefficient of correlation between fractional means of simultaneous gross measurements of X and Y taken at random.

To prove that $r_{m_x m_y} = r_{xy}$

In general, if a particular value of Y be not empirically known, its most probable value, Y, may be deduced from the empirical value of X which corresponds to it, according to the regression-equation

$$Y = M_y + r_{xy}\frac{\sigma_y}{\sigma_x}(X - M_x) \qquad \text{(I)}$$

If equation (I) be used in the estimation of N values of Y, on the assumption that $Y = Y$, the resulting errors will be $Y_1 - Y_1$, $Y_2 - Y_2$, ... $Y_N - Y_N$; and the standard error of estimation will be

$$\sqrt{(1/N)\Sigma_0^N (Y - Y)^2}$$

which, according to Yule (*21*), Kelley (*15*) and others, is equal to $\sigma_y\sqrt{1 - r^2_{xy}}$. The factor $\sqrt{1 - r^2_{xy}}$ is sometimes called the "coefficient of alienation."

In the special case in which $r_{xy} = 0$, or in which its sign as well as its value is unknown, the second term in the right member of (I) cannot be used; in which case (I) becomes $Y = M_y$, which is the best guess that can be made in the circumstances, and which is subject to a standard error equal to σ_y.

If the coefficient of alienation be not considerably less than unity, the use of X as a means of estimating the corresponding value of Y from the regression-equation does not appreciably increase the precision of estimation over the precision which attends the bare guess that $Y = M_y$. In such case the two variables X and Y may be said to be *practically* uncorrelated; for regardless of the reliability with which the value of the coefficient of correlation has been determined, the latter is too low to have practical significance.

From equation (I),

$$Y_1 = M_y + r_{xy}(\sigma_y/\sigma_x)(X_1 - M_x),$$
$$Y_2 = M_y + r_{xy}(\sigma_y/\sigma_x)(X_2 - M_x), \ldots\ldots\ldots\ldots \text{and}$$
$$Y_n = M_y + r_{xy}(\sigma_y/\sigma_x)(X_n - M_x)$$

By definition (6), the result of summation and division may be written

$$m_y = M_y + r_{xy}(\sigma_y/\sigma_x)(m_x - M_x)$$

On the assumption that $m_y = m_y$, the last equation becomes

$$m_y = M_y + r_{xy}(\sigma_y/\sigma_x)(m_x - M_x) \qquad \text{(II)}$$

The error implied in this assumption is equal to $m_y - m_y$; hence, if equation (II) be used to estimate f values of m_y, the standard error of estimation will be

$$\sqrt{(1/f)\,\Sigma_0^f\,(m_y - m_y)^2}$$

From fundamental principles in correlation it is evident that equation (II) may be written

$$m_y = M_y + r_{m_x m_y}\left(\sigma_{m_x}/\sigma_{m_y}\right)(m_x - M_x) \tag{III}$$

whence the standard error of estimation of m_y is $\sigma_{m_y}\sqrt{1 - r^2_{m_x m_y}}$.

Inspection of equations (II) and (III) shows that

$$r_{xy}\,\sigma_y/\sigma_x = r_{m_x m_y}\,\sigma_{m_x}/\sigma_{m_y}$$

from which it is obvious that

$$r_{xy} = r_{m_x m_y}$$

in case $\sigma_{m_y} : \sigma_y = \sigma_{m_x} : \sigma_x$,

which remains to be shown.

By definition (8),

$$d^2 = \Delta^2 + 2\,(m - M)\,\Delta + (m - M)^2$$

and by summation

$$\Sigma_0^n d^2 = \Sigma_0^n \Delta^2 + 2\,(m - M)\,\Sigma_0^n \Delta + n\,(m - M)^2$$

The second term in the right member being equal to zero, division by n yields an expression which by definition (9) may be written

$$(1/n)\Sigma_0^n d^2 = s^2 + (m - M)^2$$

By summation between o and f, the last expression gives

$$(1/n)\Sigma_0^f\,\Sigma_0^n d^2 = \Sigma_0^f s^2 + \Sigma_0^f (m - M)^2$$

which upon division by f becomes

$$(1/N)\,\Sigma_0^N d^2 = (1/f)\,\Sigma_0^f s^2 + (1/f)\,\Sigma_0^f\,(m - M)^2$$

By definition (10) the left member is σ^2, and by definition (11) the second term of the right member is σ^2_m; hence the last equation becomes

$$\sigma^2 = (1/f)\,\Sigma_0^f s^2 + \sigma^2_m \tag{IV}$$

By definition (12), and subject to the restriction on definition (3), if any empirical value of s be unknown its most probable value is $\sigma_m\sqrt{n-1}$; hence,

$$s^2_1 = (n - 1)\,\sigma^2_m,$$
$$s^2_2 = (n - 1)\,\sigma^2_m, \ldots \quad \text{and}$$
$$s^2 f = (n - 1)\,\sigma^2_m$$

Summation and division yield

$$(1/f)\,\Sigma_0^f s^2 = (n - 1)\,\sigma^2_m$$

By substitution and collection in (IV)

$$\sigma^2 = n\,.\,\sigma^2_m$$

whence $\sigma^2_m = \sigma^2/n$

and $\sigma_m = \sigma/\sqrt{n}$

Reverting to equations (II) and (III), one may note that since $\sigma_{m_y} = \sigma_y / \sqrt{n_y}$ and $\sigma_{m_x} = \sigma_x / \sqrt{n_x}$ and since, m_x and m_y being means of simultaneous numbers, $n_x = n_y$,

$$\sigma_{m_y} : \sigma_y = \sigma_{m_x} : \sigma_x$$

whence

$$r_{m_x m_y} = r_{xy} \qquad \text{Q. E. D.}$$

Geometrically the facts may be represented thus: if the several values of X be plotted against the corresponding values of Y, and the several values of m_x be plotted against the corresponding values of m_y, the two systems of points will lie within two similar ellipses, whose corresponding axes bear the ratios of $\sqrt{n} : 1$. The effect of correlating fractional means of gross measurements taken at random, instead of the gross measurements themselves, is the same as the effect of grouping the data into classes, by multiplying the two units of measurement by a constant factor. In either case, the resulting coefficient of correlation is the same as the correlation between the gross measures.

It follows from the above exposition that the two coefficients of alienation $\sqrt{1 - r^2_{xy}}$ and $\sqrt{1 - r^2_{m_x m_y}}$ are also equal. Since in the experiment of Cassel and Dallenbach $r_{m_x m_y} = r_{xy} = -0.25$, approximately, the fractional mean m_y of a group of numbers taken at random, designating the reactor's estimates of the clearness of his content, can be deduced from the mean m_x of the corresponding reaction times, subject to a standard error of estimation which is 0.97 times as large as the error attending a bare guess from the average.[8]

The conclusion is inescapable. Under the conditions of the experiment the tendency of the larger numerical values of 'clearness' to be associated with the shorter reaction-times was so weak that knowledge of a particular reaction-time, or of the mean of a number of reaction-times, does not enable prediction, with tolerable accuracy, of the unknown clearnesses[9] asso-

[8]The standard deviation of the empirically obtained times (X) in Cassel and Dallenbach's Table 2, from the values calculated from the regression-equation, is approximately 0.965 times as large as the standard deviation from their general empirical average. This value agrees well with the theoretical value of 0.97. In making the calculation I assumed that each of the standard deviations from the partial means given in the table is 1.25 times the mean variation from those means, as in a normal distribution.

[9]The term "clearnesses" is used intentionally. If the averages of the distributed times in the authors' Table 2 represent distributions which are approximately normal, then from Chauvenet's criterion we should expect the following. In the highest class of clearnesses reported, *i. e.*, "95," are some measurements associated with times longer than the time associated with the lowest degree, "75." In the class of clearnesses designated as "80", which is the lowest class containing more than one measurement,

ciated with them. The latter have to be ascertained by direct methods.[10].

Quotations 2 and 3, p. 608 of this paper, and the portion which I omitted from the quotation from 3, 307, assert that the degree of clearness can be reliably inferred from the mean variations from the partial averages of times. This point may be disposed of without prolonging the discussion. If averages of distributed times are meant, the objections cited on pp. 608f of this paper apply. If times, whether distributed or undistributed, are referred to, the assertion is an inconsequence of the data exhibited.

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(1) E. E. Cassel, and K. M. Dallenbach, The effect of auditory distraction upon the sensory reaction, this JOURNAL, 29, 1918, 129-143.

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(3) K. M. Dallenbach, Dr. Johnson on reaction-time experiments, *ibid.*, 35. 1924, 305-307.

(4) K. M. Dallenbach, The measurement of attention, *ibid.*, 24, 1913, 465-507.

(5) D. Jackson, The algebra of correlation, *Am. Math. Mo.*, 31, 1924, 110-121.

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(9) L. R. Geissler, The measurement of attention, this JOURNAL, 20, 1909, 473-529.

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are some measurements paired with times longer than the average time associated with the highest [illegible] clearnesses, "95." The times within a range of 0.064 sec. are associated [illegible] every grade of clearness reported. Obviously, then, the most representative value of clearness, Y, associated with a particular value of time, X, and *a fortiori* with n times whose mean is m_x, is m_y.

It is admitted that these ranges, as well as other characteristic features of the assembled data, have been estimated from parameters computed by approximate methods, which are strictly applicable only to normal distributions. But the empirical data, in the form of a scatter-table or scatter-graph, were not made available—a fact that has caused me much regret.

[10]Even the distributed means of times, despite their perfect correlation with clearness, lack in some cases the indicative value which the authors claimed for them. The highest correlation reported was for the results obtained with the tuning-fork as distractor, in Table I. Its value is given as [−] 0.999. The two highest classes of clearness contain 98% of the measurements. The difference between the distributed averages of times is but 1.3 times its probable error, and in absolute units is but 0.0013 sec. This difference is smaller than the limit of accuracy within which the Hipp chronoscope can *measure* the time required for a free body to fall from a constant height.

In the procedure used to get these correlations there are some peculiarities which are interesting though not relevant to the main question, [illegible] the procedure itself is fundamentally invalid. The Pearson formula was applied to distributions which contained too few classes to render it applicable. (In the instance cited but two classes were used.) This fact by itself insures a large coefficient. In some instances classes which deviated widely from the central class were eliminated because of the small number of measurements which lay within them.

(10) D. Jackson, The algebra of correlation, *Am. Math. Mo.*, 31, 1924, 110-121.

(11) H. M. Johnson, Reaction-time measurements, *Psy. Bull.*, 20, 1923, 562-589.

(12) H. M. Johnson, Speed, accuracy and constancy of response to visual stimuli as related to the distribution of brightnesses over the visual field, *J. Exp. Psy.*, 7, 1924, 1-44.

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(15) T. L. Kelley, *Statistical Methods.*

(16) E. B. Titchener, *The Psychology of Feeling and Attention*, 1908.

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(18) H. Woodrow, Outline as a condition of attention, *J. Exp. Psy.*, 1, 1916, 23-29.

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(21) G. U. Yule, *An Introduction to the Theory of Statistics*, 3d ed.

THE MEASURE OF APPROXIMATION OF DATA TO THE PHI-GAMMA HYPOTHESIS

By Gilbert J. Rich

Urban[1] recently called attention to observations by Hoisington and by Thomson which indicate that the agreement between the observed values in a psychophysical experiment and the values computed according to the *phi-gamma* hypothesis tends to increase with practice, and suggested that the relationship could be worked out in detail from existing data. Fernberger,[2] acting upon this suggestion, computed the sums of the squares of the differences between observed and calculated values for two sets of experiments upon lifted weights. It should be noted, however, that the conclusions of Hoisington and of Thomson were arrived at by applying different criteria of approximation, and that Fernberger used in his calculations only the method of computation employed by Hoisington, not the more adequate method of Thomson.

Hoisington[3] calculated the sums of the squares of the deviations of the percentages obtained experimentally from the theoretical percentages obtained according to the *phi-gamma* hypothesis. Properly to understand the differences between the two methods of measuring agreement, several points with respect to this criterion must be noted. (1) The raw or unweighted squares of the deviations are used in the computation. (2) The sum of the squares is a measure of *badness* of fit of the theoretical curve to the data, since a low value indicates a close approximation and a high value a poor approximation. (3) There is no standard of reference when this criterion is used. The final values obtained range from zero (perfect agreement) upward to a possible maximum of as many units as there are comparison stimuli. The values for different experiments are comparable only in case the number of comparison stimuli is the same (although they may be made comparable by division in each case by two less than the number of stimuli, as in the work of Williams[4]). (4) Finally, the number of observations made does not enter into the calculation, so that the resulting

[1]F. M. Urban, The Approximation of Actual Data to the Phi-Gamma Hypothesis, this Journal, 34, 1923, 496-497.

[2]S. W. Fernberger, The Approximation of Actual Data to the Phi-Gamma Hypothesis, *ibid.*, 34, 1923, 498-500.

[3]L. B. Hoisington, An Example of the Fractionation of Data from the Method of Constant Stimuli for the Two-Point Limen, *ibid.*, 28, 1917, 588-596.

value becomes no more delicate as an indication of agreement as the number of experiments increases.

Thomson's[5] conclusions as to the effect of practice were arrived at by means of the application of his adaptation of Pearson's method for testing for goodness of fit[6]. This differs from the older and simpler method (of Urban) in every one of the respects noted. (1) The bases of computation are the weighted instead of the unweighted squares of the deviations between the experimental and the theoretical percentages. This difference is of utmost importance, for it alone is responsible for the divergent results obtained by the two procedures. (2) The final value obtained, P, is not itself the sum of the squares, but an inverse function (ogival in character) of that sum. Thus, P is a measure of *goodness* of fit. It is high when the approximation is close and low when it is poor. (3) P is not a random value but is a percentage probability, varying from a possible minimum of zero to a maximum of 100% (perfect agreement). It measures the probability of obtaining in a random sample of the size taken (number of observations) a curve as bad as or worse fitting than that actually determined by experimentation, if the hypothesis used were true. (4) The sum of the squares, before being used in the calculation of P, is multiplied by the number of observations made at each comparison stimulus. This gives a final value that is dependent upon the extent of the experiment, becoming a more delicate test as the number of experimental series increases. That it should do so is inherent in the definition of P as just given, which takes into consideration the size of the sample.

We cannot enter here into a consideration of the relative merits of the two criteria of approximation. Thomson[7] has fully treated the underlying mathematics and, it would seem, has demonstrated that his method is the more logical of the two and the more adequate for scientific purposes. Out interest lies in the fact that they are essentially different. We should expect, if they agreed, to find that, in comparing closeness of approximation of two sets of data, wherever the figure obtained by Urban's method (the method used by Hoisington and Fernberger) was the larger for one set, the value of P according to Thomson's method would be the smaller for that same curve.

[4]H. D. Williams, On the Calculation of an Associative Limen, *ibid.*, 29, 1918, 222.

[5]W. Brown and G. H. Thomson, *The Essentials of Mental Measurement*, 1921, 90ff.

[6]K. Pearson, *Tables for Statisticians and Biometricians*, 1914, xxxiff.; G. H. Thomson, The Criterion of Goodness of Fit of Psychometric Curves, *Biometrika*, 12, 1919, 216-230; W. Brown and G. H. Thomson, *op. cit.*, 77ff.

[7]G. H. Thomson, *loc. cit.*

As will be shown below, this is by no means always the case. The reason for the discrepancy is to be found in the difference between the use of unweighted and of weighted squares of the deviations. None of the other differences between the two methods would account for inversions in cases where the number of observations does not differ.

To apply the more rigorous criterion of goodness of fit to the present problem of the effect of practice upon the approximation of data to the *phi-gamma* hypothesis, we have computed the Pearson-Thomson P for the same experimental data for which Fernberger calculated the raw sums of the squares.[8] The resulting values are given in Tables 1 and 2, which are numbered to correspond with the Tables in Fernberger's paper (corresponding to Urban's Table 18 and Table 19, respectively).

TABLE 1

Group Number	Lighter Judgments	Heavier Judgments
10	.953	.667
11	.963	.870
12	.835	.968
13	.326	.84[illegible]
14	.399	.292
15	.897	.557
16	.065	.685
17	.443	.968
18	.423	.570
19	.592	.270
20	.561	.435
21	.672	.036
22	.498	.353
23	.670	.113
24	.926	.244
25	.003	.820
26	.945	.291
27	.995	.985
28	.520	.990
29	.948	.835
30	.876	.998
31	.487	.350
32	.738	.993
33	.785	.023
34	.707	.622
35	.747	.630
36	.493	.529
37	.627	.072

TABLE 2

Group Number	Lighter Judgments	Heavier Judgments
1	.349	.846
2	.967	.318
3	.992	.895
4	.106	.387
5	.005	.848
6	.817	.889
7	.399	.356
8	.756	.989
9	.398	.662
10	.059	.094
11	.326	.853
12	.788	.497
13	.359	.303
14	.969	.833
15	.700	.861
16	.931	.632
17	.952	.967
18	.751	.969
19	.695	.632
20	.837	.344
21	.556	.833
22	.128	.889
23	.557	.862
24	.790	.960
25	.916	.248
26	.757	.030
27	.945	.951
28	.337	.899

A comparison of these figures with those obtained by Fernberger shows that, as stated above, the two criteria of approxi-

[8] Dr. Fernberger kindly provided me with his figures for squares of the deviations and the theoretical percentages (involving over half of the calculation necessary for the results given here). For the remaining computations I am indebted to my wife, Enid Rosabelle Rich, and to Miss Verzie V. Pickens.

mation sometimes yield divergent results. It will be remembered that one criterion (Urban's) is a measure of poorness, the other (Thomson's) of closeness of agreement, so that we should expect that, for any pair of groups considered, the sum of the squares should be the larger for one group and P the larger for the other group. In the majority of cases, this relation holds. But a few exceptions will test the rule and prove it to be untrue. For example, in Table I, Lighter Judgments, the sum of the squares is larger for Group 37 than for Group 36. But P is also the larger for Group 37. Thus, by one criterion Group 37 is a poorer approximation to theory than Group 36, while by the other criterion it is a better approximation. Other examples (chosen at random) are Groups 10 and 11, Table 1, Heavier; Groups 4 and 5, Table 2, Lighter; Groups 14 and 15, Table 2, Heavier. It is needless further to multiply examples. The lack of agreement between the two criteria is self-evident.[9]

We may next consider the effect of practice as shown in these Tables. The variation from group to group is entirely too great to admit of any conclusion being drawn by inspection of the figures. Boring[10] attempted to find whether or not any general tendency existed in the midst of the variability of Fernberger's results. He fitted straight lines by the method of least squares to the values given by each set of experiments, and found that in two cases the slope of the line was negative (increased approximation with practice) and in the other two cases positive (decreased approximation). In the same way, we have adjusted straight lines to our four sets of figures by the method of least squares. But in this case a positive instead of a negative slope will indicate that the agreement becomes better as practice increases.

Table	Class of Judgment	Equation of Adjusted Line
1	Lighter	$y = .0028x + .6050$
1	Heavier	$y = -.0063x + .6638$
2	Lighter	$y = .0071x + .5304$
2	Heavier	$y = .0020x + .6429$

[9] *Cf.* J. L. Ernst, F. E. Smith, L. R. Moessner, E. S. Rudisill and M. J. Atwater, this Journal, 35, 1924, 255-261. These investigators tested the fit of a number of curves to mnemometric data by both methods, and their results show a number of cases in which the two criteria lead to opposite conclusions.

The probabilities (P) found above are much higher than those quoted by Thomson (Brown and Thomson, *op. cit.*, Chap. IV), which are also for lifted weights. But Thomson uses data for 450 observations at every comparison stimulus, while our groups are of 50 observations each, and the value of P depends upon the size of the sample (number of observations).

[10] E. G. Boring, Is there a Generalized Psychometric Function? this Journal, 35, 1924, 75-78.

In the equation of these lines, x is the successive group (of 50 series of observations) as given in the 1st, and y the value of P as given in the 2nd and 3rd columns of Tables 1 and 2.

We are concerned with the slope of the line, which is given by the coefficient of x. It will be seen at once that this coefficient is positive in three cases and negative in one case. What significance may be attached to this fact? Probably none at all. While it is true that in 75% of the cases studied the closeness of approximation of the data to the *phi-gamma* hypothesis increases with practice, yet it must be remembered that only four cases have been studied. And four cases are surely not enough to justify the use of probabilities. We cannot even say that the chances are 3:1 that approximation increases with practice. The sample taken for study is too small to admit of generalization.

It is to be noted that we find three positive slopes out of the four, while Boring found two negative slopes. Here, again, the divergence between the criteria used is evident. In one case they show opposite results of practice from the same experimental data. The slope of the line adjusted to the values for the heavier judgments of Table 2 is positive for both Boring and us, whereas it should be positive in one case and negative in the other. The result obtained is a function of the mathematical procedure (ultimately of the basic assumptions upon which the procedure is built). One method of calculation shows increased approximation with practice, the other method decreased approximation.

Boring[11] suggests that where the scatter is so great it is unfair to represent the practice curve by a straight line. Some form of combination of adjacent groups seems to be called for. We have attempted a rough sort of combination by averaging the values of P for the two halves of the experiment (of 14 groups each). For comparison, similar averages of the sums of the squares are also given.

Table	Judgment	P		Sum of Squares	
		1st 14	2nd 14	1st 14	2nd 14
1	Lighter	.592	.700	.0143	.0117
1	Heavier	.544	.599	.0122	.0109
2	Lighter	.520	.704	.0136	.0091
2	Heavier	.628	.720	.0112	.0092

It will be noted that in every case P is larger and the sum of the squares smaller for the second 14 fractions than for the first 14. But we must be most cautious in attaching meaning to this fact, if, indeed, any meaning may be attached to it. The

[11] E. G. Boring, *op. cit.*, 76.

mathematical procedure must be kept in mind. Although each value given is for 700 judgments (14 × 50) at every comparison stimulus, it does not represent the closeness of fit of the psychometric function for the entire 700 series, but the average closeness of fit of 14 separate psychometric functions each for 50-series, a very different matter. Once more we see the effect of method of treatment upon the result obtained. The general trend of the values of P for the heavier judgments of Table 1 is to decrease, yet the second 14 values have a higher average than the first 14. A similar situation holds in two of the four cases when the sums of the squares are considered. Which method of smoothing out the irregularities (least squares or averaging) is the more legitimate scientifically? We do not know.[12]

No general conclusion as to the effect of practice seems possible from the data at hand. Some method of treating the results so that the extreme variability will not mask any possible effect of practice is obviously necessary. One method tried, least squares, leads to equivocal results and is objectionable because it attempts to fit a straight line to a progression that is probably non-linear. The other method used, averaging, yields consistent findings but its mathematical soundness may be doubted for a number of reasons. A more direct method of combination, not open to any of these objections, would be to regroup the original experimental results (percentages of judgments in each category) into groups of, say, 500- or 1000-series instead of 50-series, and then to calculate the limens, measures of precision and P's for these larger fractions. The results should be instructive since the variability of the finer fractionation that has been used ought to be minimized, and the number of limens involved is not so great as to make the work unduly laborious.

The point that we wish to emphasise is that there are two measures of approximation which at times yield divergent results. They are based upon somewhat different mathematical assumptions. The more nearly the basic assumptions of a method square with the facts which are to be treated mathematically, the more adequate will the resulting procedure be to the problems that are to be solved. In this respect, Thomson seems to have shown the superiority of his adaptation of Pearson's test for goodness of fit as a measure of the approximation of experimental data and hypothesis.

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[12]We have also computed the averages of P for quarters (7 groups) and sevenths (4 groups) of the total. In these cases the variability has not been smoothed out and the progression is uneven. These averages, therefore, are not of sufficient importance to be given here.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

Communicated by E. B. TITCHENER

LXXIII. THE DETERMINATION OF MEMORY SPAN BY THE METHOD OF CONSTANT STIMULI

By J. P. GUILFORD and KARL M. DALLENBACH

The many investigators of the problem of immediate memory have not only worked with a variety of materials and procedures, but have also complicated matters by their use of different methods[1] of evaluating their data. So greatly do these methods differ that there is little in common between the values reported.

A review shows that the methods used in the evaluation of the data fall into three groups; standards of comparison are determined by 'raw' scores, that is, by the number of items correctly reproduced; by 'weighted' scores, credit being given for partial successes; and by 'calculated' scores, derived by treating the data by certain accepted formulae. Every one of these groups shows a number of variants.

Raw Scores. There are 16 different methods in which the reports have been evaluated from the raw scores.

(1) The earliest method, and that most frequently followed, is that of Jacobs,[2] who in 1887 began the investigation of immediate memory. Jacobs presented series of materials of various lengths, gave 2 series of every length, and accepted the longest series correctly reproduced in either trial as the limit or "span of prehension." No account was taken of the series in which transpositions, misplacements, or partial reproductions were given. This method was used also by Galton;[3] by Binet and Simon in their 1905[4] scale for evaluating the reports for 3 digits, and in their 1908[5] and 1911[6] scales for determining immediate memory for digits of 4 year old children; by Yerkes and Bridges,[7] and by Humpstone,[8] Mitchell,[9] Ide,[10] Leaming,[11] Starr,[12] Brotemarkle,[13] and Clark.[14]

(2) The first variation from Jacobs' method was introduced by Münsterberg[15] in 1890. He had series of 4-10 letters read to him, 10

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[1] A. Pohlmann, *Experimentelle Beiträge zur Lehre vom Gedächtnis*, 1906, 17-31; 42-56. G. M. Whipple, *Manual of Mental and Physical Tests*, 2, 1915, 150-204. S. H. Watkins, Immediate Memory and its Evaluation, *Brit. J. Psych.*, 7, 1915, 322-329.

[2] J. Jacobs, Experiments on 'Prehension', *Mind*, 12, 1887, 75-79.

[3] F. Galton, Supplementary Notes on "Prehension" in Idiots, *Mind*, 12, 1887, 79-82.

[4] A. Binet and T. Simon, Methodes nouvelles pour le diagnostic du niveau intellectuel des anomaux, *L'Année psychologique*, 11, 1905, 191-244.

[5] Le developpement de l'intelligence chez les enfants, *ibid.*, 14, 1908, 1-94.

[6] Mesure du niveau intellectuel, *ibid.*, 17, 1911, 145-201.

[7] R. M. Yerkes and J. M. Bridges, *A Point Scale for Measuring Mental Ability*, 1915, 142-143.

[8] H. J. Humpstone, *Some Aspects of the Memory Span; a Study in Associability*, 1917, 10.

[9] D. Mitchell, Variability in Memory Span, *Jour. Educ. Psych.*, 10, 1919, 448.

[10] G. G. Ide, The Educability Level of Five-Year-old Children, *Psych. Clinic*, 13, 1920, 146-172.

[11] [illegible] Leaming, [illegible] Fifteen Year Old [illegible], *ibid.*, 14, 1922, [illegible].

[12] A. S. Starr, An Analytical Study of the Intelligence of a Group of Adolescent Delinquent Girls, *ibid.*, 14, 1922, 148.

[13] R. A. Brotemarkle, Some Memory Span Test Problems: An Analytical Study at the College-Adult Level, *ibid.*, 15, 1924, 229-258.

[14] A. S. Clark, Correlation of the Auditory Digit Memory Span with General Intelligence, *ibid.*, 15, 1924, 259-260.

series of every length, beginning with the smallest and progressing to the largest, and measured his span by the longest series that he correctly reproduced in any one of the 10 trials.

(3) Bolton[16] in 1892 introduced the percentile measure. 12 trials at every series-length (digits of 5-8 places) were given the *S*s, and from the proportion of correct trials the probability of error for every trial for every series-length was computed and was used as a standard of age, school-grade, intelligence, etc.

(4) Percentile measures were also used by Bourdon[17] in 1894. The percentage of correct trials for every series-length was calculated for the different experimental groups and was used for comparative purposes.

(5) Hawkins[18] in 1897 used the percentile measure in a third manner. Series of constant lengths were presented to the *S*s, and the percentages of items correctly reproduced, apparently without regard to the serial order, were used for comparison. This method was also used by Pohlmann.[19]

(6) Binet and Henri[20] in 1895 took as their standard of comparison the number of repetitions of a series of 12 digits necessary for an errorless reproduction.

(7) Johnson,[21] also in 1895, accepted as the standard "the limit of the power of the memory to reproduce from a single hearing or seeing, immediately and without error, a succession of figures, nonsense syllables, or letters, etc." Johnson's method of determining the span is very similar to Jacobs'; it differs in that only one trial, instead of two, was given at every series-length. Gates[22] used the same method as Johnson, but, as he found that a *S* often failed at one length of series and succeeded at a longer length, he modified the method by adopting the rule "When there is but one failure followed by a success the latter is taken as the span."

(8) An eighth method was employed by Netschajeff[23] in 1900. Netschajeff presented to his *S*s series of 12 impressions (objects, noises, numbers and words denoting various kinds of ideas—visual, verbal, tactual, emotional and abstract) and took, as the measure of his reports, the number of items correctly reproduced. The method was also used by Lobsien,[24] who reduced the number of serial impressions to 9; and by Reed,[25] who used it as one of his alternative measures.

(9) A method in which the correct reports and errors were systematically classified was introduced by Smith[26] in 1903. 3 series of letters of various lengths were read to the *S*s, and the reports from immediate memory were classified for every length of series for every experimental group of *S*s in 10 different ways. Averages were figured for the number

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[15]H. Münsterberg, Die Association successiver Vorstellungen, *Zeits. f. Psych. u. Phys. d. Sinnesorg.*, 1, 1890, 99-107.

[16]T. L. Bolton, The Growth of Memory in School Children, this JOURNAL, 4, 1892, 363.

[17]B. Bourdon, Influence de l'âge sur la mémoire immédiate, *Rev. philos.*, 38, 1894, 148-167.

[18]C. J. Hawkins, Experiments on Memory Types, *Psych. Review*, 4, 1897, 290.

[19]*Op. cit.*

[20]A. Binet and V. Henri, La psychologie individuelle, *L'Année psychologique*, 2, 1895, 442.

[21]G. E. Johnson, Contribution to the Psychology and Pedagogy of Feeble-Minded Children, *Ped. Sem.*, 3, 1895, 268, 269.

[22]A. I. Gates, Mnemonic Span for Visual and Auditory Digits, *Jour. Exp. Psych.*, 1, 1916, 393, 394.

[23]A. Netschajeff, Experimentelle Untersuchungen über Gedächtnisentwickelung bei Schulkindern, *Zeits. f. Psych.*, 24, 1900, 321-351.

[24]M. Lobsien, Experimentelle Untersuchung über die Gedächtnisentwickelung bei Kindern, *ibid.*, 27, 1902, 40.

[25]H. B. Reed, A Repetition of Ebert and Meumann's Practice Experiments on Memory, *Jour. Exp. Psych.*, 2, 1917, 320.

[26]W. G. Smith, The Range of Immediate Association and Memory, *Arch. of Neurol.*, 2, 1903, 767-805; also A Comparison of Some Mental and Physical Tests in Their Application to Epileptic and to Normal Subjects, *Brit. J. Psych.*, 1, 1905, 243f, 248.

of letters (1) correctly reproduced and rightly placed; (2*a*) correctly reproduced, but transposed with group; (2*b*) correctly reproduced, inversion, right position; (2*c*) correctly reproduced, inversion, wrong position; (3) correctly reproduced, wrongly placed; (4) omitted; (5) inserted; (6) repeated; (7) short number of letters given; and (8) excess in number given.

(10) Binet and Simon, as has already been noted, used the method of Jacobs in establishing the memory span at the lower ranges. In the higher reaches, however, in all of their scales, a different method was used. 3 series of every length were given the *S*s, and 1 correct reproduction out of the 3 trials marked a success. This is the method of most mental tests. It is continued by Terman[27] and Kuhlmann[28] in their revisions of the Binet-Simon scale.

(11) As in the experiments of Binet and Simon, Hentschel,[29] in 1912, presented 2 series of each of the shorter lengths (2 and 3 consonants), and 3 series of every one of the longer lengths (4-7 consonants). He took as his standard, however, the greatest number of consonants reproduced in any one of the 3 trials of the various series. Thus, if a *S* reproduced one of the series of 3 consonants correctly, and 3 consonants in the 4-member series, and 4 in the 5-member series, Hentschel regarded the span of that particular *S* as 4.

(12), (13) Watkins[30] in 1915 used 2 new methods: one with numerals and the second with nonsense syllables. He presented both kinds of materials visually in rows of from 2-10 units, 5 rows of every length for the numerals, and 10 rows of every length for the syllables. "The threshold is determined by the longest complete row of units which can be immediately and correctly repeated." This statement, however, is not clear; for it is apparent from Table I, in which he gives the thresholds and the number of correct reproductions for every series-length for every *S* (p. 328), that he does not mean that the threshold is determined by the 'longest row correctly repeated' in one of the 5 or 10 trials, because in no instance is the threshold placed at this value. His determination of the threshold appears to be based, for numerals, upon that value which is correctly repeated in 4 of the 5 trials; and for syllables, upon that value which is correctly repeated in 6 of the 10 trials.

(14) Mitchell[31] in 1919 used likewise 2 methods of grading his reports. He presented 2 series of 3-10 place numbers and scored the reproductions by Jacobs' method, as we have already seen, and by a second and new method in which he determined the score by "the longest series, in both attempts at which the child was successful at reproduction and in no series previous to which he had failed."

(15) The next method was introduced in 1923 by Starr,[32] who presented 4 series of digits of every length, beginning with the 3-place numbers and progressing with the success of the *S* to the higher. "When the response was correct another series increased by one digit followed, and this procedure was repeated until the child failed to recall correctly. Two correct responses in four trials were required, in order to eliminate the possibility of chance."

(16) The most recent method is that of Easby-Grave,[33] 1924. Using series of digits of varying lengths, she determined the memory span by the position of the first true failure. "If the child fails on a series of digits,

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[27] L. M. Terman, *The Measurement of Intelligence*, 1916.
[28] F. Kuhlmann, *A Handbook of Mental Tests*, 1922.
[29] M. Hentschel, Die Gedächtnisspanne, *Zeits. f. päd. Psych.*, 13, 1912, 562-579.
[30] S. H. Watkins, Immediate Memory and its Evaluation, *Brit. J. Psych.*, 7, 1915, 319-349.
[31] D. Mitchell, Variability in Memory Span, *Jour. Educ. Psych.*, 10, 1919, 445-457.
[32] A. S. Starr, The Diagnostic Value of the Audito-Vocal Digit Memory Span, *Psych. Clinic*, 15, 1923, 61-85.

he is given another series of the same length. If he also fails the second, he is given a trial at the next longer series. If he fails this we consider that the first failure showed the true limit of his span."

A summary and classification of the methods of evaluating the 'raw' scores reveals the following types and sub-types. Immediate memory is measured:

I. By the length of the longest series correctly reproduced
- (*a*) in 1 trial for every series-length (7th method);
- (*b*) in 2 trials " " " " (1st ");
- (*c*) in 3 " " " " " (10th ");
- (*d*) in 10 " " " " " (2nd ");
- (*e*) 2 times in 2 trials for every series-length (14th method);
- (*f*) 2 " " 4 " " " " " (15th ");
- (*g*) 4 " " 5 " " " " " (12th ");
- (*h*) 6 " " 10 " " " " " (13th ").

II. By the probability of error at every series-length (3rd method).

III. By the percent. of correct reproductions
- (*a*) of series (4th method);
- (*b*) of items (5th method).

IV. By the number of items correctly reproduced
- (*a*) average (8th method);
- (*b*) greatest number in one of 3 trials (11th method).

V. By the number of repetitions necessary for an errorless reproduction (6th method).

VI. By a qualitative analysis and classification of the correct reports and the errors (9th method).

VII. By the first true failure (16th method).

Weighted Scores. The methods of evaluating the reports from the 'raw' scores did not take into account the manner in which the reproductions were given. It was for the correction of this defect that graded systems of marking were adopted. 9 different methods of weighting the *Ss*' reports have been employed.

(1) (2) Smith[34] in 1895 introduced the method of weighting. He used two systems which complemented each other; "each took into account what the other neglected." The first method consisted merely in counting the errors of (*a*) omission, (*b*) addition, and (*c*) transposition. "Each error has the same weight attached to it and counts 1; the total number of errors gives a basis for estimating the work of memory in any instance." The second method consists in an arbitrary assignment of marks; 1 mark was credited for every unit "given in an entirely wrong position, or its position is quite unknown;" 2 marks for every unit whose position was only imperfectly known, *e. g.*, "the letter is put in the right line, or in its correct place in a group of letters which is in a wrong position"; and 3 marks were credited when "everything is right." Comparisons of the scores thus obtained were made among the *Ss*. This later method was used by Winch[35] in both of his studies of immediate memory.

(3) Ebbinghaus[36] in 1897 likewise computed his results by 2 different methods. The first, a rough and ready method, consisted merely in counting the incorrect series, without regard to the number or kind of error.

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[33] C. Easby-Grave, Tests and Norms at the Six Year Old Performance Level, *ibid.*, 15, 1924, 268-269.

[34] W. G. Smith, Relation of Attention to Memory, *Mind*, 4, 1895, 51, 52.

[35] W. H. Winch, Immediate Memory in School Children, *Brit. J. Psych.*, 1, 1904, 127-134; also Immediate Memory in School Children, *ibid.*, 2, 1906, 52-57.

[36] H. Ebbinghaus, Über eine neue Methode zur Prüfung geistiger Fähigkeiten, *Zeits. f. Psych.*, 13, 1897, 423.

The second, the third weighted method to be proposed, took account of these factors. Ebbinghaus counted every incorrect item and omission as 1 error and every transposition as 1/2 error, added together these values, and took the sum as a comparative measure of his *S*s immediate memory. Ritter[37] subsequently used the same procedure.

(4) Ebert and Meumann[38] in 1904 introduced a fourth method. They counted omissions and additions as 4/4 error, displacements in the series of more than one place as 3/4 error, displacement of one position as 2/4 error, and a correction as 1/4 error. This system of marks was later used, for comparative purposes, by Reed.[39]

(5) Meumann[40] in 1905 used another method. He counted an omission and a totally incorrect substitution as 3/3 error; a transposition of 2 places and a substitution which was acoustically similar to the given item as 2/3 error; and a transposition of 1 place, and for the youngest children every transposition, as 1/3 error.

(6) Burt[41] in 1909 used a method of marking based, he says, on Meumann's system; in fact it more nearly resembles the converse of Ebert and Meumann's method. "Each word correctly reproduced in its correct place counted 4, a correct word misplaced counted 3 if its position was altered by only one place, and 2 if removed by more than one degree; an incorrect word, if either initial consonant sound, or final consonant sound, or medial vowel sound alone was incorrectly altered, counted 3, if in its right place; 2, if one place removed, and so on; if two such components were altered, it counted 2 if rightly placed, and 1 if wrongly placed; the omission of a word, or the substitution of an extraneous word, counted 0."

(7) Pyle,[42] in 1913, adopted the simple method of giving one credit for every item correctly reproduced and a second credit for every item placed in its correct position.

(8) Dallenbach[43] in 1914 gave one credit for every item correctly reproduced and deducted from the total 1/2 credit for every error of transposition or insertion.

(9) Because the rules thus far devised made no allowance for crediting a correct sequence of items in the wrong part of a series, Woolley and Fischer[44] in 1914 introduced a new set of rules and system of weights: a correct digit in the correct place counts 2; in series in which there is but one mistake, whether of omission, addition or misplacement, 2 is deducted from the possible score; in series in which more than one error occurs (*a*) 2 is counted for every correct digit in the correct place, and 1 for every correct digit one place removed; (*b*) no credit is given for a correct digit more than one place removed unless it forms part of a correct sequence of 3 or more digits, in which case a credit of 2 is given for all except the first digit, which is given no credit if it is more than 1 place removed, and a credit of 1 if it is but one place removed.

The methods of weighting may be classified into three types:

I. the negative methods, in which only the errors are weighted (1st, 3rd, 4th and 5th methods);

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[37] C. Ritter, Ermüdungsmessungen, *ibid.*, 24, 1900, 406.

[38] E. Ebert and E. Meumann, Über einige Grundfragen der Psychologie der Übungsphänomene im Bereiche des Gedächtnisses, *Arch. f. d. ges. Psych.*, 4, 1904, 11.

[39] H. B. Reed, A Repetition of Ebert and Meumann's Practice Experiment on Memory, *Jour. Exp. Psych.*, 2, 1917, 315-346.

[40] E. Meumann, Intelligenzprüfungen a[illegible], 1, 1905, 63 f, 67 f.

[41] C. Burt, Experimental Tests, *Brit. J. Psych.*, 3, 1909, 142.

[42] W. H. Pyle, *The Examination of School Children*, 1913.

[43] K. M. Dallenbach, The Effect of Practice upon School Children, *Jour Educ. Psych.*, 5, 1914, 334; also The Effect of Practice upon Feeble-Minded, *ibid.*, 10, 1919, 61-82.

[44] H. T. Woolley and C. R. Fischer, Mental and Physical Measurements of Working Children, *Psych. Mon.*, 18, 1914, 124.

II. the positive methods, in which only the correct reports are weighted (2nd, 6th and 7th methods); and

III. the mixed methods, in which the correct reports are weighted positively and the errors negatively (the 8th and 9th methods).

Calculated Scores. The methods of weighting are at best but arbitrary attempts to take account of errors and partial successes. The assignment of credits is based upon the assumption that every member of a series has an equal reproductive place-value; and this assumption is not true. The first and last members of a series, as experiments[45] have repeatedly shown, are best retained; those in the middle lie at positions of disadvantage. It is therefore an error to weight equally all the items of a report. This fact has been recognized, and two attempts have been made to avoid it. The first was introduced by Krueger and Spearman[46] in 1907 and the second by Wagner[47] in 1913. In both of these studies, correlations, based on the place-values, were calculated between the order of the impressions as reproduced and as presented. Krueger and Spearman used Spearman's 'footrule' formula, and Wagner a formula derived especially for the problem. Both methods demand separate consideration for every report of every *S*. The amount of labor involved, as Mitchell[48] has pointed out, is prohibitive where many reports have to be considered.

27 methods, of all degrees of accuracy and complexity, used in determining the efficiency of immediate memory,—and no accepted principle underlying them! It was with the hope of supplying such a principle, and of showing how the methods of psychophysics may be used in solving the problem, that the present Study was undertaken. We propose to show, as Fernberger[49] has already done for the problem of range of attention, that the principle of the statistical limen is a more reliable and more easily determined measure than any thus far proposed.

The limen of immediate memory or the memory span is defined as that length of series which has the probability 0.5 of being retained; in other words, that length of series which is as likely to be remembered as not. The method proposed for determining this statistical value is the method of constant stimuli.[50] The data of many of the Studies mentioned in the above review are susceptible to this treatment, and would yield memory-spans as thus defined. We, however, for purposes of illustrating the principle and method, have conducted two tests: an intensive study upon a few *S*s; and an extensive study upon a large class.

In both Studies series of digits of various lengths, constructed according to accepted methods,[51] were read to the *S*s, without rhythm and at a constant rate, as timed by a soundless metronome, of one digit in 0.7 sec.

In the intensive study, series of 6-12 place numbers were presented, 100 series of every length, in haphazard order to 5 *S*s, who, upon a signal given immediately after the reading of the last digit, wrote what they retained upon specially prepared report sheets. These sheets were so constructed that the *S*s could not see, in writing one report, what they had

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[45]T. L. Bolton, *op. cit.*, 379; A. Binet, La mémoire des nots, *L'Année psych.*, 2, 1894, 12.

[46]F. Krueger and C. Spearman, Die Korrelation zwischen verschiendenen geistigen Leistungsfähigkeiten, *Zeits. f. Psych.*, 44, 1907, 71; cf. also G. M. Whipple, *op. cit.*, 161 f., for a description of the method based on an explanation by Spearman.

[47]P. Wagner, Über die rechnerische Behandlung der Ergebnisse bei der Prüfung des unmittelbaren Behaltens, *Zeits. f. päd. Psych.*, 14, 1913, 60-63; *cf.* also S. H. Watkins, *Brit. Jour. Psych.*, 7, 1914, 324ff.

[48]D. Mitchell, *Jour. Educ. Psych.*, 10, 1919, 448.

[49]S. W. Fernberger, A Preliminary Study of the Range of Visual Apprehension, this JOURNAL, 32, 1921, 121-133.

[50]For a description of this method *cf.* F. M. Urban, Hilfstabellen für die Konstanzmethode, *Arch. f. d. ges. Psych.*, 24, 1912, 236-243; also E. G. Boring, Urban's Tables and the Method of Constant Stimuli, this JOURNAL, 28, 1917, 280-293.

[51]The 0 was not used; runs of 3 or more digits in their natural or reverse order were not given; in series of 9 digits or less, no digit was repeated; in series longer than 9, the same digit was not used in succession or at the beginning and ending.

written in earlier reports[62]. The distracting influence of other series of numbers in the field of sight was thus avoided.

The experiments extended over a period of 7 days. At the beginning of every experimental hour the following instructions were read to the *S*s: "I am going to read a series of numbers containing from 6 to 12 digits. Listen carefully, and after I have completed the series, write in the open rectangle of your record sheet as many of the numbers in their correct order as you can remember. Some series may be so easy that you can remember all of them; others may be so difficult that you can not. Do not let a failure worry you. After writing what you remember, pull the strip of paper upward until the numbers you have written are just out of sight, and be ready to listen for the next series."

The reports were marked 'right' or 'wrong' according as the order and digits were correctly reproduced. The memory-spans and the degrees of precision were computed from the 'right' cases for every *S*. These data are given in Table I, which shows besides these values the number of trials correctly reproduced by every *S* at every series-length.

TABLE I

Showing for Every *S* the Number of Trials, at Every Series-Length, Correctly Reported, the Memory-Span, Expressed as a Limen, and the Degree of Precision

S	Series Length in Number of Digits							Limen	Precision
	6	7	8	9	10	11	12	(L)	(h)
B	67	42	26	5	2	0	1	6.713	.426
C	96	95	78	45	26	12	2	9.357	.464
E	98	97	83	77	42	29	16	9.985	.382
G	81	70	43	36	24	23	14	8.215	.226
M	93	81	68	34	21	8	5	8.592	.392

In the extensive study, a series of 4-13 place numbers was read in ascending order to 100 *S*s. The *S*s wrote their reproductions upon narrow strips of paper. They began at the top and, after every reproduction, folded the paper so as to turn the numbers they had just written out of sight. The reports were graded 'right' or 'wrong', and the memory-span and degree of precision were computed from the 'right' cases for the group. These data appear in Table II, which also shows the number of reports given correctly at every series-length.

TABLE II

Showing for 100 *S*s the Number of Reports Correctly Given at Every Length of Series

Number of Correct Reports at										Limen	Precision
4	5	6	7	8	9	10	11	12	13	(L)	(h)
100	96	86	62	38	18	5	2	1	0	7.583	.446

The results of both Studies show that we are dealing with a continuous function. The frequency of correct reports is high for the short series and low for the long series. No inversions of the first order occur, and the curves of the 5 *S*s and the curve of the group show the ogive form.

The memory spans of the 5 *S*s vary from 6.7 to approximately 10 (9.985); the memory-span of the group is 7.583—a value almost identical with that of Gates[63] (7.666), from whose experiment the digit series 4-12, used in the extensive study, were drawn.

The advantages of evaluating the data of immediate memory by the method of constant stimuli are many. (1) The measure, the memory-span,

[62]Two pieces of cardboard were fastened together. Between them ran a strip of paper from an adding-machine roll. In the upper cardboard a window 2 x 6 cm. was cut, and [illegible] the *S* [illegible] the strip. After the report had been given, the strip was pulled upward until the window was again blank.

[63]*Op. cit.*, 394.

is unequivocally defined, and the definition rests upon the accepted psychophysical principle of the limen. (2) An exact determination is obtained more readily and more quickly than by any of the methods thus far proposed; if groups are investigated, the memory span may be obtained from a single presentation of the experimental series; if studies of individuals are undertaken, 5 or 10 trials at every series length may suffice.[54] (3) All the reproductions, failures as well as successes, are taken into account. A success at a long series is not disregarded because of failures at shorter series. (4) Complicated rules and weights are unnecessary. (5) Single representative values with their degree of precision are obtained. (6) Comparisons may be made between individuals or groups from two points of view: liminal values and the amounts of dispersion may be compared.

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[54] E. G. Boring, The Number of Observations upon which a Limen may be Based, this JOURNAL, 27, 1916, 315-319.

BOOK REVIEWS

The Four Daughters of God: a Mirror of Changing Doctrine. BY HOPE TRAVER. Modern Language Association of America, 1925. pp. 92.

The Alexandrian method of Biblical interpretation, which uses a clause in literature as a metaphor, as other writers use objects in nature, would open an interesting investigation to the psychologist. We know today that this method, which has never died out in the church, has its inner source in two unlovely traits of personality,—in indolence, which substitutes the fascination of ingenious mind-play for the patient labor of historical research; and in vanity, which flatters itself that God has revealed to certain favorites inner truths which the rest of the world is too spiritually torpid to discover.

It is this type of mind that has fastened on such passages of the Bible as that which it names "The Four Daughters of God". In the work before us, Dr. Traver, in a brilliant survey, has used this famous allegory as a mirror, in which the most important change in theological thought is traced. Dr. Traver, professor at Mills College, is a scholar of high attainments. A previous monograph, *The Four Daughters of God: a Study of the Versions of this Allegory with Especial Reference to those in Latin, French and English*, one of the Bryn Mawr College Monographs, is here carried farther, with the aid of the Dutch versions, and allied originals, in the British Museum, the Paris International Library, and elsewhere, as throwing light from a new point of view on the present controversy between the extremes of religious thought. This service is the more welcome because it treats of the central theme in Christian theology, the principle of Reconciliation.

The varied versions of the story itself, in their original languages, their Biblical sources and analogies, the patristic apocalypses, related and similar myths, as well as the influence the allegory has exerted on the theologians of both the Eastern and the Western Churches, are examined with thoroughness. The relation of the Apocalypses, particularly, to the Processus Belial, is interestingly established, and the yet more important story of the rise of the cult of the Virgin Mary, as she gradually displaces the archangel Michael as a defender of humanity before the divine seat. "Mariolatry is a natural development from three causes: (1) a tendency to conceive and worship the ideal woman; (2) the desire for symmetry, for a new Eve to counterbalance Christ as the new Adam; (3) the longing for an intercessor whose interest would be for mercy without the necessity for thought of justice."

The author's central purpose is to show the shift of emphasis in theological thinking from the juridicial to the human view of the Atonement. The reader follows, with the aid of sources hitherto too little known, the gradual change of view from the avenging justice of God to his mercy, against which both the Devil and Truth contend in a persistent but losing fight. "As opposed to these somewhat external and almost mechanical conceptions is the idea of mercy embodied in the Incarnation of Christ not so much as substitute for man in satisfaction of the law of justice or in fulfillment of the sentence of death which God's truth demanded, but as a revelation of God's self to man, and as an example of obedience and love, thereby inciting man himself to repentance and confession and to a life devoted to contemplation of God and mystic union with Him. Such are the doctrines whose changes are mirrored in the allegory of *The Four Daughters of God.*"

We shall look forward with interest to Dr. Traver's coming studies in the evolution of the allegories related to that of "The Four Daughters".

Berkeley Baptist Divinity School A. S. PHELPS

L'Odorat. By H. Zwaardemaker. Paris, G. Doin, 1925. pp. 315.

This little book, published in the *Bibliothèque de psychologie expérimentale*, may be characterized as a translation of an abridged and revised edition of the author's *Physiologie des Geruchs*. It lacks the Introduction, the chapters on the Technique of Smell-measurement and on Deviations from Normal Olfactory Sensitivity, and the three sections of the Appendix. But it contains in order all of the remaining chapter-headings, and a translation of as much of the material under them as the author could use. This retention of the plan of the old book would seem to mean that in the author's mind the problems of the olfactory sense remain the same; but the thirty years that separate the two volumes have seen much new work within and without his laboratory on these problems. The technique of experimentation has been further refined, new apparatus has been devised and the old improved, the range of stimuli has been enormously increased, and the reasons for the variability of result are better understood. The outcome of all this work is written into the new parts ot the book; some of it overtly, with new figures and tables, and some of it casually in the run of discussion. The author's general conclusions, however, on all of the fundamental problems remain substantially the same; the reciprocal effects of smell-exhaustion and the compensation of odors are still 'facts', and the classification of odors is unchanged. The new note is one of greater caution as regards the significance of the quantitative results, and of emphasis on the difficulties of experimentation.

Throughout the book Zwaardermaker is, of course, always a physiologist, and he gives scant attention to writers on the psychology of smell. Henning is, with minor exceptions, mentioned only in the discussion of the classification of odors. The smell-prism is rejected for two reasons. First, the division of the *Odores aromatici* into the spicy and resinous groups is, the author thinks, unjustified, because the groups contain odors which behave similarly in small-exhaustion experiments. Furthermore, he says (mistakenly, we believe), that Henning finds no place for the amber-musk, hircine and garlic odors. In general, the author's attitude toward psychology is strikingly like that of Helmholtz. For example, unable to find either in the chemistry of the stimuli or in the physiology of the sense organ an explanation of the compensation of odors, he thinks it probable that this process is of a psychological order similar, perhaps, to Heymans' mental inhibition.

Nevertheless, psychology will be grateful for the book. It does not entirely replace the earlier work; but it is worth much to have the same problems discussed after so many years, particularly when, as in this case, the exposition is characterized by the easy familiarity, the sure touch, the ready frankness, and the authoritative tone of a master in his field.

H. P. W.

Laboratory Studies in Educational Psychology. By Egbert Milton Turner and George Herbert Betts. New York, D. Appleton and Co., 1924. pp. ix, 218.

Laboratory courses in Educational Psychology are now finding a place in the curricula of normal schools and schools of education. The problem in such courses is to decide whether one considers it of more value to take up for experimental study a wide range of subjects or to limit the work to a smaller number of experiments which are carried on by the student in a more intensive manner. In teaching such courses the reviewer has adopted the plan of taking up a relatively small number of problems, giving variations of the same problems to students, and insisting upon careful bibliographical detail. The students acquire in this manner some notion of the

experimental procedure in education and, what seems almost equally important, learn to use psychological and educational literature independently.

The present volume presents fifty-two experiments with an appendix upon educational statistics. The authors have not endeavored to describe experiments which exhaust the topics considered; the experiments are intended to be suggestive, to make general "observers" of students; and the instructor is asked "to warn the students not to generalize too much" about the experiments. The bibliography is fairly complete. Some of the experiments, owing to the simplicity of their presentation, may perhaps be misleading and harmful. For example, the casual observation of instinctive tendencies (Experiment 36) might tend to belittle the scientific difficulty of differentiating between native and learned responses. This case may be taken as illustrative of the danger involved in attempting to cover a broad range of subjects; and seems to point to the more intensive training, outlined above, as being of more value even to the average student.

Vocational Self-Guidance. By DOUGLAS FRYER. Philadelphia, J. B. Lippincott Co., 1925. pp. ix, 385.

The reviewer is at a loss to know how to approach this work upon vocational self-guidance. The first part is made up of five chapters upon vocational psychology, including a discussion of methods of self-measurement and of evaluation of personality. The second part of the book, and by far the more valuable, is given over to twenty-one chapters written by successful specialists in the various business professions. Here the material is concrete, and the values are the values of the business world. Apart from these chapters, the book seems vaguely inspirational and uplifting: broad statements are made which purport to rest upon an adequate basis in fact. But do they? Is it safe and reasonable to say, for example, that an A-man (mental age 18.0 and up) may expect occupational success as a technical salesman, while a B-man (mental age 16.5 to 17.9) may expect success as a physician? In spite of the army figures, such a statement approaches absurdity.

The reviewer can but ask whether such a book ought to be written. If there be a field of vocational guidance, does it not consist in a careful study of the individual's interests, abilities and opportunities, and an attempt upon the part of the examiner to make the individual self-reliant and independent?

Cornell University SETH WAKEMAN

NOTE

We owe the portrait of the late Professor Franz Brentano, which appears in the present number, to the courtesy of Professor Mario Puglisi of Florence, the author of the sketch of Brentano's life in our number of July, 1924. The signature is taken from the book of O. Kraus, *Franz Brentano: zur Kenntniss seines Lebens und seiner Lehre*, 1919.

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AUTHORS

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